CSG_9105

ENDED HERE 9/8/99
Pg 89
Date: Fri, 10 May 91 08:40:02 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Control Definitions

Date: Wed, 1 May 91 09:58:28 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: psy_delprato@EMUNIX.EMICH.EDU
Subject: Re: CSG-NET & The Classroom

[FROM: Dennis Delprato]

Gary,

After I sent my query re. arranging for my students to have access to CSGnet, I thought that it might be best for me to do what you suggested as a method, i.e., extract messages and forward to them as a group. We do have this capability. So, I'll do this, and in line with CST, will have them sample the material for a while after which we will participatively devise an educational exercise that will be graded--unfortunately, these institutions require grades.
copies of what I'd want forwarded. How can I access them? Dennis Delprato
Addendum: My flying fingers may have forwarded not quite what I wanted. I'll check what, if anything, I receive back to determine if I need to re-submit this message.
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Date: Wed, 1 May 91 10:11:21 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: psy_delprato@EMUNIX.EMICH.EDU
Subject: Re: CSG-NET & The Classroom

[FROM: Dennis Delprato]
Gary,
Second paragraph to previous message should read:
I do have hard copies of previous messages that I would like to forward to the students. How can I access them?
Dennis Delprato
Dept. of Psychol.
Eastern Mich. Univ.
Ypsilanti, MI 48197
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Date: Wed, 1 May 91 16:32:49 GMT
Reply-To: Paul-Pomes@uiuc.edu
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: Paul Pomes <paul@UX1.CSO.UIUC.EDU
>
Subject: News version of csg-l available

UIUC readers of the CSG-L list can now read the list via their favorite UNIX news reader (rn, rrn, nn, gnus, etc). The group is named info.csg .
/pbp
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Date: Wed, 1 May 91 11:46:56 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: "Gary A. Cziko" <g-cziko@UIUC.EDU
>
Subject: Re: CSG-NET & The Classroom

Dennis:

> Second paragraph to previous message should read:

> I do have hard copies of previous messages that I would like to
> forward to the students. How can I access them?

I think the easiest way would be for you to send me the author, date, time and subject of the messages you want to forward to them. I should then be able to find them from my disk and send them again to you. This shouldn't be too much of a chore if there aren't more than 10-15 or so.--Gary
Gary A. Cziko Telephone: (217) 333-4382 Associate Professor
FAX: (217) 244-0538
of Educational Psychology Internet: g-cziko@uiuc.edu Bureau of Educational Research
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Champaign, Illinois 61820-6990
USA
==========
Date: Wed, 1 May 91 12:50:24 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: John Reinitz <reinitz@CUBMOL.BIO.COLUMBIA.EDU
> Subject: CSG-L
UNSUBSCRIBE
==========
Date: Wed, 1 May 91 10:08:03 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: marken@AEROSPACE.AERO.ORG
Subject: Theory, Competition

[From Rick Marken]

First, thanks to Izhak Bar-Kana (910429) and Joel Judd (910430?) for the nice comments on my posting about the importance of demonstarting the phenomenon of control before pushing the theory of control. Now I want to point out the importance of theory once you do know what phenomena are to be explained. I count myself amongst those who do recognize the fact of control, understand what it is (I hope) and see it as chararteristic of everything that we consider significant about the behavior of people and other living things. Human behavior, from my point of view, involves the control of a hugh number of variables, from simple muscle tensions to complex principles such as honesty; and at any one time many of

these variables are being controlled simul-taneously. I am also convinced that many variables are controlled in order to control other variables -- there is a hierarchical relationship between con-trolled variables. I have demonstrated these phenomena of control to myself (and others) over and over again. Control theory provides a framework for understanding how this happens -- Powers hierarchical control model is now part of my mental model of what causes the phenomenon of control as I experience it in myself and in others.

Powers' theory of control not only helps me understand the, usually simple, phenomena of control that I can easily demonstrate. It also provides a framework for understanding more complex control phenomena -- such as what happens when two or more control systems interact. The theory makes PREDICTIONS about what we would see if people were organized as hierarchical control systems. I believe that it was in this spirit that Bill Powers brought up the topic of "Social Systems" and the problem of competition. Bill's model makes some interesting predictions about what happens when people interact in a world where there are fewer degrees of freedom available than those needed to be varied by all systems in order to achieve their goals. One of the most interesting predictions, to me, is that it is not physical degrees of freedom that limits control -- it is perceptual degrees of freedom. This means that even though the environment may provide enough degrees of preedom for N people to satisfy their goals simultaneously, it is possible for the people to perceive the environment as though it had only N-1 (or fewer) independent degrees of freedom. That will create conflict and competition -- even though the competition is not intentional.

I think this "degrees of freedom" problem should be fleshed out better; but I think it is one aspect of many of the problems that we appear to have in our society -- the one's Bill aluded to, among others.

Off the top of my head, I kind of agree with Izhak (910430) and Ed Ford (910430) that the apparent value that our society places on competition is not necessarily a big contributor to whatever our problems might be. I think people verbally extoll "competition" more than they actually practice it. I think competition -- real competition -- the kind where people act to deprive others in order to have for themselves, is , I think, a side effect of the degrees of freedom problem and the way certain people end up perceiving the world. One piece of evidence for this, I think, is that the most fierce advocates of COMPETITION will happily collude (cooperate) with the competition (and even break the law to do it) if it is to their mutual benefit.

I don't agree with Ed and Izhak's proposed solutions to whatever problems we perceive in society. Izhak says we should toleate violence less -- but I havn't met many people who tolerate it. Violence is competition (which I believe is a side effect of the degrees of freedom problem) in a runaway condition. Killing ALL perpetrators of violence might cut down violence a bit -- but, I think, because doing so would free up some degrees of freedom for the survivors. I'd rather find ways to increase the degrees of freedom available to all systems (this does not mean that I condone violence, of course, but who does? I bet if you gave a survey and asked "do you approve of violence against innocent people for no reason other than to rob them of to prevent them from testifying?" you would probably get 99.99% "No"). As to Ed'd solution, I don't see how it is informed by the control model. How does faith in a "higher power" improve the ability of control systems to cooperate for their mutual benefit? My experience has been that, since these faiths are based on verbalisms rather than phenomena, people tend to perceive the meaning of these words slightly differently. Since many of the faithful have goals about what they want to perceive others believing, we see efforts at corrective action to bring people to the "true faith"--ie, theirs. It took years for Western societies to free themselves of this source of conflict. Of course, we are not completely free of it. Faith may be great individually (I can't do it because my thought processes keep getting in the way) but as a

solution to social problems I think it ranks up their with economic ideologies as a singular cause of those problems.
In summary, I want to suggest that the value of theory is that it provides a framework for understanding complex phenomena based on a model of simpler phenomena. I think the control model is relevent to understanding complex phenomena like competition in social systems. I think we should base a discussion of competition on the model -- rather than suggesting solutions that we could have picked up as easily from conservative newspaper columnists or Sunday evangelists.

By the way, I just got the book "Feedback thought in social and systems science" by Richardson (U Penn Press, 1991). It's excellent. It gets added to the exalted CSG bookshelf.

Best regards

Rick M.

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Los Angeles, CA 90024 Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)

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Date: Wed, 1 May 91 17:33:37 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU
Subject: Competition; verbalisms

[from Joel Judd]

Bill (910430) says,
> I think that Bruner is right in saying that at the verbal levels, we make
> up stories about ourselves and other people, and communicate them to
> others. He is, in fact, doing that. But you can't "get the story right"
> unless there is a background of nonverbal experience against which to
> compare the meaning of a narrative. That background comes first, the
> story later.

I interpreted his conclusions in _Child's Talk_ to mean just that--we are prepared very early to represent linguistically certain basic behaviors (eg. representation) before we attain the linguistic ability to do so. In fact, the claim among some PLA researchers is that if there is anything INNATE, it is these "cognitive predispositions" to learn to participate in our family/society/culture. This participation happens to later require, for humans, sophisticated and variable linguistic abilities. I have wondered (quietly, to myself) if these initial predispositions are in fact some of the intrinsic reference signals which drive initial perceptual organization?

> I prefer to think of these narratives as attempts to describe nonverbal perceptions, rather >than as the causes of anything. And so here again there is another argument for >considering language as part of the perceptual hierarchy, and not separate from it, as you >have mentioned before. And I think that we, not the "world," modify our desires and

beliefs, >and that we really modify our desires and beliefs, not just "expressions" of them. That's because in my model desires and beliefs have real existence.

I must have reported this poorly, because I interpreted the world to be disturbances to our perceptions of the environment.

> If I am hearing Bruner correctly through your words, he seems to be
> proposing ways in which objective observable factors drive our behavior
> -- in this case, our narratives. I don't think that's true. I think that
> Bruner is still on the old track -- not surprisingly -- of trying to
> explain behavior in terms of external influences. I don't know what he
> really thinks, of course, but we all know the audience he is writing for.

Unfortunately, some of his conclusions smack of linear models. I tried to summarize them in a way that would hide that, but I can see that it's no use against an old pro like yourself.

I prefer, though, to see (or should I say perceive?) the narrative idea as a reflection of our perceptions, and it serves two purposes. And I just remembered a book with related ideas by Gazzaniga called _The Social Brain_(?) which also deals with the first of these, and that is making sense of the world to ourselves. Through verbalisms we interpret, convince, and confabulate what we perceive--(is this too far off the mark?) We do so (at least initially) according to patterns and interpretations which come to us from family, friends, society (thoughts about raising children have cropped up on the NET before). Narratives reveal the way we justify, explain, account for disturbances to canonical concepts that we have learned, through verbal and non-verbal perceptions: the way you treat family members; marriage/divorce; education; etc. The second purpose is to convince others that our narrative (perception) is valid, or to go further and convince them that our interpretation of the world is the correct one. This is one area where conflict arises among members of society.

This leads into recent comments from Ed and Rick (910430/910501) about what we base our values on. I've withheld commenting about religion so far as I've enjoyed comparing CT with my own beliefs privately. I think it's OK to propose that something like CT can provide information about societal problems and solutions to them. But I don't rule out the idea that higher-level reference levels could be adopted from a "higher authority" instead of "evolving" by trial and error, or some other method. Rick says:

> As to Ed's solution, I don't see how it is informed by the control
> model. How does faith in a "higher power" improve the ability of control
> systems to cooperate for their mutual benefit? My experience has been that,
> since these faiths are based on verbalisms rather than phenomena, people
> tend to perceive the meaning of these words slightly differently. Since
> many of the faithful have goals about what they want to perceive others
> believing, we see efforts at corrective action to bring people to the "true
> faith"--ie, theirs.

I don't see faith in a higher power as inherently problematic, nor does faith automatically translate into cooperative, loving control systems. This comment provides both what can be right, and what can go wrong with one's faith. If the faith inspires system concepts of the sort that foster peaceful coexistence and mutual cooperation--where is the harm in that? If there is only lip service being paid to the values, then we have what's commonly called "hypocrisy." Unfortunately, we do deal with higher levels in "verbalisms," so what I perceive by "love thy neighbor" may not be exactly what you perceive. However, there are ways of judging what the way another perceives values, one of them being "by their fruits ye shall know them."

Another problem Rick presents is the tendency which humans have, once they feel they have the "truth," to try and convince/coerce others to perceive things the same way. This type of behavior is not all that different from fanatics of political ideology or any other ideology. This has two effects: 1) to attempt to take away another's free agency (ie. control) and 2) to discourage one from looking to religion AT ALL for answers about our existence. A related comment would be that if one were to consider the possibility that there might be a worthwhile religious organization somewhere on earth, we would still have to face the fact that running it and belonging to it would be the same old imperfect control systems we find everywhere else. So one should be careful not to throw out the system because of the people who are involved in it.

> Faith

> may be great individually (I can't do it because my thought processes keep
> getting in the way) but as a solution to social problems I think it ranks
> up their with economic ideologies as a singular cause of those problems.
> In summary, I want to suggest that the value of theory is that it provides
> a framework for understanding complex phenomena based on a model of
> simpler phenomena. I think the control model is relevant to understanding
> complex phenomena like competition in social systems. I think we should
> base a discussion of competition on the model -- rather than suggesting
> solutions that we could have picked up as easily from conservative
> newspaper columnists or Sunday evangelists.

And I think anyone familiar with such matters would agree that faith has to be an individual matter; I can't "give" it to you any more than I can give you good manners. But I think Ed's comment gets not to the proposal that a particular RELIGION would solve society's problems, but that certain VALUES might. And CT explains how and why these values would--they being a high level of control. I don't understand a separation of the two.
Joel Judd

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Date: Wed, 1 May 91 22:15:30 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: UPPOWER@BOGECNVE.BITNET
Subject: Higher power

[From Bill Powers]

Rick Marken (910501) --

> How does faith in a "higher power" improve the ability of control
> systems to cooperate for their mutual benefit?

The method of levels might have something to say on this subject. One of the unjustified postulates behind this method is that awareness usually operates as if from some particular level, which gives form to the current point of view. What you see from this point of view is the set of all perceptual signals of lower levels, with the current point of view projected into them as an attribute of this apparent external world. So if you're working from the category level, it seems that all the relationships, events, transitions, configurations, sensations, and intensities that you experience are exemplars of

categories. You aren't conscious of categorizing; you just see that the categories are there, as if they existed objectively. So you're unaware of the operation of the level currently occupied by awareness. You're aware of the lower levels THROUGH it. This is all very metaphorical and I don't know what it's a metaphor FOR, but pragmatically it seems to reflect experience.

Working this metaphor in the other direction, the implication is that you are also unaware of the operation of control systems of HIGHER level than the "occupied" level (the level in the state we call conscious, to be slightly more operational about this). In particular, you're not aware of what is setting the reference signals at the occupied level: they are experienced simply through realizing that some perceptions are in the wrong state (you feel an effort to change them) and others are OK. You see a square with one side bowed out, and that looks WRONG. You want to push it straight and make it into a better square -- a better exemplar of squareness.

As far as consciousness is concerned, then, the definition of OK and not OK is given, not chosen. If you happen to be conscious at the logical level, the next thing that happens is a lot of reasoning about where this OK-ness is defined. Ahah, it is clearly coming from a Higher Power. And that is perfectly correct: it is coming from higher levels, principles and/or system concepts, systems that are running automatically in the form they had after the last reorganization -- but not consciously.

Which brings us to the next sentence in your post:

> My experience has been that, since these faiths are based on verbalisms <rather than phenomena ...

Not so fast. What I've just been proposing is a phenomenon that a lot of people may have experienced throughout history. They don't have to be theoreticians to experience it, but if they are theoreticians and don't have any constraints on their theories like science, they are free to propose any explanation they like. One of the theories is that this advice from above about what is OK and what is not OK comes from a supernatural power outside of you (perhaps acting on your insides, but basically existing in a universe larger and more powerful than yours). Moses came down from the mountain with ten Principles engraved on tablets. Could this be a story reflecting the first conscious human experiences at the principle level? Moses' theory, of course, was that the principles were handed down from a Higher Power -- which we, of course, recognize as the System Concept level. Moses heard a Voice that commanded him. If the highest organized level in which your awareness can reside is the principle level, the REFERENCE principles will seem to come to you out of nowhere, but that doesn't stop you from trying to devise a Where.

One of the constants across religions is a belief in the power of prayer or submission to divine guidance. Instead of thinking about the content of prayers, think about the attitude behind them. One has to deliberately seek a state in which guidance is sought and accepted. In other words, the rational system (if that's the highest conscious level) has to find a logical way to accept that it is not the highest level, and so not resist any changes IN ITSELF that it can't explain rationally (or more generally, characterize in terms of its typical mode of perception, evaluation, and action). I think this is an attitude that fosters going up a level, because it encourages you to observe the conscious level rather than just interpreting the world through it. You begin to experience it AS a level, and you can't do that FROM that level.

Of course the next level has to exist if any of this is to happen, and it has to be functioning at least a little bit. I think that theories are proposed most flexibly when the next level up is still forming and isn't working very well. It's possible that the principle level formed in historical, or at least legendary, times. And it's possible that we are still in process of forming the highest level I have any inklings of, the system

concept level. Control theory is a system concept, surely. Where did it come from? Don't ask me: there it was. There must have been a time in the history of homo sapiens when NO system concept would have made any sense, NO principle, NO program. It's hard to imagine how the world would have looked when the highest level was sequence.

Human beings have been thinking about system concepts in an organized way for less than a few centuries, I would guess. Well, maybe that's an exaggeration, especially as it implies that everyone develops the next level simultaneously. But just look at the way people have been trying to model human beings since the 1940s. There has been an explosion of conjecture, with all sorts of new ideas showing up out of nowhere. There has been a quantum change in the very way we ask questions about organized systems. So it may be that our system concept levels have just started becoming functional on a wider scale. No wonder we aren't very good at this kind of control.

And another implication is that a new level above system concepts is starting to bestir itself, poking random reference signals into the existing system concept level, saying "let's try this one on, or that one, or maybe that other one." What's IT going to be about? There will probably come a time when people begin to get a strong sense that something is telling them to choose PARTICULAR system concepts and avoid others: something that speaks to them from a direction they cannot comprehend any more than the first flint-knapper comprehended where the idea of sharpening stones came from. They are bound to wonder where that advice is coming from. There may be human beings alive now who wonder why I am having such a problem imagining why we pick one system concept rather than another.

So, Rick, I think there IS a phenomenon, and that religious and philosophical thinkers have been trying to comprehend it. I don't agree with their theories, but I don't claim that they have been theorizing about nothing, or just verbalizing.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Thu, 2 May 91 08:09:33 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: marken@AEROSPACE.AERO.ORG
Subject: Religious behavior

[From Rick Marken]

In response to Joel Judd (910501) on religion:

At the risk of offending everyone, let me share my own thoughts about the relationship between control theory and religion. Religion, from a control theory perspective, is just something people do. In the model, religions are system concepts. The particular religion that you follow is (according to the model) determined by the highest level references in the model. So, in theory, there is no way to change references for religious system concepts other than by reorganization -- and given the rather remarkable shifts I have seen people go through in their searches for spiritual fulfillment, random reorganization seems like how it works. A religion is a perception derived from lower level perceptions of principles (values, morals) programs (rituals) , relationships (worship, prayer), etc. Different religions represent different
combinations of these lower order variables controlled at different reference levels.

So "being religious" is something that a ten level hierarchical control system can do, like "being a dodger fan" or "being a control theorist" (though don't ask me to build a working version of a religious control system this weekend -- give me about 300 million years). I don't believe that there is some "right set of values" for getting along in life or getting along with others any more than I believe that there is a correct way to hold your right hand. There are certain values (rules) and rituals(programs) that are right if you want to perceive yourself as a "catholic" or a "buddhist" or a "dodger fan" just as there is a correct way to hold your right hand if you want to say the pledge of allegiance correctly. "Right" for a control system means "matching a reference signal"; and the principle, program, relationship, etc level reference signals that define a particular religion are set by the system level religion control systems.

Since nothing really sets the reference for the highest level systems (other than reorganization due to intrinsic error) there is no experience of anything that says "be catholic" or "be a secular humanist" so, I think, we have the experience that we take our system concepts "on faith"; they just are true; they are what we like. We may attempt to rationalize why we want to maintain a particular system concept but, ultimately, if it is really a system level reference (and not just, for example, a program level perception that you are controlling in order to, say, "please your parents" -- a principle level perception) then there is really no more of "you" left to adjust system level references to satisfy any higher level goal. Some system concepts (the religion ones) are sometimes thought of as more important than others (the sports fan ones), but I'm not impressed that this is anything other than a historical accident; if things go on as they are in soccer fandome, there will soon be as many people who have died (and killed) for the home team as there were who died (and killed) for yahweh (or christ or mohammed or whomever).

I don't want this to be taken as anti religious in any way. Control theorists just want people to behave "up to specs" (in Bill's wonderful phrase) -- and that means, to be able to control the variables they need to control without interfering with other people's ability to control what they need to control. Many people seem to get great satisfaction, inspiration and spiritual fulfillment from faith (ie, controlling religious system concepts) and they do it without messing up other people. That's just great. All I want to argue is that the control model should be able to explain all of human behavior and that certainly includes behavior that is called religious. The control model implies nothing about what the best set of principles are for people to adopt in order to live best and get along best with others. There is reason to suspect that many different sets of principles will do. However, there are certain principles which will lead to problems -- not because god said so (though s/he may have -- s/he just never says much to me) but because they are inconsistent with the nature of human nature. So a principle that allows a person to enslave other people (a principle, incidentally, that god never saw fit to condemn -- the hebrews started enslaving people, apparently with god's blessing, shortly after they themselves were freed from slavery) may work for some time (it did) but it's not a good long-term basis for running a society; because the slaves are control systems and they will always try to get as much control as they can. And people waste much of their productivity doing what is needed to keep the slaves slaves. It also violates the "up to specs" rule since a slave probably has a hard time finding the set of references that eliminates intrinsic error.

I would hope that control theory might be able to give a theoretical basis for understanding the best way for people to get along with each other and do the best for themselves as well. If the result of this theoretical exercise says "thou shalt have no other gods before me" then I shalt not.

Well, enough ranting for tonight. Let's see what this does.

Note: I wrote this before receiving Bill's (910501) post this evening. I'm going to post it anyway because I hate to waste all this time ranting and raving for nothing. But it

does answer, sort of, Bill's complaint about my claim that religions are based on verbalisms rather than phenomena. I agree that that claim of mine was wrong. As a matter of fact I have had religious experiences (perceptions of religious phenomena) myself (almost always while listening to Bach, Mozart or Beethoven). What I meant to describe (and what I will stick to) is my impression that many institutionalized religions, which take "scripture" very seriously when it comes to articulating their principles, tend to mistake the words for for whatever wisdom (phenomena) those words may be trying to articulate. If you need to read a book in order to find out that it is wrong to kill and steal then let me be the first to encourage you to keep reading that book.

Regards

Rick M.

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Richard S. Marken USMail: 10459 Holman Ave The Aerospace Corporation
Los Angeles, CA 90024 Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)

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Date: Fri, 3 May 91 09:57:33 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: UPPOWER@BOGECNVE.BITNET
Subject: New address; Closed-Loop

[From Bill Powers]

Mighty quiet out there. Yeah, TOO quiet. You think mebbe we skeered them critters off? Naw, they're out there, all right, jest keep your eyes peeled. Shore is quiet. Yeah.

The new address for MAILING, way out West, will be:

Mary and Bill Powers
P.O. Box 2566
Durango, CO 81302-2566

The STREET address for visiting will be

4088 County Road 203, Unit 2
Durango, CO 81301

That's 4 miles north of the start of CR 203, which runs parallel to State highway 550, the road from Durango to Silverton. Going north from Durango, 203 goes first on the left, then on the right of 550, like a frontage road that got lost. We're a couple of miles north of the crossover.

Our phone number will be 1-303-247-7986.

Computer gets packed the evening of May 5. Movers arrive May 6. We're out of here May 7.If the movers can find us, our furniture will arrive May 13 or 14.

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Greg Williams has not received enough replies to his request for permissions for publication of exerpts from CSGnet to carry out his plan of putting another round of "Closed Loop" in our next newsletter. This plan is still alive, though, soplease drop everything, fill out this form, and send it TODAY to

Greg Williams
Rt. 1, Box 302
Gravel Switch, KY 40328

TO GREG WILLIAMS:

YOU HAVE MY PERMISSION TO USE EXCERPTS FROM MY POSTS ON CSGNET IN "CLOSED LOOP." I RETAIN ALL COPYRIGHTS TO MY POSTS, AND YOU WILL INDICATE THAT FACT BY INCLUDING A LEGAL COPYRIGHT NOTICE IN "CLOSED LOOP" FOR EACH EXCERPT FROM MY POSTS. I MAY CANCEL PERMISSION (NON-RETROACTIVELY) WITH REGARD TO ANY PORTION OF MY POSTS BY GIVING YOU SIX WEEKS' NOTICE.

SIGNED _____

DATE _____

Please print:

NAME _____

ADDRESS _____

_____

_____

Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Fri, 3 May 91 08:44:23 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: marken@AEROSPACE.AERO.ORG
Subject: Cognitive Controlled Variables

[From Rick Marken]

Gee. I guess I did offend everyone. In an effort to start back down a less touchy track, how about cognitive controlled variables again. One thing that cognitive psychologists were very interested in was how people solve formal problems (like math problems). In one paper from way back (that I really liked) a couple of cognitive psychologists (one was Peter Poulson, now at U Colorado in Boulder, and I forgot the other) studied how people solve "water jar problems". In the problems they studied there were three jars, each of which could hold a known amount of water -- say 8, 5 and 3 pints, respectively. You start with the 8 pint jar filled and the other 2 empty. The goal is to divide the 8 pints evenly (4 and 4) into the first two jars by pouring from one jar into the other (you can only measure the water in terms of how much fits into each jar).

There is a well defined series of moves (the problem space) that represents the amount of water in each jar at each point in the problem. For example 8,0,0

> 3,5,0
> 3,2,3
> 6,2,0

is one possible sequence of amounts of water in the three jars. The goal is to get to 4,4,0. There are actually two paths through the problem space that will get you there. What Poulson and X found was that people have particular difficulty at certain points in the problem -- these are the points where moves are required that would make the set of amounts in the jars look less like the goal -- ie going from 6,2,0 to 6,0,2 instead of 3,5,0. 6,0,2 is less like 4,4,0 (by taking jar by jar differences) than is 3,5,0. Their theory of problem solving basically says that one variable subject's try to control is the difference between the current and goal (reference) state of the problem. They call this "means-ends analysis". They point out that, if this were the only goal the subjects pursued they would never solve the problem. I think this kind of problem is a nice place to do some control theory research on the kinds of variables subjects control in order to solve these types of problems. It would be pretty easy to introduce disturbances (just change the state of the problem and see if the subject resists the change by going back to a previous state or not). I think it would also be nice to develop a working control system model that could solve these types of problems -- then we could call it AI and get people's attention. What do you think?

Regards

Rick M.

****

Richard S. Marken USMail: 10459 Holman Ave The Aerospace Corporation
Los Angeles, CA 90024 Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)

==========

Date: Fri, 3 May 91 10:03:49 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: UPPOWER@BOGECNVE.BITNET

[From Mary Powers]

Thanks for agreeing to be the CSG's registered agent. The officers have de-cided to give up and fold the corporation, however, which is a trremendous relief, so your offer won't be taken up. The CSG, of course, goes on --minus the superstructure and the grief.

==========

Date: Fri, 3 May 91 11:34:05 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU

>
Subject: Re: Cognitive Controlled Variables

Rick (910503),

> Gee. I guess I did offend everyone.

Well, not me. But I can tell when the discussion gets kind of "far afield" it seems most people would rather "stay in the house." Talking about higher levels seems kind of ethereal I guess; not terribly scientific.
Joel Judd

==========

Date: Fri, 3 May 91 11:51:54 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: Jeffrey Horn <jhorn@UX1.CSO.UIUC.EDU
>
Subject: RE: CT and AI

(from Jeffrey Horn)

> Rick Marken writes:

> I think if would also be nice to develop a working control system model that
> could solve these types of problems -- then we could call it AI and get peo-
> ple's attention.

Well, you got my attention. I am particularly interested in control systems as an AI architecture, and it would be nice to see how such systems perform on traditional AI tasks, such as planning. As for Means Ends Analysis, it is usually applied in a non-sequential manner. That is, the MEA planner tries to reduce the differences between two states (initially, the start and goal states) by working forward from one state or backward from the other. Perhaps a hierarchical control system could model this behavior, where a higher level loop would alternate between working backwards from the goal or forwards from the start state (e.g., "if I could just get to 4-3-1, I could then easily get to 4-4-0, and I could get to 4-3-1 if I could just get to..."). But MEA, like most AI planners, gets stuck, as you pointed out, because it is only hill climbing. It uses some measure of state difference, such as your sum of differences in each jar's level, and takes the action that reduces this measure the most: gradient descent. The better the measure (i.e., the fewer local maxima it induces on the search space), the less likely MEA is to get stuck. So how good a hill climber can CT produce? Can we design a control systems that doesn't just climb hills? Hmmm, lots of issues and results of interest to the AI community, I believe.

-jeffhorn@uiuc.edu (jhorn@ux1.cso.uiuc.edu)

==========
Date: Fri, 3 May 91 14:15:57 -0400
Reply-To: coombs@cs.rochester.edu
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: David Coombs <coombs@CS.ROCHESTER.EDU
>
Subject: Re: CT and AI
In-Reply-To: Your message of Fri,
03 May 91 11:51:54 -0500.
<9105031658.AA18688@cayuga.cs.rochester.edu
>
If you haven't seen it, you might be interested in Jens Christensen's paper "A Hierarchical Planner that Generates its Own Hierarchies" in the proceedings of AAAI-90.

dave
David Coombs Dept of Computer Science
coombs@cs.rochester.edu University of Rochester
...!rochester!coombs Rochester, NY 14627-0226 USA
==========
Date: Fri, 3 May 91 15:21:13 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU
> Subject: Closed-loop copyright
In-Reply-To: Message from "CSG-L@VMD.CSO.UIUC.EDU" of May 3, 91 at 9:57 am
[From Cliff Joslyn]

> Greg Williams has not received enough replies to his request for
> permissions for publication of exerpts from CSGnet to carry out his plan
> of putting another round of "Closed Loop" in our next newsletter.
Of course, Greg is the best and final judge, but I believe that it is not technically necessary to get such permission. By default, publications (such as posts to CSG-L) are in the public domain, UNLESS the author claims copyright by including e.g. the following:
Copyright 1991 Cliff Joslyn

somewhere in the post.

Perhaps someone can correct me, or Greg has good reasons for not doing this.

O----------
> | Cliff Joslyn, Cybernetician at Large, cjoslyn@bingvaxu.cc.binghamton.edu | Systems Science, SUNY Binghamton, Binghamton NY 13901, USA V All the world is biscuit shaped. . .

==========
Date: Fri, 3 May 91 14:40:31 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: Fred Davidson <DAVIDSON@VMD.CSO.UIUC.EDU
> Subject: Re: Closed-loop copyright
In-Reply-To: Message of Fri,
3 May 91 15:21:13 EDT from <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU
>
[From Fred Davidson]

[re. Joslyn 910503, appended]
I am still trying to remember where I read about that convention on copyright and ownership rights. Did anybody else come across that announcement? I saw it in a newspaper someplace. Presumably the proceedings or conference program from that convention would help us understand this.

If we can recall what it was, or to put it more apropros of CSG-L, if I can reduce the error in my newspaper-reading system, then I heartily volunteer to do some digging at our library to see what was said.
-Fred Davidson.

On Fri, 3 May 91 15:21:13 EDT Cliff Joslyn said:

> [From Cliff Joslyn]

> Greg Williams has not received enough replies to his request for
> permissions for publication of exerpts from CSGnet to carry out his plan
> of putting another round of "Closed Loop" in our next newsletter.
>

> Of course, Greg is the best and final judge, but I believe that it is
> not technically necessary to get such permission. By default,
> publications (such as posts to CSG-L) are in the public domain, UNLESS
> the author claims copyright by including e.g. the following:
>

> Copyright 1991 Cliff Joslyn

> somewhere in the post.

> Perhaps someone can correct me, or Greg has good reasons for not doing
> this.

> O--
>
> | Cliff Joslyn, Cybernetician at Large, cjoslyn@bingvaxu.cc.binghamton.edu
> | Systems Science, SUNY Binghamton, Binghamton NY 13901, USA
> V All the world is biscuit shaped. . .
==========
Date: Fri, 3 May 91 15:28:49 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: m-olson@UIUC.EDU
Subject: Re: Cognitive Controlled Variables

Rick said,

> Gee. I guess I did offend everyone.

Then Joel said:

> Well, not me. But I can tell when the discussion gets kind of "far afield"
> it seems most people would rather "stay in the house." Talking about higher

> levels seems kind of ethereal I guess; not terribly scientific.
>

>
Rick,
Not me either.

Joel,
Ethereal, maybe. Scientific, maybe not. Surely interesting, though! It's hard to conceptualize a systems level analogy of a tracking task. It sure would be nice to make the ethereal scientific.

Anyway, the idea that the systems level is a recent (a few thousand years) development is interesting. Could we develop a classification system of the animal kingdom based on the number of hierarchy levels each species possesses? My guess is that we would find a relationship between the amount of "rights" we give to a species and the number of hierarchy levels that species possesses. This idea just occurred to me, and, no, I am not particuarly interested in animal rights as a topic in itself.

I've gotta go and read (as opposed to skim) the last few entrees on this topic. As far as I'm concerned, though, we shouldn't avoid it cause it sound unscientific--talking "unscientifically" often leads to an Idea which when tested "revolutionizes" science. In other words, another variable mean to an agreed upon end.

Carpe' Diem

Mark Olson

==========
Date: Fri, 3 May 91 14:29:52 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: marken@AEROSPACE.AERO.ORG
Subject: Animal Hierarchies

[From Rick Marken]

Mark Olson (910305)

> Anyway, the idea that the systems level is a recent (a few thousand years)
> development is interesting. Could we develop a classification system of
> the animal kingdom based on the number of hierarchy levels each species
> possesses? My guess is that we would find a relationship between the
> amount of "rights" we give to a species and the number of hierarchy levels
> that species possesses. This idea just occurred to me, and, no, I am not
> particuarly interested in animal rights as a topic in itself.

I agree -- Bill's idea of a recent origin of the systems level is extremely interesting. I kind of don't believe it because I have this notion that the levels of perception are structurally imposed by the nervous system -- and, thus, are a result of evolution rather than learning. I read the physiological evidence as pointing in this direction; that is, there are cells, for example, in the lateral geniculate (I think) that look for patterns (configurations) rather than for other classes of perception (transitions, etc). I think the type of configuration the cell sees can be learned -- a curve rather than a line, maybe. I don't know of any evidence for this learning capability in cell receptive fields.

But I think that the learning would be within a class. If the control model represents, to some extent, both the functional and structural organization of the nervous system, and if there is a systems concept level up there in the cortex, then that's what it perceives - systems. Any kind of system, maybe, but just systems. If there were a level higher than systems, than I think it would have shown up by now. On the other hand, maybe it has always been there --it just didn't have much material to work with until now. Maybe that's why the system's level appears to show a historical development. It was always there, maybe, (in homo sapiens) it just didn't have much to work with early in the going.

Your suggestion about looking a species in terms of the levels that they can control is very interesting. I thing an extremely good start at this is provided by F. X. Plooij (1980) The behavioral development of free-living chimpanzee babies and infants (Norwood, NJ: Ablex). He does a hierarchical control analysis of the behavioral capabilities of apes. For example, by observing the speed with which the ape baby's head oscillates during rooting they conclude that transition control emerges at about 2 months. Plooij claims to find evidence for 8 levels of control (up to principles) in the ape. I don't know if I buy it all but it sure is fun reading if you like natural history.

Best Regards

Rick M.

==========

Date: Fri, 3 May 91 17:46:29 cdt
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: "McClelland,Kent" <MCCLEL@GRIN1.BITNET
>
Subject: Query

I'm new to CSG-net, having only been following the exchanges for about a week, so forgive me if I violate the norms of the system by speaking of the personal rather than the profound.

As a sociologist, I've been interested in Control Theory ever since I read BEHAVIOR: THE CONTROL OF PERCEPTION several years ago, but I didn't get time to dig into it deeply until a sabbatical finally came along last spring. When my sabbatical ran out last summer, I had an unfinished, rather sprawling manuscript on connections between control theory and sociology. Of course, teaching at a liberal arts college, I haven't had time to revise it, but I hope to get back to it in the next month or two. My question is whether there are people out there with the time or interest to look at the draft and give me some comments on where to go next.
The draft contains the following: an introduction focusing on Jack Gibbs's recent book, CONTROL: SOCIOLOGY'S CENTRAL NOTION, saying that Gibbs missed the boat by ignoring Powers's Control Theory; a very brief but fairly comprehensive review of the multi-disciplinary literature on Control Theory (now a year out of date); a section attempting to explain the basics of Control Theory in hopefully not-too-simplistic terms to an audience of sociologists; a final section applying Control Theory to a discussion of interpersonal power.

By the way, I got connected to CSG-net after meeting Clark McPhail at a recent convention, where he gave a very slick demonstration of some computer programs Bill Powers wrote to simulate crowd behavior. Clark suggested I subscribe to the network, which seems to have been good advice, judging by the interesting things I've seen so far.

Kent McClelland
Associate Professor of Sociology

Grinnell College
Grinnell, IA 50112
USA

Bitnet: mcclel@grin1
Phone: 515-269-3134

==========

Date: Fri, 3 May 91 18:48:25 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: marken@AEROSPACE.AERO.ORG
Subject: Animal Hierarchies

[From Rick Marken]

Mark Olson (910305)

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> development is interesting. Could we develop a classification system of
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Best Regards

Rick M.

****

Richard S. Marken USMail: 10459 Holman Ave The Aerospace Corporation
Los Angeles, CA 90024 Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)

==========
Date: Sat, 4 May 91 09:55:17 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: UPPOWER@BOGECNVE.BITNET
Subject: system Concepts (Bill)

[From Bill Powers]

When the issue of religion, higher power, faith, and so on appeared on this net, only a couple of voices were heard against a vast silence. This is interesting. I happen to know that there are some strong opinions out there, a few favorable and many unfavorable, on this subject. I jumped right into it with a control-theory-based conjecture about the way religious perceptions and phenomena fit into the control model, and Rick, after expressing his views along the same lines, noted that we seem to have hit a touchy subject and offered to change it (not that we're limited to one subject at a time). And Joel Judd may have expressed more than one person's view when he said

> Talking about higher levels seems kind of ethereal I guess; not terribly
> scientific.

The interesting aspect of Joel's comment is that it IS a higher-level point of view. To say that something isn't terribly scientific is to imply that we try to say things that ARE scientific. From this I deduce that one can perceive the degree of scientificness of a discussion. If the degree is less than some desired degree (very scientific), something must be able to detect the difference between the actual degree of scientificness and the desired degree. This difference, I take it, is the basis for whatever action is taken concerning the discussion, such a writing a sentence saying that it's pretty ethereal. Clearly, there must be a system concept about what "scientific" means, and there seems to be a control system related to it.

It seems to me that for those who consider stick-wiggling boring and want to get into the more interesting higher-level aspects of the control-system model, we have here a wonderful laboratory in which to explore the real system, the one we carry around in our heads all the time. If I say something that bears on religion, your first reaction to it is evidence about the system concepts you have and are willing to defend. If it is possible for you to observe those reactions and bring out a fuller description of them, you will have one foot in the point of view from which you can evaluate system concepts as a phenomenon, using a real live example. As you observe this example of a system-concept control system in action, you will see how control actually works at this level, and gain a deeper understanding of the way system concepts guide and use lower levels of organization such as those having to do with principles and programmatic thinking -- logic.

Of course in order to do this, it is necessary at least for the moment to cease identifying with any particular system concept -- that is, treating it as your own point of view. I would wager that very few of those who saw the "religious" topic go by did anything but identify with whatever system concept was operable at the moment. The

disturbance was successfully counteracted; the incipient error was kept small. If the topic had switched immediately back to one of the other lower-level topics that have been popular, there would have been a little sense of relief, of relaxing the guard. The disturbance would have gone away.

And now here it is back again. So what's happening now? Same sense of error again? Same generalizations about why it's not a good topic? Same strategy for making it go away? Have you been here before? If so, why not observe what's going on this time? You don't have to identify with a system concept to do that. It's just a system concept, a phenomenon. It relates to principle thoughts and logical thoughts and familiar words and phrases hooked up into familiar sequences. When you're just observing it, it isn't a good concept or a bad concept; it's just what it is and it works the way it does.

Phenomena first. Theory second. Hearken to Marken.

Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

==========

Date: Sat, 4 May 91 09:56:11 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: UPPOWER@BOGECNVE.BITNET
Subject: System Concepts (Mary)

[from Mary Powers]

Wonderful! In the midst of the final throes of packing along comes this new thread - religion - which I can't keep my hands off. We're talking a bunch of systems concepts here - organized religions of various flavors, God, and what Ed referred to, as the 12-step groups do, a Higher Power.

I don't hold with organized religion any more than Rick, and for similar reasons - they don't do anything for me, and, in their names, people have done and do horrible things to each other. The latter is not so much a flaw of religion, though, as it is a result of the human bias to consider only as truly human the members of one's own group - those you treat with the Golden Rule, etc., but those others, unbelievers, heretics, etc. -anything goes (but that's another thread).

I don't believe in God either, simply because giving a concept like that a name concretizes it, and soon you have paintings of a man with a white beard zapping Adam into life. I love myths and fairy tales, but I don't believe them as explanations of how things came to be. I prefer stories that work - models - to explain thi-smology, evolution, contineal drift.

w t nd of story is eliminated, there is a major part of religion still left, and that is concerned with the principles one lives by. I'm not in favor of buying any particular religion's list, but I am in favor of spending some time thinking about such things and whether what one is doing with one's life is relevant and consistent with them. (Ed is concerned with what he perceives as a decline in morality - I am impressed by the huge jump in the last couple of years in books on ethics that have come into the library where I worked until recently.)

Of the three concepts I listed in the first paragraph, the one that makes the most sense to me in terms of control theory is the idea of a higher power. God, as they say, is everywhere, which means inside as well as Out There. Acknowledging a higher power is to

recognize that there's a lot more to oneself than one's conscious Self. Think of that forgotten name that appears an hour after you stopped trying to remember it, or, more seriously, the new idea or a solution to a problem (which can be intellectual, artistic, emotional, spiritual, moral or whatever) that just appears, again not through conscious effort. One must consciously prepare the ground, but the answers come from a higher level than where one is consciously at, and it's no particular surprise that in a religious context they are called gifts from God.

It seems to me that this kind of thing happens best with practice, and the practice is letting go (the Twelve-Steppers say "Let go and let God"). The letting go is often done by sleeping. I take long hot baths. Many people do it by prayer and meditation. The interesting thing to me is that effortfully trying to get an idea or solve a problem looks very much like pushing on a conflict. As was discussed in the psychotherapy thread,control theory says that you cannot force a solution to a conflict, but resolve it by - whaddaya know! - going up a level. To one's higher power, or certainly to a higher level in oneself.

Whether or not doing this eventually leads one to being a more decent, moral person I do not know, but it seems likely to me. Over the last few millennia the religious life has produced (in addition to the bureaucrats, the power freaks, and the sadists) some very mellow souls, and it's worth looking at what they have to say. Because they are talking (obscurely and metaphorically, usually) about levels of the mind that control theory, coming from the bottom up, is as yet only pointing at.

==========

Date: Sat, 4 May 91 09:04:32 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: marken@AEROSPACE.AERO.ORG
Subject: Feedback

[From Rick Marken]
Are my posts getting out? I'm not getting any feedback from the server. I sent the same post twice yesterday. Was it received?

Thanks for the info (if I get it).

Rick Marken
marken@aerospace.aero.org

==========

Date: Sat, 4 May 91 12:20:00 LCL
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: Kampis Gyorgy <h1201kam@ELLA.HU
>
Subject: help with CT models

Hi there,

I do not know if this got through to you - I did not get any response but this is no wonder as our mainframe was broken down for a week. If you flooded me with answers I apologize and ask you to repeat them - pls send them to my net address

h1201kam@ella.uucp or h1201kam@ella.hu

Thank you

here's the original:
==========
I'm engaging myself, with my students, in a study of computerized behavior control models. As part of this activity, we are collecting/reviewing models other people have done.

Could anyone give me references on concrete brain/mind models based on CT?

(I'm relatively new to the list - since I am here there was no mentioning of such models). I would appreciate.

George Kampis

==========
Date: Sat, 4 May 91 18:23:44 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: UPPOWER@BOGECNVE.BITNET
Subject: Bye for now

Everyone out there: looks like I'd better pack the computer tonight if I'm to finish everything that needs doing tomorrow. So long to all, see you again as soon as possible.

==========
Date: Sat, 4 May 91 17:43:31 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: marken@AEROSPACE.AERO.ORG
Subject: Delays, Delights, Dependable

[From Rick Marken -- posting from home so I cannot edit or carry on for a long time because I am in real time]

Kent McClelland (910503) I would be happy to review your paper. Send it to me at
10459 Holman Ave
LA CA 90024
or post it to my email address
marken@aerospace.aero.org

Bill and Mary Powers (910504)

Sometimes I get the chill from the brilliance of Bill's stuff. This one really sent me. A week without Bill on the net will be eternity for me. I am also always surprised at the brilliance of Mary's posts when she deigns to do them. Could Mary be the one who is ghost writing all Bill's stuff? Anyway, while I agree that all should hearken to Marken, I think we should also send flowers to Powers (both) for the sheer intellectual joy they provide. I look forward to hearing from you both again soon.
Have a great trip.

Gyorgy Kampis (910504) I assumed that someone else would post the materials to you. I don't have time now but I promise, on monday, I will post a list of references on control theory that might be what you need.

To all: I am apparently controlling my perception of my input mail with a substantial lag. It's like steering the Exxon Valdez. I turn -- the ship responds 10 minutes later. Anyway, I did just get ACK (on saturday) of everything I posted on friday. Sorry for the double posts.

Continue having a great weekend.

Regards

Rick Marken

==========

Date: Sun, 5 May 91 12:01:46 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: Ed Ford <ATEDF@ASUACAD.BITNET
>
Subject: stuff

From Ed Ford

Greg is publishing for our late May newsletter Closed Loop. It will be sent to ALL PAID UP MEMBERS. Hint, hint!! Now that we have this network, I suppose it will be harder to get writers for the newsletter. Deadline is May 20th. The application form to attend our annual CSG conference in Durango will be in the May newsletter (Tom, please note).

Rick, we all love ya. It just takes time to digest the input, that's all.

Bill & Mary, if it's not to late, have a safe trip. Look forward to your first transmission from Durango.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, Arizona 85253

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Date: Sun, 5 May 91 19:03:50 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: "Gary A. Cziko" <g-cziko@UIUC.EDU
>
Subject: CT & Sociology MS.

[from Gary Cziko]

McClelland (910503):

> The draft contains the following: an introduction focusing on Jack Gibbs's

> recent book, CONTROL: SOCIOLOGY'S CENTRAL NOTION, saying that Gibbs missed
> the boat by ignoring Powers's Control Theory; a very brief but fairly
> comprehensive review of the multi-disciplinary literature on Control Theory

> (now a year out of date); a section attempting to explain the basics of
> Control Theory in hopefully not-too-simplistic terms to an audience of
> sociologists; a final section applying Control Theory to a discussion of
> interpersonal power.

> My question is whether there are people out there with the time or interest

> to look at the draft and give me some comments on where to go next.

I would like to see the draft, but I'm afraid that since I'm not a sociologist I wouldn't be too much help to you, but it sounds very interesting.

The best way would be for you to put your draft in electronic form (if it isn't already) and send it to my personal email address (see end of message). In the meantime, people on the list interested in seeing the ms. could let me know and I would forward the ms. to them electronically. While perhaps a bit more trouble at the start, it will be much more convenient and cheaper in the end. We have done this before with two versions of a ms. by Rick Marken and it worked quite well.

You will have to translate your ms. to an ASCII file if it is in a word processor format before uploading, but I'm sure that someone at your college could help you with this if you aren't sure how to do it.

I'm looking forward to receiving the ms. from you for distribution AS WELL AS HEARING FROM THOSE ON THE NET INTERESTED IN RECEIVING IT.--Gary

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Date: Mon, 6 May 91 08:21:46 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET
> Subject: Re: Query
In-Reply-To: Message of Fri, 3 May 91 17:46:29 cdt from <MCCLEL@GRIN1
>

Dear CSG'ers, (FROM CHUCK TUCKER (910506)
I have just seen the note from Kent McClelland about his paper. I received a copy of the paper from Clark and it is one of those papers that I say "Gee, I wish I had written this." I would recommend it to all of you who have an interest in taking the theororetical formulation to the higher levels. Please request one and comment on it.

By the way, nice to know that you are on the NET, Kent.
Regards,

Chuck

==========

Date: Mon, 6 May 91 10:35:03 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: cutmore@BEN.DCIEM.DND.CA
Subject: RE: the monkey and the coconut

[From Tim Cutmore]

It seems to me that one of the functions of "planning activity" in the effort to navigate a problem space from the current state to a goal state is to forecast or project the system through possible trajectories. In this way the system can "imagine" being in other states and compare these to the goal state for an improvement in its location in the problem space. Sometimes the system may have to produce large increases in the distance to ultimately reach the goal state. I am not that familiar with control theory to see how CT avoids problems of "local minima" or getting trapped in its attempt to always reduce error. Is there a natural way to include planning activity in a control system? One way that a neural network can escape local minima is to increase a parameter for random walk behavior (so called simulated annealing). Do control systems evidence properties like this?

A second, and perhaps related problem is one in which problem spaces interact.
The monkey and the coconut problem illustrates such a problem. For those unfamiliar with this example: To catch a monkey hollow out a coconut and leave a hole such that the hand can be inserted but not retrieved if an object is grasped inside it. Tie the coconut down and place a desirable object (food) inside. The monkey gets trapped because it refuses to release the object.

The problem of obtaining food (one of the important means of survival) interferes with another means of survival (not getting caught). It would seem that the monkey has been trapped in a local minima for surviving.

In CT language the problem appears to be one of trying to satisfy conflicting error signals - one tells the monkey to hold the coconut, the other tells it to let go.

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Date: Mon, 6 May 91 10:24:35 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
> Comments: Please Acknowledge Reception,Delivered Rcpt Requested From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET
>

Subject: CSG meeting

In (910505), Ed Ford mentioned the next issue of the newsletter, which will include the formal call for the meeting of CSG in Durango, Colorado, 14-18 August 1991. If any listeners on the net want more information about the meeting BEFORE the next newsletter, I believe Ed has some copies of the previous one, which contained much general information about the meeting, costs, opportunities for vacations and recreation in the Durango area, and more. Contact him if you want a copy.

Also, if anyone wants to be included on the program, which is structured rather informally at CSG, contact me as soon as possible. I will include your name on the list of participants in the general call. If you need written confirmation from me, on my university letterhead over my signature as president (to convince the people with the money that you are legitimate), contact me. Several people who joined in on CSG-L plan to attend. I hope more of you will decide to do that.

Best wishes,

Tom Bourbon <TBourbon@SFAustin.BitNet
>
Dept. of Psychology
Stephen F. Austin State Univ.
Nacogdoches, TX 75962 Ph. (409)568-4402

==========

Date: Mon, 6 May 91 18:17:00 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: m-olson@UIUC.EDU
Subject: referenced for error

One of the things that gives me pure enjoyment is when I learn something that "completely screws up my ideas about reality" (is this systems concepts or lower--I still don't have a good feel for exemplars of each level). Anyway, I notice that for most people I've met in my life, these sorts of things are extremely undesirable. They can't deal with the error, I guess. Hhmmm. Interesting...so I am saying that I deisire error?! Is this a contradiction? Probably not--would you say that a higher control system wants error in a lower one? If so, how would you draw that? Are there other examples of this phenomenon (at different levels).
My train of thought just brought me to the idea of desiring paradoxes, related but not quite the same. And from there I jumped to humor. Everyone likes humor. The kind I like (the Far Side) must create alot of error. SO WHY DO I LIKE IT? If life is all about error reduction, then why do I seek it?

Someone will probably say that anyone in academia desires error. Maybe, and that would be interesting to explain too. But I'm not speaking of simple assimilation here--I'm talking major accomodation (is there a difference?)

Do I ask too many questions when I write from the top of my head? Carpe' Diem,
--Mark Olson

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Date: Tue, 7 May 91 09:07:43 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD

From: "Gary A. Cziko" <g-cziko@UIUC.EDU
>
Subject: Purposeful Computers

[from Gary Cziko]

I'm reading a student's paper about computers and the thought struck me as to what extent computers are purposeful. To what degree and how do they control input in face of disturbances.

For those of you using a modem to upload and download messages to and from the net, there is the obvious example of error correction protocols like Kermit, xmodem and MNP5, etc. The sending computer gets feedback of some type from the receiving computer that the data has been accurately transmitted. If not (due to noise in the telephone line or my daughter picking up the extension a dialing her friend's number--teenagers dont' listen for dial tones), the "packet" of data is sent again until the input from the receiving computer to sending computer matches the reference level of the sending computer. In this sense, it looks like a purposeful system.
I wonder to what extent control systems like this are used in the internal functioning of a computer. I know that I can say "verify on" on my ms-dos machine when copying files, but what other control systems exist? I suppose there must be many others working within the electronics to make sure that frequencies, voltages and such remain within working limits.
Who knows more about this?--Gary

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Date: Tue, 7 May 91 08:38:58 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: marken@AEROSPACE.AERO.ORG
Subject: Cognitive Control

[From Rick Marken]

Jeffrey Horn (910503) David Coombs (910503) Tim Cutmore (910506) all responded to my comments about solving the water jar problem (cognitive control). I think your comments are right on target. Let me try to just give some control theory thoughts on this problem. First, let me say right off that control theory in itself is not going to provide any magic solutions to the problems of simulating cognition; it just might suggest some new places to look for solutions. Ultimately, cognitive control involves the control of cognitive variables -- relationships, programs and even principles. Since people are having trouble building systems that will reliably perceive even relatively simple configurations (like words) it is probably some time before we will be able to build systems that can perceive relationships between these configurations or programs of configurations or principles exemplified in sets of programs, etc. I say this only so that people don't say -- well, if control theory is so great why don't you build a control

system that solves water jar problems. I think the AI modellers are doing a great job on this.
Anyway, here are some general thoughts of a control theorist on "cognitive control". One thing to consider is -- what are we modelling? If we are modelling a person's ability to control the program that results in the goal amounts in the jars then we must prove to our satisfaction that we know what variables are being controlled when the subject solves a water jar problem and that the subject can, indeed, control those variables. Control means keeping variables in intended states. If the controlled variable is a program of moves, then the subject is controlling it if that program is carried out consistently. Different versions of the water jar problem (which are solved by the same program of moves) can be considered disturbances. If the subject can solve these problems (without making mistakes in carrying out the program -- eg, taking blind alleys) then the subject is probably in control of this kind of problem. Now the modelling can begin -- and it should be possible (though not easy) to build a model that will control this variable.

I think most problem solving simulations are modelling the behavior of subject's who are not really in control of the program that solves the problem. They know the moves that are possible (the relationship between one step -- problem configuration--and another) and they may even know that some program (network of contingencies) might solve it. They just don't know which. So they are in the midst of reorganization -- a process of randomly selecting moves or even program contingencies (like "if I could get a jar with 1 pint, then I could add it to the 3 pint jar"). These hypothesized program contingencies (unstated by the subject him/herself) are what are seen as the apparent "heuristics" that the subject uses. Since the subject is just reorganizing, it is not clear that he/she will hit on the heuristics that will solve the problem. Part of reorganization is "seeing things in a new way" and this mean seeing a contingency (like the one quoted above) that they may not have ever seen before. It depends on what kinds of program level control the subject already has as a resource.

Anyway, that's my first thoughts. We must first be able to distinguish (behaviorally -- by observing the variables being controlled) whether the problem solving behavior we are seeing is an example of the use of an existing set of control systems (ie. it is an example of control) or whether it is an example of trying to learn how to control (ie. reorganization -- learning to get the intended result with existing or newly constructed control systems). I think, incidentally, there are examples of controlled problem solving (the first kind). Psychologists studying "set" have had subjects solve the same kind of water jar problem (one that could be solved using the same program) over and over. The problems, which are very difficult a first (as indicated by the time and number of moves required) become trivially easy once the program is learned. Now give a problem that looks the same but requires a different program for its solution. The subject's have a hell of a time with it. They have to learn to control a new program. Their inability to give up attempts to make the "old" program work is called "problem solving set".

Just a couple more notes. The hierarchical control model is a "hill climber" in the sense that control systems only work to minimize error -- keeping perceptions matching references. By doing so, control systems at one level may be in "local maxima" with respect to the larger picture as seen by a system trying to control a higher order variable. When the higher order systems cannot solve their "problems" (keep their inputs matching their intentions) then they must reorganize -- just as is done is most problem solving models. This reorganization would be unnecessary if the hierarchy of control systems were organized properly (for solution of the problem at hand, that is) and finding such an organization should be possible. The higher level
systems must control a program that produces the correct changes in problem states contingent on the current state of the problem. Problem solving programs that work this way (and some do) are probably reasonable candidates for models of "skilled" problem solvers --at least they are candidates for representations of the program that is being controlled by the problem solving control system.

I hope this all makes some sense. I think my main point is that it should be possible to distinguish control of a program (skilled problem solving) from learning to control a program (reorganization in order to solve a particular, unfamilair problem). Once we know the pheneomena to be explained (in control terms -- the variables controlled) then we can do some productive modelling of problem solving behavior. Skipping past the phenomenon to concentrate on models (as I think AI researchers have done) can lead, I think, to some confusion about what a model of problem solving should be like. A return to the study of problem solving itself, as an example of the phenomenon of control (using "the test for controlled variables" to see what variables are or are not being controlled) might remove some of the confusion about what a model of problem solving can and cannot accomplish.

For those interested, I would suggest starting with the study of skilled problem solving--like the solving of algebaic equations by experts in algebra. This may seem trivial because the algebra experts already know how to solve the equation; it's easy. But I think there is possible gold here (maybe some of this has already been done) if we can test hypotheses (by introducing disturbances and looking for lack of effect) about the configurations, relationships, sequences, programs, and even principles that are being controlled (with skill) when these problems are solved. I havn't got any great ideas about how to do it -- but I've got a great subject -- my math wiz teenager. Ah, a new way to make daddy even more obnoxious suddenly occurs to me. Naa. I'll leave it to you folks to do the research.

Hasta Luego

Rick M.

****

Richard S. Marken USMail: 10459 Holman Ave The Aerospace Corporation
Los Angeles, CA 90024 Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)

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Date: Tue, 7 May 91 10:55:41 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: BARKANA@DUPR.OCS.DREXEL.EDU

[From Izhak Bar-Kana]

We have been having troubles with our mail, so I hope this one goes through.
Rick said,

>

> Gee. I guess I did offend everyone.

>
Not me either.
But the question was how to get human relationship among human beings. If one feels that God helps him to be a mentch, I have no objection. Furthermore, in a world where slavery reigns supreme, I wish someone would write a code for slaves' rights: no killings, one day

rest a week, etc. I wish such rights were respected 2000 later. And also in a society where human sacrifices are the rule, if someone decides: No more! (Abraham), it's not to bad. So, if one needs God for that, or to be trusted by others, I have no problem.

I agree with you that this does not protect us from the other aspect: when man uses God as a reason for opressing or killing his Brother Man. For this reason, I would rather each one believe in himself and in Man. This is not easy: instead of liberating, some atheistic movements only replaced God with some earthling. And I respect the religions, at least that I know, for trying to teach people that if YOU are not God, neither is anyone else around here. I do object your taking my words and arguing with arguments that are not mine. And, again, I am not sure we call same names to the same things. When you mix free competition with stealing, something is wrong here. Bill Powers may have a good theory, and I may see that people respect him for that. I may see that he is very successful, at least within this small Universe called CSG. I may try to do better, and this is all competition is about. If I try to steal his ideas, then I am a thief. I may try to call him names, I may become violent, but this has nothing to do with free competition. May be this is related to the modern trend in sociology "why ain't I entitled to the same things?" Second, I am not interested in the public opinion about violence, as I am not interested in the public opinion about education, drugs, etc., ESPECIALLY in this country. As a simple engineer, I am interested in deeds. My friend, you may be killed in front of a lot of people, and no one will interfere. Even worse, they will run away...from the police, so they would not get involved, become witnesses, etc. The amount of violence in this country, that people seem to get used with is unbelievable. A guy from Beirut was scared to death in the streets of an American city (no names). The American soldiers were lucky to be sent to the Middle East. 25000 people will be murdered this year here, not to mention other forms of violence. So, please, no polls. And now, you may also want to listen to the following:

Bill Powers:
Good luck in the new home.

I have been far from my computer for a few days.

Now, you seem to repeat some arguments that I was trying to use when I started writing to this group, and I felt that we call same name to different things, and vice versa. But I understood from you, and even more so from Rick Marken, that things are much more profound.

I am sorry, bu all my (engineering) life I have been used that "input controls" and "output is controlled." The "control system" includes everything, and, of course, the plant. The part of the control system that controls the plant is the "controller." Now, if the input to the control system (to the controller, and through it, to the plant) is not zero, it will affect the plant. If it is zero, it will not affect the plant. In a closed-loop system, the input is obtained by the difference betwen the reference input (in tracking system, control systems with feedback gain one, it is also the desired output) and the measured output. It is clear that the control signal, the signal that affects the controller and the plant can be only the measured value of the real signals. Similarly, biological systems can only use the sensorial perception as CONTROL SIGNAL, to affect their control system, and all the various stages and values, up to the value which is called "the controlled variable." I really don't understand why this language, which I understand you do know, had to be changed, in such a way that Rick Marken cannot even talk to what I call a control guy, because the old fashioned engineer cannot accept the idea that any control system controls its input. Now, we are in a closed-loop, and you can again change the order. I think it is regretful that it separates you from the general family of control research. More so, since we do want to learn about the behavior of

organism and of the brain from psychologists. After the time I spent reading your letters I can ignore the linguistic differences, or at least try to, and try to get the ideas, because I don't know a better group and discussion. But what do you have to speak French in the middle of English?

Besides, the most intelligent system I may dream to design, does not come even close to the simplest organism. In my humble opinion, again, one of the reasons for the huge and not always motivated (apparently) redundance in the organisms is intended to prevent an ocasionally wrong measurement (or input feedback) from replacing the correct output that the control system is meant to control.

Izhak Bar-Kana
Visiting Professor
ECE Department
Drexel University
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Home: (215) 649-2901

==========

Date: Tue, 7 May 91 12:03:50 -1100
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU
>
Subject: Re: referenced for error

> Interesting...so I am saying that I deisire error?! Is
> this a contradiction? Probably not--would you say that a higher control
> system wants error in a lower one? If so, how would you draw that? Are
> there other examples of this phenomenon (at different levels).
> If life is all about error reduction, then

> why do I seek it?

Sounds like you have a reference level for "open-mindedness."
Joel Judd

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Date: Tue, 7 May 91 14:30:33 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD
> Comments: Please Acknowledge Reception,Delivered Rcpt Requested From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET
>
Subject: Manuscript,Control of input

GARY CZIKO: Please send me a copy of Kent McClelland's ms. I had requested a paper copy directly from him, but this is easier. (Kent, I'll still accept the paper, if you think figures, footnotes and the like would help.)

IZHAK BAR-KANA (910507). You have the respect of those of us who labor to understand LIVING control systems. I am certain your life would be simpler were you to decide that we are a bunch of misguided nuts who cannot get our control-system diagrams and labels right! Perhaps I am wrong, but part of the problem that occurs when you speak of engineering (designing and building) a mechanical control system and we speak from the perspective of trying to describe and explain the control that is created by the living things we find already acting in the world, is we can't design and know all about the living systems. What is more, the variety of control theory we are trying to develop must compete with a host of already-established and widely-believed theories and disciplines, so we must direct most of our effort to persuading followers of those disciplines that there is even anything out there to notice that is different from what they already know. It IS unfortunate that, in the process, some of what we say seems wrong to the part of the engineering community that is probably closest to us.

Living control systems were not designed by us: we found them inhabiting a world that had already buried them in a host of sciences and disciplines -- the life sciences, social sciences and behavioral sciences -- recently joined by the neurosciences, cognitive sciences and many, many more. For the most part, the practitioners of those disciplines and sciences do not recognize that living systems control ANYTHING. Rather, they speak of the behavior (actions) of living things as CONTROLLED BY antecedents, whether they are from the environment (eg., stimuli, contexts, gods, societies) or from somewhere inside (eg., mind, soul, schema, plan, commands from the motor cortex). The invoke linear cause and they reject control by living things.

I have more to say on this, but I must stop for a while to help resolve a crisis in the academic life of one of my students. I will compose and send the remainder after a while. Izhak, I hope you will bear with me until then.

Best wishes,

Tom Bourbon <TBourbon@SFAustin.BitNet
>
Dept. of Psychology
Stephen F. Austin State Univ.
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Date: Wed, 8 May 91 00:40:27 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: More to Izhak

[from Tom Bourbon]

To continue my earlier post (Bourbon, 910507a), in reply to Izhak Bar-Kana (910507), control theorists who try to understand living control systems must contend with behavioral and life sciences which, for the most part, deny that living things control anything. But we recognize that living things are living control systems -- that they act on their environments to create and maintain things.

All that a living system knows of "the world" is its own sensory experiences of the world, so it follows that all a living control

system can control, from its own perspective, is its own sensory experiences. And there is abundant and conclusive evidence that sensory experiences do not correspond DIRECTLY with the environment. Perceptions as simple as those of brightness, hue, loudness, heaviness and the like reflect states of the perceptual apparatus of the organism -- DIRECTLY -- and they FAIL TO CORRESPOND DIRECTLY with any unique state of the environment. The state of adaptation of sensory receptors, the surrounding stimulus field, the relative sizes or magnitudes of different elements of the stimulus field, the relative temporal durations of stimulus elements and many other variables can combine in many different ways to produce the SAME perceptual experience. Hence, a person, like any other organism, can have the same perceptual experience in the presence of a near infinite array of different combinations of elements in the environment and in the organism's own physiology.

Because perception does not correspond one-to-one with any unique state of the environment, it follows that an organism which acts to control its own perceptions is not controlling a unique state of the environment, hence is not producing (controlling) a unique state of its actions (output). The specific actions of the organism, and the remote environmental consequences of those actions can vary dramatically, yet the organism experiences uniform percpetions. And it is certainly true that an organism that produces always the same actions and remote consequences in the environment will experience variable, not constant and controlled, perceptions.

In your engineering applications, zero input (by that do you mean zero perceived error -- a state INTERNAL to th eorganism?) leads to zero output. But an organism which adopts a new reference to experience an absent perception experiences zero perceptual input, which creates in the organism a non-zero error, which drives the behavioral actions (output) of the organism to create the desired perceptual experience, which does not, for a perceiving organism correspond directly with an objective state of the environmant. A bird with a reference to sense a not-yet constructed nest experiences zero perceptual input of nest, and it acts until it experiences that perception. And a sculptor who decides to sculpt a bird on a nest experiences zero perceptual input and acts until that experience exists -- whether any other person recognizes the finished sculpture as bird-on-nest, or not. To the artist, that is not important (not even if the artist must sell the sculpture to buy food -- all that matters is that someone else desire the sculpture and pay what the artist asks).

Those are the kinds of control we find in the world of living control systems. The best we can do is look for situations in which the variables through which the organism or person achieves its control of perception are also sufficiently stable from OUR perspective that we have a clue as to what the organism or person is controlling -- in its own perceptions. Certainly the one we observe is not controlling our experience -- not as its primary goal.

Interestingly, it is true that the category of humans known as control system engineers DO enjoy a privileged position relative to the control systems they design, construct and study. They do know the references and the "objective" states of the relevant variables in the environments of those systems. In fact, what the control system engineer intends is that her or his

perceptions of the states of those variables in the environment of the artificial control system will match her or his chosen reference. In that context, it is easy to understand why the engineer would speak of the artificial system controlling ITS output -- what the artificial system represents is a way for the designer and builder to control HER or HIS perceptions, relative to her or his references.

I do not know if any of this helps, Izhak. If anything I say violates too many of your ideas about control processes, please tell me.

Best wishes,

Tom Bourbon <TBourbon@SFAustin.BitNet>
Dept. of Psychology
Stephen F. Austin State Univ.
Nacogdoches, TX 75962 Ph. (409)568-4402

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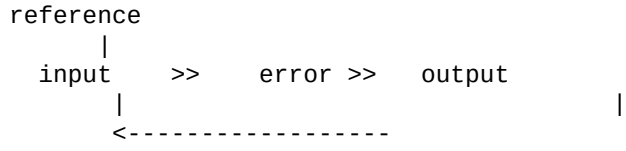
Date: Wed, 8 May 91 08:23:50 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Control of Perception, Social Problems

Itzak (910507)

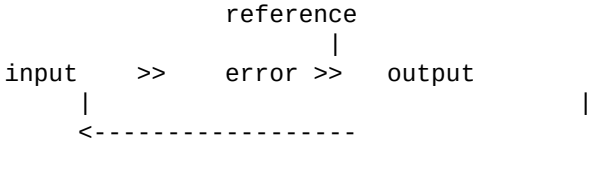
The difference between input control and control of input is not just a language difference -- its the whole mishuguyas (sp?).

Here is the model of a tracking task for an engineering psychologist



The organism is between >> and <<. In other words, the organism experiences error due to the discrepancy between an objective reference and input event (the target and cursor in tracking; sometimes the error itself is considered the stimulus).

Here is the same model for a CSG control theorist



Now both the error and the reference are INSIDE the organism. The reference can be adjusted by the organism (by higher level control systems) so the organism determines what constitutes an error; the organism is in control of the environment, not vice versa -- a rather significant difference. The difference accounts for the appearance that organisms can voluntarily change the value at which an environmental input variable is controlled -- it's as though the therostat suddenly decided to keep the room at 65 rather than 72.

This is the phenomenon that control theory is trying to point to -- voluntarity or, better, purposefulness.

The controlled environmental variable is probably what you call the output that is controlled by the system. That's fine -- but, of course, it is this output, AS PERCEIVED BY THE ORGANISM, that is controlled, not the output itself. With organisms there is no independent means of checking the validity of the perceptual representation of the environmental variable that is being controled-- all the organism gets are perceptions of the environment. We cannot look past our perceptions to see if we are controlling what we intend to be controlling (as you do when you design a control system and make sure that it is controlling what YOU intend for it to be controlling; you can look beyond the sensors, the control system itself cannot). So, for a living control system, reference states of perceptions (not environmental outputs) ARE the intended ends of control actions.

Note, by the way, that the mathematics of the engineering psychology and the CSG psycholgy approach to control are nearly the same (at least, control works in both cases). The difference is in where you put the variable r (the reference signal). That's all there is to it. Small step for control theory; giant leap for understanding the nature of living systems.

I have been trying to work on a demo to illustrate that it is perception that is controlled. Yesterday I wrote a program that displays the projection of a rotating trapaziod. The direction of motion of this projection is ambiguous because people tend to see it as the projection of a rotating rectangle. My thought was to have people control the angular velocity of rotation. At points where there is an aparent shift in the direction of rotation, the polarity of control should suddenly shift. Thus, a change in perception results in a change in control. The problem so far is that the rotating motion is too ambiguous -- it is hard to perceive the projection as doing anything other than oscillating back and forth. I have other ideas that I plan to try out. By the way, Wayne Hershberger did a wonderful study to illustrate the fact that it is PERCEPTION and not environmental OUTPUT that is controlled by living systems. This work is described in the following paper:

Hershberger, W.(1987?) An approach through the looking glass. Animal Learning and Behavior, 14(4), 443-451

Maybe Wayne could give a brief summary for us; it's really a cute study (if you're not one of the chicks who get's it's world reverse).

As far as social issues, I'm sure we both have the same hopes and goals for a humane society where everyone can lead satisfying lives. I'm sure there are many obstacles to this goal but I rate racism, poverty, prejudice and ignorance as much greater problems than a decline in values. In fact, my impression is that the winners in society (the rich, the powerful, the majority, the lucky) are always the first to point to a deterioration of values amongst the masses as the source of societal problems, even as they pig out on the very resources that these poor valueless slobs need to get along (at least from their perspective -- perception again). I guess that, besides being a war monger I'm a bit of a bleeding heart liberal too. Go figure.

Hasta Luego

Rick M.

****

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Date: Wed, 8 May 91 08:31:24 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Control of Perception a la Tom

[From Rick Marken]

Tom Bourbon (910508)

I wrote my note to Itzak before I knew of yours. It's nice that we said nearly the same things. Either we both finally understand this stuff or we are equally befuddled. Nice work.

Best Regards

Rick

****

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Date: Wed, 8 May 91 10:35:05 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Warning -- original Sender: tag was
From: "Manoj K. Jain"
<ames!scapa.cs.ualberta.ca!manoj%harvard@HARVUNXW.BITNET>
Subject: sign off

signoff csg-l

==========

Date: Wed, 8 May 91 15:40:19 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: Re: referenced for error

>Sounds like you have a reference level for "open-mindedness."
>
Joel,
That's what I would call it too. Now, what does that MEAN in CT terminology? Would this mean that an open-minded person (OMP) would be referenced for error, or that an OMP would be the same as a nonOMP except that the former has a greater tolerance for error, or neither?

On another topic:
In one of my classes I just learned that Universities in the 60's would conduct experiments with LSD (and maybe other drugs). This was news to me, given that I was maybe an infant at the time. Does anyone know what Psychology learned from this? Specifically, does it bear any insights into CT? I'm curious whether different drugs have their effects on specific parts (perception, output, comparator, error signal) and whether any are level-specific. For example, alcohol seems to have its effects on the lower levels of the hierarchy and seems to mess up the output; whereas LSD seems like it would have its effects on higher levels (when the world doesn't make sense that sounds like a Categorization problem) and mess up the error signal (tell the system there is always error or maybe tell it there is no error), and probably the perceptions. Any ideas?

Oh, heck, I might as well throw in another esoteric question...with LSD there seems to be something like a loss of sense of self (of "me"). Maybe I'm wrong, but it brought to mind what CT has to say about what consciousness is. No one has to answer that cause I know the theories, but does CT have anymore to say on the topic. I've always kinda interpreted the Fall in the Garden story as an allegory for when Man acquired a sense of self. Given Bill's comments last week on the evolution of the Systems level, it seems like there might be a connection there. I know I'm asking the big questions, but that's the goal of psychology so I don't think its out of place here.

(And if someone could weave in why people on LSD lose a sense of time, that would be wonderful. Why does time go faster for children and LSD'ers?)

Carpe' diem
Mark Olson
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Date: Wed, 8 May 91 16:17:44 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: McClellan Manuscript

[from Gary Cziko]

The manuscript "Perceptual Control and Sociological Theory" by Kent McClelland has been sent electronically to Bourbon, Lubin, Marken, McPhail, Powers and Tucker for comments.

If anyone else would like a copy, please send me a personal note. The manuscript is about 115 kilobytes long.--Gary

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==========

Date: Wed, 8 May 91 15:36:44 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: more stuff

From Ed Ford

Gary - Would like a copy of the McClelland manuscript.

Mark & Joel - Your thoughts on intentional error were most intriguing. We create error for a variety of reasons: because we enjoy the process of reducing it as well as the outcome (writing a book, creating a model on the computer); though the process is painful, we want the outcome (eating less to enjoy slim figure, floss teeth and go to dentist to enjoy continued use of teeth). Any other thoughts?

Fred Davidson and all newcomers - The CSG Newsletter is sent to all members. Membership is $25 a year with check payable to: CSG. If you plan to attend the conference in August, your dues for the following year are automatically added to the conference fee. Anyone wanting a copy of the last newsletter, put your request on the CSGnet and be sure to add your address and phone numbers (for our files).

Tom and Rick, liked your thoughts on CT.

Last words on religion and competition: My reference to a high power or religion was only to establish AN EXAMPLE of a system of values (systems concept level), a system that varies with each individual, from mere lip service, to control or to harm others, to genuine concern for others. Within our CSG, we have establish an unusually high degree of rapport because we have all accepted similar values and standards. It isn't the values themselves, but our (to quote Bill) attitude or perception of our individual goals and wants that determines how each of us deals with each other.

And yes, faith (maybe a misused word) can be based on fact. My belief that Geroge Washington lived is based on fact. So is my belief in the basic message and messenger of the particular religion I adhere to. That also is based on fact (just look at today's date).

Rick, your comment that it took "years for western society to free themselves from this source of conflict" is most interesting. Our faith in a higher power doesn't improve our ability to deal more equitably with others unless we translate those values to standards and decisions in a way that respects the internal control systems of others. Unfortunately, people have used these ideas as an excuse to control, abuse, and manipulate others (to quote Shakespeare "even the devil can cite scripture to his means). As a control theorist, what makes any living systems concept valid is that it has as its basis a

respect for the choice making ability of others, or the control system that resides in all of us. I really intended to use my words as an example of a systems concept in my discussion about competition, not create an issue about the validity of religion.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
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Date: Thu, 9 May 91 10:24:27 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: more stuff

[from Joel Judd]

>We create error for a variety of reasons: because we
>enjoy the process of reducing it as well as the outcome (writing a
>book, creating a model on the computer); though the process is
>painful, we want the outcome (eating less to enjoy slim figure, floss
>teeth and go to dentist to enjoy continued use of teeth). Any other
>thoughts?

Well, doesn't this assume a HIGHER level that "enjoys" error? That was my only thought. We also tend to automatically attribute negative connotations to "error" don't we? But one man's error...

>And yes, faith (maybe a misused word) can be based on fact. My
>belief that George Washington lived is based on fact.

>Our
>faith in a higher power doesn't improve our ability to deal more
>equitably with others unless we translate those values to standards
>and decisions in a way that respects the internal control systems of
>others. Unfortunately, people have used these ideas as an excuse to
>control, abuse, and manipulate others (to quote Shakespeare "even the
>devil can cite scripture to his means). As a control theorist, what
>makes any living systems concept valid is that it has as its basis a
>respect for the choice making ability of others, or the control
>system that resides in all of us.

Well said. Since this topic is still alive, I'll repeat what I said last fall about the initial attraction of CT, and that is its inherent respect for one's autonomy. Apart from the practical and conceptual shortcomings of behaviorism/cognitivism, what I dislike the most about them is the way they ultimately tend to take away one's choice, or at least responsibility, since we are just reacting to stimuli. My own religious beliefs are centered around the concept of "free agency," and CT just confirms my belief that we are all free to choose. Freedom, of course, doesn't mean 'anything goes,' but it's in deciding what goes and what doesn't that groups of people get into trouble.

Joel Judd

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Date: Thu, 9 May 91 08:30:45 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

From: marken@AEROSPACE.AERO.ORG
Subject: Error Production, Disciplined Imagination

Mark Olson (910508)
asks how people's apparent interest in producing "error" for themselves fits into the control model.

Ed Ford (910508) suggests that:

> We create error for a variety of reasons: because we
>enjoy the process of reducing it as well as the outcome (writing a
>book, creating a model on the computer); though the process is
>painful, we want the outcome (eating less to enjoy slim figure, floss
>teeth and go to dentist to enjoy continued use of teeth). Any other
>thoughts?

I think this term "error" is an unfortunate one. In control theory, "error" is just an unsensed signal representing the magnitude of the discrepancy between an intended and an actual state of affairs. In a normally operating control system error is always VERY small. Control systems always act to REDUCE error. Error is one signal in a causal loop; the only way these errors can be increased is for the causal influences that propagate around the loop to net out to having a net positive sign -- ie positive feedback. When this happens, there is no control. So I don't see how intentional increases in error can be incorporated into the control model and have the model behave like most people behave -- with stability. I think the problem is that there is another, informal meaning of "error" which is synonymous with something like "pain". In this case, I think the term error is describing a perception (or, at least, an aspect of a perception) and there is no reason why people cannot set different reference levels for this perception. For example, in that great early scene in Lawrence of Arabia, Lawrence puts out a match with his fingers (without flinching). The fellow watching tries it and, of course, goes OUCH and asks "what's the trick?" and Lawrence says (great movie line) " the trick is not to MIND that it hurts". Now Mark and Ed might say that Lawrence was getting an error signal and not minding it. I would say that Lawrence had set a reference for experiencing pain that was quite different than his friend's. He got what he wanted at the level at which he was doing the behavior. As a side effect there was probably intrinsic error (also, I think, unsensed except in terms of side effects).

So, if we take the control model seriously, I think we would have to say that, as it sits now, it says people do not act to intentionally produce error (a discrepency between a reference and perception) but they can act intentinoally to produce unpleasant (for other people) perceptions, probably to satisfy higher level goals (as Lawrence was doing, validating whatever weird system concept he was trying to validate with his masochism). Thus, I like Joel Judd's explanation of Mark's willingness to experience "error" -- its a particular system concept that Mark has that others may not have.

The only time that the control model opens itself up to increased error (in the technical sense of increased discrepency between reference and actual perception) is when it reorganizes. But the increased error that MIGHT result from reorganization is not intentional -- because reorganization doesn't try to increase error. It is just randomly changing control systems, possibly for the worst. But it won't keep bad solutions for long and the ultimate goal of reorganization is the reduction of intrinsic error.

Of course, it is possible that people do act to intentionally increase error (in the technical sense of error). But I would have to have to see a clear demonstartion of that phenomenon before I would start trying to make rather substantial changes in the control model to accomodate those observations. The closest thing I have seen to what looks like intentional production of (technical) error is found in my article with Bill Powers in Hershberger's Volitional Action book. In the polarity reversal experiment there is a 1/2 second period where the subject actually makes things WORSE -- increasing the discrepency between target and cursor in an accelerated, positive feedback sort of way. When you are a subject in this experiment you can actually feel it happening "against your will" -- it is moderately unpleasant because you are not only losing control buy YOU yourself (a lower level system of you) are the one doing it. But this happens only because the higher level systems cannot correct things fast enough. It is explained just fine as the behavior of a two level negative feedback control system that is trying (as always) to minimize (technical) error.

There may be cases where people are able to take control systems "off-line" so that their technical error is not corrected. I think this happens in hypnosis. But I can't think of an example of behavior that involves intentional creation of error -- this would mean deliberately setting up one of your control systems for positive feedback -- and, thus, exponentially increasing error. The error that Mark is after is under control; it is, I believe, another pole of perception, a pole that may have intrinsic consequences (and, is therefore, considered unpleasant by most people) but it is just a perception nevertheless. It is not a "judgement" that something is wrong (as technical error is) -- it is simply the representation of a state of affairs.

This topic does need some thought. Hope this helps.

Regards

Rick M.

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Date: Thu, 9 May 91 11:05:18 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Correction

[From Rick Marken]

In my previous post I said:

asks "what's the trick?" and Lawrence says (great movie line) " the trick is not to MIND that it hurts". Now Mark and Ed might say that Lawrence was getting an error signal and not minding it.

What I meant was that Ed (and possibly also Mark, but he asked about it, Ed suggested an answer) might say that Lawrence was intentionally creating an error. I don't think he was intentionally creating "technical" error, though he may, indeed, have been ignoring it (as in hypnotic pain surpression). But I think he was intentionally producing a perception of pain (which is non technically caller "error" but is not the same as error in a control loop).

Back to work.

Rick M.

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Date: Thu, 9 May 91 13:08:00 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Perceptual Control Theory

I've started to read Kent McClelland's manuscript. He provides a very nice intro to control theory; I'm just getting into the sociological stuff and so don't have much to comment about this yet.

However, I like very much the name he has chosen for the discipline that unites the people this network. He calls it PERCEPTUAL CONTROL THEORY (PCT for short). I think this a great name, and it distinguishes our psychological concerns from the engineering ones.

I've never liked the label control theory very much since it often first means to newcomers the opposite of what it is all about (Hey, a new theory for controlling people!). Perhaps we should rename ourselves the PCSG?--Gary

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Date: Thu, 9 May 91 15:32:40 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: BARKANA@DUPR.OCS.DREXEL.EDU
Subject: re: Perceptual Control Theory

Gary A. Cziko (910509):

On: "PERCEPTUAL CONTROL THEORY" name.
I, for one, definitely agree!

Izhak Bar-Kana
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Date: Thu, 9 May 91 13:13:45 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: PCSG, Control of output

[From Rick Marken]

Gary (910509) -- Yes, great name. Perceptual Control Theory. Perfect.

I am also only starting McClelland's paper. It really is quite good (if you are listening Kent, nice going. Where do you plan to try to publish it? It really is very good.)

Incidentally, Kent's discussion of "controlling perception rather than input" in the paper made me think of a couple of points. First, when you point this out you should make it clear what you mean by "controlling". Controlling means "causing a variable to stay at a specified value". Perception is controlled because it is caused to stay at the value specified by the reference variable. Output is not controlled because it is not made to stay at some specified value. The only system variable that could conceivably be a reference for the output is the error variable. But, in fact, to the extent that the output is "controlled" at all, it is controlled by something outside the system -- the disturbance (this is easily derived from the feedback equations: setting the reference at 0 we get

O = -[k2/(1+k1k2)]d

where O is output, d is external disturbance to the input and k1 and k2 are input and output scaling factors, respectively. So an external variable "controls" the output. But this is not completely true because we have set the reference to a constant, o. If the reference is not a constant, then we can see that some of the variance in the output is INFLUENCED (not controlled) by the control system

O = Gr - k1Gd

where G = k2/(1+k1k2) and r is the reference variable (signal, whatever). If k2 (the output factor) is large and >> k1 then the output is proportional to the sum of the variances of r and d. So varying r does not really control O because the value of O is also determined by d. So O, what we call the "action" or sometimes the "behavior" of the system is not really controlled by the system or the environment (when the reference is not fixed). Both the system and environment have a causal influence on O, just not a controlling one. But p(the perceptual variable) which is influenced by both d and O, is ALWAYS controlled relative to r.

To get really picky, even the error signal is not a controlled output of the control system. Disturbances (such as neural noise) can be added to the error so that error is no longer proportional to (r-p) -- the difference between reference and perceptual signal. Rather error = r-p+de (where de is the

disturbance to error). Error variability will depend mainly on de and d (the environmental disturbance) so the system doesn't even control a variable (error) that is inside itself. The only variable in a control loop that is controlled is p, the variable the is subtracted from the reference.

Amazing, but true.

Back to work again. Boy, this is sure a hell of a lot more fun than work (and probably more worthwhile too. sigh)

Regards

Rick M.

****

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Date: Thu, 9 May 91 13:37:22 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Correction Again

[From Rick Marken]

Silly me. I said:

>Incidentally, Kent's discussion of "controlling perception rather than
>input"

That obviously should be "controlling perception rather than OUTPUT"

Sorry

Rick M.

****

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Date: Thu, 9 May 91 16:03:52 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: PCSG, Control of output

[From Gary Cziko]

Rick Marken (910509b):

I'm glad you too like Kent McClelland's new name: Perceptual Control Theory. Won't Bill Powers be surprised when he gets to Colorado to find we've changed the name of his theory!

Concerning control, you said:

>First, when you
>point this out you should make it clear what you mean by "controlling".
>Controlling means "causing a variable to stay at a specified value".
>Perception is controlled because it is caused to stay at the value
>specified by the reference variable.

I wonder if this is not a bit to restricting. Shouldn't we say something like "controlling means causing a variable to stay at a specified value or take on a specified pattern of values over time"? Yes, I realize that the repetitive pattern of perceptions I get as I jog are represented by a single higher-order value for jogging which we assume is constant (until a still higher system tells me to run faster), but can't we say that at a lower level control need not be limited to a single value? Otherwise it starts sounding a bit too homeostatic to me.--Gary

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Date: Thu, 9 May 91 17:30:55 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Error Production, Disciplined Imagination

Tick Marken (910509):
>The closest thing I have seen to what looks like intentional production of
>(technical) error is found in my article with Bill Powers in Hershberger's
>Volitional Action book. In the polarity reversal experiment there is a
>1/2 second period where the subject actually makes things WORSE -- increasing
>the discrepancy between target and cursor in an accelerated, positive feedback
>sort of way. When you are a subject in this experiment you can actually feel
>it happening "against your will" -- it is moderately unpleasant because you
>are not only losing control by YOU yourself (a lower level system of you)
>are the one doing it. But this happens only because the higher
>level systems cannot correct things fast enough. It is explained just fine as
>the behavior of a two level negative feedback control system that is trying
>(as always) to minimize (technical) error.

There is a fairly common real-life parallel to this effect that has always interested me. I tried to get the people in this human-factors institute to explain it to me when I was a student here 30+ years ago, and I still haven't found anyone with an answer. Maybe CSG people have an answer.

Here's an example situation; there are parallel situations where the same thing happens, but I think one example is enough:

In a certain corridor there is a doorway with two swinging doors (i.e. when both are open it's a very wide doorway, but opening one is all one needs to do to pass through.) Usually, one of the two doors is locked shut, and people pass through the other. It is always the same one that is locked. The first time someone encounters this doorway, they may go through the openable door, and if so, everything is OK thereafter. But if on this first encounter they try the locked door, and then move to the unlocked one after failing to get through the locked one, there is trouble. For a long time thereafter, even if they use the doorway several times a day, they are liable to try the wrong door first. The subjective impression (it has happened to me a few times) is that one mentally oscillates "I know it isn't the one I first thought it was, which means it is not that one, because I think it is that one, so it must be the other one....BANG!" It seems that the more times one goes through this routine, at least for perhaps tens of experiences, the more likely it is that one will eventually choose the wrong door. It's a very frustrating thing, very common (other people confirm it happens to them, and it can be observed casually), and without any explanation that has satisfied me yet. Is there a CT explanation? It sounds a bit like Rick's experimental situation, though that corrects itself more quickly.

Martin Taylor (mmt@ben.dciem.dnd.ca)

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Date: Thu, 9 May 91 14:58:30 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Control

Gary Cziko (910509b?) says:

>I wonder if this is not a bit to restricting. Shouldn't we say something
>like "controlling means causing a variable to stay at a specified value or
>take on a specified pattern of values over time"?

Of course, yes. I should say that, but I assume it. It's just so obvious (and important) to me that the specification, r, is a VARIABLE. So to me, the specification for a controlled variable can be a variable, just as the cause of the value of a dependent variable can be a variable. I did take the fact that r is variable into account in the derivations I reported. But thanks for mentioning it. Perceptual control theorists should always remember that references (intentions, specification, purposes) or whatever you want to call them, are variables, not fixed "set points" -- though, of course, they can be fixed for some time if that satisfies some higher order purpose.

By.

****

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The Aerospace Corporation Los Angeles, CA 90024
Internet:marken@aerospace.aero.org

213 336-6214 (day)
213 474-0313 (evening)

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Date: Thu, 9 May 91 16:24:41 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: hello in passing, definitions

[From Bill Powers in Limbo]

I'm at my son Denny's house. I do not have a house. Neither do I have a place to stay in Durango (until Saturday). Homeless! Denny is setting me up with a way to upload and download via his logon at CU, so it looks as though I will have a backup right away. I'll catch up later via the archives, but a quick comment --

Rick Marken [9105??] --

There are three terms we need to keep straightened out: influence, determine, and control.

INFLUENCE: A influences B if A is ONE of several variables on which the state of B depends. The engineer's hand on the throttle INFLUENCES the speed of the train; the speed is also influenced by the brake lever and by the slope of the tracks.

DETERMINE: A determines B if, given A, B is completely predictable (i.e., B depends on A and ONLY on A). The path of a train is normally determined by the configuration of the tracks.

CONTROL: A controls B if, for every disturbance applied to B, A changes its influence on B in such a way as to counteract the effect of the disturbance on B.

From the definition of control just given, we can see that a disturbance influences but neither determines nor controls the output of a control system. We can see whether a disturbance D controls the control system's output by applying the test for the controlled variable. First we apply D and observe that the control system's output changes equally and in the opposite direction. Then we apply a test disturbance Dt directly to the output variable, of sufficient magnitude to cause a measurable change in the output. Will the first disturbance, D, then alter in such a way as to restore the output to its former state, counteracting the effect of Dt? No. So neither the first disturbance nor the test disturbance controls the output of the control system.

Well, more from Durango. "Perceptual Control Theory" sounds OK to me.

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Date: Thu, 9 May 91 21:47:41 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Perceptual Control Theory
In-Reply-To: Message from "Gary A. Cziko" of May 9, 91 at 4:03 pm

> I'm glad you too like Kent McClelland's new name: Perceptual Control
> Theory. Won't Bill Powers be surprised when he gets to Colorado to find
> we've changed the name of his theory!

I'm not sure that I have standing or knowledge to offer a serious critique, but I do have a thought against the term Perceptual Control Theory (PCT). Something that has always struck me about CT, and have discussed with Bill on this list a few months ago (I can look up the reference), is the "prejudice" towards concentrating on Control in HUMAN, COGNITIVE systems, and thus of regarding CT as a kind of psychological theory. Bill suggested that this was a result of an accident of history and a lack of hands to broaden CT applications. CT is such a vast, systemic theory that this is a serious limitation. CT concepts not only apply, but are critically important, to most if not all levels in living and artificial systems.

In particular, we must see basic metabolic and biochemical genetic processes, primitive neurological systems, and the origin of life itself as the origin of levels of Control: every living systems is a natural control system, and every control system, even the thermostat, rests on a living organism. Responding to Marken's definition, we must see Life itself as a controlled variable maintained against constant environmental pertubation: hunger is error. This links us to Schrodinger's negentropy definition of life and the self-organization school of anti-equilibrium thermodynamicsts: control is the maintenance of a higher level dynamic equilibrium against the inexorable drive towards lower level thermodynamic equilibrium, life against death.

I'm ranting. It's just that these are my concerns, and Control Theory is becoming an important way in which I conceptualize these ideas. They have little to do with "Perception" per se. Perception is a high level psychological construct. The thermostat does not "perceive" the room's temperature; I doubt that the amoeba "perceives" the chemical gradient it's traversing, and may not even "sense" it. Control Theory applies to all these systems. Control Theory, like all the Systems Sciences, must be INCLUSIVE OF and ORTHOGONAL TO academic discipline and system composition and level. Control Theory is not JUST Perceptual Control Theory. Control Theory is NOT a branch of Psychology.

O-->
| Cliff Joslyn, Cybernetician at Large, cjoslyn@bingvaxu.cc.binghamton.edu
| Systems Science, SUNY Binghamton, Binghamton NY 13901, USA
V All the world is biscuit shaped. . .

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Date: Fri, 10 May 91 08:40:02 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Control Definitions

[From Rick Marken]

Hi Bill and Mary,

Welcome to Colorado.
I hope moving in goes relatively smoothly.

Thanks for the definitions of influence, determine and control. You are right: you can test to see that the disturbance does not control the output (even when it looks like it does when r is fixed) by applying another disturbance to the output and finding that this new disturbance does have the expected effect.

My thoughts on this were motivated by my intuition (which proved to be consistent with the steady state mathematics of control) that a disturbance applied directly to the error signal would influence the output variable but not the input variable. So even if you look at error as the output of the control system, even this output is not controlled (by your definition, neither r nor p (the two variables that determine error) change their influence on e in such a way as to counteract the effect of disturbance on e). The equations say that de (the disturbance to the error) will have the main influence on e (assuming K1*K2, the loop gain, is large). If the effect of de on the output is K, then my calculations say that the effect of de on the output is not attenuated at all by the closed loop (it is proportional to K) whereas the effect of de on the pereptual variable is proportional to (K/(1+K1K2). So, if K1*K2>>K, the effect of de on p is largely removed.

I do agree that we should be careful in the use of words like control, influence and determine. I think control theory really came home to me when you pointed out that Skinner's claim that the environment controls behavior reveals a primatively animistic view of nature. It implies that nature intends to see you doing behavior A and will take steps to compensate for disturbances that would change your behavior to something else. Poor Skinner, and he tried so hard to exorcise the soul from psychology. He ended up putting it back where people always assumed the spirits lived -- back INTO THE WOODS (my daughter's current favorite musical).

Regards to all.

****

Richard S. Marken USMail: 10459 Holman Ave
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213 336-6214 (day)
213 474-0313 (evening)

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Date: Fri, 10 May 91 11:14:05 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: error

Rick,
Thanks for the comments on error. Once again I find myself intrigued with an idea and leaving the net for two weeks--Ugh! So this is probably my last post. I would still like to hear comments relating to some of the other questions I asked concerning drugs, consciousness, self, and time, but I guess I'll have to wait anyway

I'm off to a meeting, hopefully I'll get to comment on your comments before

i leave, but maybe not.

Carpe' Diem
--Mark Olson

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Date: Fri, 10 May 91 15:24:28 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Re: PCT

From Tom Bourbon --

Gary Cziko (910509) reports that McClelland suggests the title, "Perceptual Control Theory," for our undertaking. I was off the net for a couple of days and I have not yet opened the packet with the manuscript. So I was surprised to see PCT as the subject of a post. About two weeks ago, a couple of grad students here suggested that CST people needed a new name, especially now that Complex System Theory has preempted CST. They suggested "Perceptual Control Theory." I urged them to activate their student access to the net and post that suggestion to CSG-L. When I saw your post, I thought they had done so.

The title is an "obvious" choice. It clearly sets our work apart from the many variants that have sprung up with mistaken emphases on control of behavior, or of output. The title preserves the ties to the original engineering control theory, but it acknowledges the significant difference in emphasis in our work -- the topic of a number of interesting exchanges between Izhak Bar-Kana and several members of our group.

I vote for the change. (Where is Bill P., now that we heed his thoughts on the subject!) And the first thing I will do when I send this is download McClelland's manuscript -- with a strong recommendation from Clark McPhail and with this evidence of his insight into the model, I can't wait to read his work!

Best wishes,

Tom Bourbon <TBourbon@SFAustin.BitNet>
Dept. of Psychology
Stephen F. Austin State Univ.
Nacogdoches, TX 75962 Ph. (409)568-4402

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Date: Fri, 10 May 91 17:11:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: controlling INPUT

Izhak Bar-Kana:

Youe wrote: "So, in spite of the "commonly accepted" fact that a control loop controls its input, I would make sure that it represents the desired output unequivocally" (CSGnet 910430).

Good point. God (Mother Nature, Darwin's Hammer) wouldn't have it any other way. If the members of a species are to control their immediate environments (read: survive) their perceptual processes have to be veridical, and the sooner the better. It is no accident that the Gestalt psychologists, who concerned themselves with perception, tended to be nativists.

However, when you write, "I would make sure that it [sensed input] represents the desired output," are you not admitting that the control system controls the value of the sensed input--and, therefore, YOU (or God, or Mother Nature, or Darwin's Hammer), not the control system, "have to make sure" that the control system's input is veridical (i.e., truth telling).

Joel Judd (CSGnet 910430)

Thanks for the book review (Bruner's Acts of Meaning)--and your other interesting comments.

Perhaps you could comment on Rick Marken's (CSGnet 910429) observations about McConkie's remarks--specifically, Rick's distinction between transduction and purposeful communication. How do you view SLA relative to this important distinction?

Gary Cziko (CSGnet 910430)

Here's my 2 cents worth. Having just read Cliff Joslyn's comments (CSGnet 910430), I am not in favor of establishing a CSG Newgroup at this time.

Warm regards to all, Wayne

Wayne A. Hershberger Work: (815) 753-7097
Professor of Psychology
Department of Psychology Home: (815) 758-3747
Northern Illinois University
DeKalb IL 60115 Bitnet: tj0wah1@niu

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Date: Sat, 11 May 91 03:01:01 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: PCT-heed; Re: Ref. for error

From Tom Bourbon --

When I sent my post (910510) on the proposed name for PCTG, I was unaware thet Bill Powers had paid a passing visit to CSG-L. I was fortunate he did not see my post. Had he seen my typo, in which I typed, "Where is Bill P. now that we heed him!" he might have thought we had begun to pay attention to him!

CLIFF JOSLYN (910509) expressed reservations over the proposed

title of Perceptual Control Theory. Some of Cliff's points are well taken, but the major one seems to turn on the rather broad meaning of "perception," in previous work on CT. You want to avoid our creating the impression that CT is merely part of psychology, and I share that concern. But from the start, when Powers, Clark and McFarland published on CT, perception was defined as any signal that is an analog of input variables, so polarization and depolarizations of receptor cells, and discharges in sensory neurons are "perceptions," just as are my kinesthetic, tactile, visual and auditory experiences while watching these ideas appear on my computer monitor. For better or worse, that broad definition certainly has put CT outside the orthodox camp in psychology, from the start!

And by that definition, the various chemical "signals" that arise in receptor sites on the outer surfaces of bacteria, amoebae and phagocytes qualify as "perceptual signals." I agree that those creatures PROBABLY do not experience perceptions very much like ours, but then they experience a world of pure intensity that is not available to us. I wonder what it would be like.

RICK MARKEN (910508) questioned Ed Ford's reply to Mark Olson's post (910507) about a reference for error. Rick, you said it was unlikely that a control system that is working well would set a reference for error. Your remark was in reply to Ed's comments about understanding Mark's feelings.

I agree with you, if we are talking about a rather simple control loop, but in a hierarchical control system, one can easily set a reference at a high level that assures the existence of long-term error, in the technical sense. Ed's example of writing a book seems apt. Immediately upon setting the reference of writing that book, the author creates error that will persist until the book is completed, or the author gives up and eliminates the reference. But that is not to say that the author will feel chronic bodily sensations of "stress" or anxiety: it is simply the case that error will exist until the project ends.

My interpretation of Mark's original post was also along that line. I didn't think he meant he had a reference for feeling chronic symptoms of elevated blood pressure, increased heart rate and the like. But he could easily enjoy creating for himself situations in which he knows he will experience error,] perhaps for a long time and with no certainty that the project he sets for himself will turn out successfully, by conventional standards. (Perhaps I think that is what he meant because it is precisely the position I am in every time I decide to undertake a new project that requires programming of any complexity -- I NEVER know if it will work as I intend and I am acutely aware of the error -- and I rather like it, especially if things work and the error vanishes in an instant when the program runs.

Ed, was that something like what you had in mand? (Mark is gone for a couple of weeks, so I guess we can't get his comments.)

Best wishes,

Tom Bourbon <TBourbon@SFAustin.BitNet>
Dept. of Psychology
Stephen F. Austin State Univ.
Nacogdoches, TX 75962 Ph. (409)568-4402

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Date: Sat, 11 May 91 08:20:49 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: Perceptual Control Theory

[from Gary Cziko]

Cliff Joslyn (910509) writes:

>Control Theory, like all the Systems Sciences, must
>be INCLUSIVE OF and ORTHOGONAL TO academic discipline and system
>composition and level. Control Theory is not JUST Perceptual Control
>Theory. Control Theory is NOT a branch of Psychology.

I appreciate Cliff's concerns, but given Tom Bourbon's (910511) broader definition of the term "perception," I still like Perceptual Control Theory. I suppose we could call it Input Control Theory, but input is relative to where you are standing. Besides, I AM a psychologist and everyone I know in CSG is interested in living control systems and one could argue that perception is the flow of information from the environment to organism and so is applicable to all cases of living control systems.

Wayne Hershberger (910510) writes:

> Here's my 2 cents worth. Having just read Cliff Joslyn's
>comments (CSGnet 910430), I am not in favor of establishing a CSG
>Newgroup at this time.

I have set this up as a LOCAL Newsgroup for my campus which is particularly useful for individuals with student accounts who have small mailboxes easily overwhelmed by the activity of CSGnet. This also makes it easier for me to tell people here about us and let them check us out without making a committment to CSGnet. It also keeps us away from the type of trouble Cliff Joslyn warned us about concerning an national/nternational newsgroup.

I would like to propose the idea of setting up a number of LOCAL newsgroups on campuses/institutions where we have PCT people who would like to share CSGnet with others and where unix newsreaders are used. This is apparently quite easily done by someone in your computer services office. All I then need is an electronic mail address for the newsgroup which I will put on the CSGnet list. Then if one of these access routes to CSGnet becomes a problem, I can knock it off the list without bothering other people's access (but then who am I to decide what a problem is). I'd like to get Cliff's reaction to this idea.--Gary

Gary A. Cziko
Associate Professor
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Date: Sat, 11 May 91 11:48:31 cdt
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "McClelland,Kent" <MCCLEL@GRIN1.BITNET>
Subject: Labels and skydiving

(From Kent McClelland)

I'm pleased to see that 'Perceptual Control Theory' has struck a chord. Although I see the point of Cliff Jocelyn's comments (910509) about not selecting a label which implicitly restricts the theory to psychological concerns, I think from a practical point of view that it's easier for those of us who are social scientists to communicate what this theory is all about to others in our own and related fields if we emphasize the view that behavior is control of perception, not output. As to whether the definition of "perception" can be broadened and applied to other than complex animals, as Tom Bourbon suggests (910511), I don't feel qualified to venture an opinion.

One other kind of whimsical note: I like the PCT abbreviation because it can symbolize "Powers' Control Theory" as well as "Perceptual Control Theory" and that seems to me to give some credit where credit's due.

A comment on Mark Olsen's enjoyment of learning things that "completely screw up" his "ideas about reality" (910506) and various ensuing comments on tolerance of error: An interesting article called "Edgework: Voluntary Risk Taking," by Stephen Lyng (1990, American Journal of Sociology 95:851-86) discusses sky divers, motorcyclists, and other young men (rarely women!) who actively seek risky avocations. Lyng argues from his interviews that the kick in all this comes from "the ability to maintain control over a situation that verges on complete chaos, a situation most people would regard as entirely uncontrollable" (p. 859). He goes on to explain that this "mental toughness" or "right stuff" is really a "sense of cognitive control over the essential 'objects' in the environment or a feeling of identity with these objects" (p. 861), and that it is achieved by "mental control" in which "perception narrows to only those factors that immediately determine success or failure in negotiating the edge" (p. 861). While Lyng isn't using PCT, the potential links are obvious.

Perhaps Mark's intellectual thrills are in some way analogous to a skydiver's physical ones?

Kent McClelland Office: 515-269-33134
Assoc. Prof. of Sociology Home: 515-236-7002
Grinnell College Bitnet: mcclel@grin1
Grinnell, IA 50112-0810

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Date: Sat, 11 May 91 11:45:01 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Correction

[From Rick Marken]

I made another error (910510). The effect of a disturbance to

the error signal is attenuated by the closed control loop.
The disturbance, de, is reduced by a factor of 1/(1+k1k2) --
as is the environmental disturbance. I'll give the system
equations and derivation on monday since I'm in real time
here at home and likely to be interrupted.

The bottom line, however, is that adding de (the disturbance to
the effect of error on output) contributes nothing to our
understanding of control. It does contribute to my understanding
of why I should not rush through the math.

Best Regards

Rick M.

marken@aerospace.aero.org
==========
Date: Sat, 11 May 91 14:46:21 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Perceptual Control Theory
In-Reply-To: Message from "Gary A. Cziko" of May 11, 91 at 8:20 am

[From Cliff Joslyn]

> [from Gary Cziko]
>
> Cliff Joslyn (910509) writes:
>
> >Control Theory, like all the Systems Sciences, must
> >be INCLUSIVE OF and ORTHOGONAL TO academic discipline and system
> >composition and level. Control Theory is not JUST Perceptual Control
> >Theory. Control Theory is NOT a branch of Psychology.
>
> I appreciate Cliff's concerns, but given Tom Bourbon's (910511) broader
> definition of the term "perception," I still like Perceptual Control
> Theory. I suppose we could call it Input Control Theory, but input is
> relative to where you are standing. Besides, I AM a psychologist and
> everyone I know in CSG is interested in living control control systems and
> one could argue that perception is the flow of information from the
> environment to organism and so is applicable to all cases of living control
> systems.

Classical epistemology makes a clear distinction between "perception"
and "sensation". The former is a high-level construct of the latter,
and involves cognition and knowledge. I can "perceive" that it is an
actor who is threatening me, and thus not react; I "perceive" that it is
a person holding a knife; I "sense" shapes and colors. Thus the higher
level control systems typically considered in CT are indeed perceptual
AS WELL AS SENSATIONAL, while lower level (e.g. 1st and 2nd order,
reflex) are sensational ONLY. Whether or not chemical reception and
antigen interactions in cells is "sensation" or not is debatable and
probably not settled. But CT covers machines also; the thermostat
neither senses nor perceives, it measures and detects.

Undoubtedly this is a purely semantic argument, but terminology is

CRITICAL to a discipline in the context of the academic community in which it interacts. I work in both "cybernetics" and "fuzzy systems", fields which have suffered from poor choice of terms. CT understands "perception" in the general sense of "input", but THE REST OF THE WORLD DOES NOT. Alas there IS no satisfactory term that covers perception, sensation, detection, measurement, and chemical interaction as a general concept. "Input" is indeed correct, but hardly captures the connotations desired. Perhaps a term sould be invented. But surely the more general existing term (e.g. sensation, input) should be used and qualified, not the more specific (e.g. percpetion). Perhaps Peter Cariani has a suggestion. Peter?

It is difficult to constantly qualify and apologize for terminology, AND you will not attract those who are NOT interested in psychology PER SE (like me). I could care less about tracking tasks (except to the extent that it's a cogent and compelling example of control). If you do NOT have an interest in extending the full promise and mandate of CT, as Powers sees it, to biologists and "semanticists" in general, but are rather content to continue with CT as a kind of psychology, then the term PCT will help you. Otherwise it will hurt Control Theory.

> I would like to propose the idea of setting up a number of LOCAL newsgroups
> on campuses/institutions where we have PCT people who would like to share
> CSGnet with others and where unix newsreaders are used. This is apparently
> quite easily done by someone in your computer services office. All I then
> need is an electronic mail address for the newsgroup which I will put on
> the CSGnet list. Then if one of these access routes to CSGnet becomes a
> problem, I can knock it off the list without bothering other people's
> access (but then who am I to decide what a problem is). I'd like to get
> Cliff's reaction to this idea.--Gary

Sounds good, but I doubt my reaction will help you. I'm not a news guru, just know a bit about the technology and politics. Your local experts can best advise you of what's possible locally. Good luck!

O-->
| Cliff Joslyn, Cybernetician at Large, cjoslyn@bingvaxu.cc.binghamton.edu
| Systems Science, SUNY Binghamton, Binghamton NY 13901, USA
V All the world is biscuit shaped. . .
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Date: Sat, 11 May 91 15:12:38 edt
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Peter Cariani <peterc@CHAOS.CS.BRANDEIS.EDU>
Subject: Perceptual Control Theory
In-Reply-To: Cliff Joslyn's message of Sat, 11 May 91 14:46:21 EDT

Well, I actually like the name "Perceptual Control Theory." I think there would be a large number of people who would understand such a term (aside from psychologists, neurobiologists, and cogsci philosophers-- roboticists, and other more practically-oriented, less literally-minded people), but Cliff is right--there is no good general term for "perception". I used "measurement" following Pattee, and while it's sufficiently abstract to be general, there are many people
who would restrict its meaning to scientific observation. I've found that for any word sufficiently common to be readily understood, many people are going to have particular meanings attached, and there will always be a

significant percentage who will cling to their more specific usage
(for whatever reasons). A general problem of communication.

I think Perceptual Control Theory is more descriptive than "cybernetics",
"fuzzy systems" or "Control Theory" for more people.

Perhaps "Adaptive Control Theory" might be another alternative.

Peter Cariani
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Date: Sat, 11 May 91 16:30:00 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Definitions

[from Gary Cziko]

Bill Powers (910509); Rick Marken (910510)

I find the definitions that Bill offered for influence, determine, and
control quite appealing. Let me first repeat them for easy reference

>INFLUENCE: A influences B if A is ONE of several variables on which the
>state of B depends. The engineer's hand on the throttle INFLUENCES the
>speed of the train; the speed is also influenced by the brake lever and
>by the slope of the tracks.
>
>DETERMINE: A determines B if, given A, B is completely predictable
>(i.e., B depends on A and ONLY on A). The path of a train is normally
>determined by the configuration of the tracks.
>
>CONTROL: A controls B if, for every disturbance applied to B, A changes
>its influence on B in such a way as to counteract the effect of the
>disturbance on B.

Then you add:

>From the definition of control just given, we can see that a disturbance
>influences but neither determines nor controls the output of a control
>system.

I'd like to try to apply these terms all around the control loop to test my
understanding both of the definitions and of the control loop. First let
me try it with passive descriptions:

1. Behavior:
   a. not controlled
   b. influenced by both the reference level and disturbance
   c. determined by neither reference level nor disturbance (since it is
influenced by both)

2. Perception:
   a. controlled by behavior (behavior compensates for disturbances to
perception)
   b. influenced by reference level, disturbance and behavior (but less so
 by disturbance and behavior as control is better?)

c. determined by neither reference level, disturbance, nor behavior since it is influenced by all three (but as control increases, it is less influenced by disturbances and "more" determined by reference level?)

3. Disturbance:
a. not controlled (at least not by the affected control system)
b. influenced by other control systems or non-control-system physical events
c. may be determined if there is only one influence (as in tracking computer tracking experiments)

4. Reference Level:

a. not controlled
b. is influenced by outputs of higher levels of control systems
c. may be determined by the output of a higher level if it receives only one such output

Now let me try it with active phrases.

1. Behavior:
a. controls perception
b. influences perception
c. may determine something outside the loop (e.g., the train engineer's behavior determines the position of the throttle, unless someone else also has his hand on the lever)

2. Perception:
a. controls nothing
b. influences behavior
c. determines nothing

3. Disturbance:
a. controls nothing
b. influences behavior and perception (but influences perception less as control is better?)
c. determines nothing

4. Reference Level:
a. controls nothing (since it remains a fixed value)
b. influences perception and behavior
c. determines nothing

This got a bit more tedious than I thought it would, but there it is anyway. Does this make sense? I'd particularly like some feedback where I put the question marks.--Gary

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==========
Date: Sat, 11 May 91 16:30:23 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Announcing Closed Loop #2

Greg Williams has put together a wonderfully edited and coherent version of recent discussions from this network into what he has called Closed Loop #2 (the first version was appended to the February CSG Newsletter). This is the kind of file you will want to print out using your laser printer and put in a binder for future reference. To borrow Tom Bourbon's phrase, it's "the closest we have" to a journal.

The two topics included are:

1. THE METHOD OF LEVELS AND INTERNAL CONFLICT
2. "CONDITIONING"

While Greg has apparently put his publication plans for Closed Loop on hold, he nonetheless wants to make this available to interested people. But the file is 77kbytes long, too long, I feel, to send indiscriminately to everyone on the net.

So here are my plans. I WILL send the file in a few days to those I consider to make up the "hardcore" of CSGnet. These people include:

Bourbon, Delprato, Ford, Joslyn, Marken, McPhail, Petrie, Powers, Roberts, Tucker

Those on this list who do NOT want to receive Closed Loop #2 should send me a PERSONAL message to that effect as soon as possible. Otherwise they need do nothing.

Those who are are not on the "hardcore" list but who DO wish to receive Closed Loop #2 (and any future issues) should send me a PERSONAL message to that effect and they will receive it shortly.

Please forgive if I have omitted some hardcores from my list or have included those who have not yet been sufficiently boiled.

I hope that Greg soon gets his private phone line so that we welcome him on CSGnet. When he does, he will deserve our thanks for his efforts.--Gary

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Date: Sun, 12 May 91 00:20:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: Everything and more

[From Wayne Hershberger]
I have been unable to read my E-mail for a couple weeks, so I'm a little out of phase, but since it was good stuff, I'm going to add my 2 cents worth as I read. Please forgive redundancies.

Marken (CSGnet 910501)
Thanks for the reference to the book "Feedback thought in social and systems science" by Richardson (U Penn Press, 1991).

David Coombs (CSGnet 910503)
Thanks for the reference to Jens Christensen's paper "A Hierarchical Planner that Generates its Own Hierarchies" in the proceedings of AAAI-90.

In an earlier post you asked about a perisaccadic flash appearing to be displaced in the direction of an impending saccade. You wondered whether the phenomenon was related to the displacement experienced with paralysis of the extraocular muscles. Yes, exactly. The saccade does not begin for 80 ms after the reference signal changes because the "pause" cells in the PPRF are inhibiting (paralyzing) the "burst" cells that drive the saccade.

Mary Powers:
Your inspired comments on religion (CSGnet 910504), are absolutely divine, honestly--all puns intended.

McClelland (CSGnet 910503):
I would be interested in seeing a copy of your ms.

Tim Cutmore (CSGnet 910506)
Very interesting post. I am on a university committee concerned with "strategic planning," and your comments elicited sympathetic vibrations in my brain, not unlike the phenomena Mary Powers was describing in her recent post (see above).

Also, the monkey trap is an instructive example. You wrote: "The monkey and the coconut problem illustrates such a problem. For those unfamiliar with this example: To catch a monkey hollow out a coconut and leave a hole such that the hand can be inserted but not retrieved if an object is grasped inside it. Tie the coconut down and place a desireable object (food) inside. The monkey gets trapped because it refuses to release the object.... In CT language the problem appears to be one of trying to satisfy conflicting error signals - one tells the monkey to hold the coconut, the other tells it to let go."

It seems to me that, perhaps, nothing is "telling" the monkey to let go. In your own words, the random walk never gets to that alternative. It is as though two hill climbing processes are going along in parallel with no recognition of environmental

crosstalk. It is a special case of conflict, isn't it? The clinicians should think about it.

Mark Olson (CSGnet 910506):

You wrote: "Everyone likes humor. The kind I like (the Far Side) must create a lot of error. SO WHY DO I LIKE IT? If life is all about error reduction, then why do I seek it? Someone will probably say that anyone in academia desires error. Maybe, and that would be interesting to explain too....Do I ask too many questions when I write from the top of my head? "

Not for my taste.

Rick Marken (CSGnet 910506):

You wrote, "I think my main point is that it should be possible to distinguish control of a program (skilled problem solving) from learning to control a program (reorganization in order to solve a particular, unfamiliar problem)."

Rick, how long do you suppose it would take a monkey to escape a coconut trap, and need this involve reorganization? If the monkey gets away "by accident," how likely is it to "learn how" to escape coconut traps? If it appears to learn, is this reorganization the development of an algorithm or a heuristic method?

Izhak Bar-Kana (CSGnet 910507):

I hear what you are saying--I think. Let me try to shed some light on the matter, because I wrestled with the paradox posed by the input/output language myself some time ago (would you belie a quarter century?). I'll try to be terse.

The yoked terms CAUSE and EFFECT (PROD and PRODUCT, INDEPENDENT VARIABLE and DEPENDENT VARIABLE, and CONTROLLING and CONTROLLED),respectively, have gotten linked to INPUT and OUTPUT in engineering just as they have gotten linked to STIMULUS and RESPONSE in psychology. This linkage goes way back--long before the development of control theory. Relatively recently, Ben Franklin "mislabeled" the polarity of electrical potentials, and just as engineers continue to use Franklin's terminology as a an acceptable convention, even though that convention has misleading connotations, so they can and do use the cause/input and effect/output conventions even though that convention also has misleading connotations.

Consider the following bizarre statement: The output of a furnace-thermostat system is an input, not an output, but this input is an output not an input. Although this grammatical sentence is NOT nonsense, it is certainly gibberish. Deciphered, the sentence reads, The output of the thermostat-furnace system (i.e., what it produces or does or controls) is a particular value of temperature sensed by a thermocouple (receptor input), not a particular amount of heat emitted by the furnace (output), but this sensed temperature (or receptor input) is an output (i.e., the dependent variable controlled by the system), not an input (i.e., it does not cause the temperature being produced).

As you can see, some truths can NOT be expressed in engineering's input-output terminology without sounding ridiculous. Conversely some things which are truly ridiculous can sound very true. As long as input means IN, and output means OUT, it is unseemly to use those same terms (input and output)

also to denote cause and effect (or controlling and controlled). As you can well imagine, when one pairs the term input with cause (or controlling) and the term output with effect (or controlled) many of the connotations are as phony as a three dollar bill. I want no truck with that currency.

I suspect that you are not bothered all that much by these mischievous verbal connotations because you think mathematically most of the time (Franklin's convention poses no problem for me when I use Ohm's law, but it is a veritable pain in the ass when I try to think about PNP versus NPN transistors). Perhaps you can understand why those of us who wish to describe control systems in ENGLISH, can ill afford to be encumbered by blind (and blinding) anachronistic language habits that were developed in the context of a technology devoted to CALIBRATED "control" systems (wherein the input/cause and output/effect convention is not problematic).

I am not for a moment saying that control engineers do not know what they are talking about when they say that control systems control their output. What I am saying, is that engineers do not realize the mischief they are making for the rest of us when they use the terms input and output in this way. I am speaking for myself, but I think the other CSGers would agree.

Tom Bourbon (CSGnet 910507b)

Although I would acknowledge that my visual perceptions (comprising a manifold of opaque surfaces and solid objects) do not square with quantum physics, these two "realities" are twin born of experience. One is not the offspring (model) of the other (if one is a mental representation, the latter, quantum physics, better fits the bill). I tend to side with Rick about perceptual processes developing genetically, as you may have noticed in my last post (which I sent before reading your post). Maybe we can talk about this at Durango later this summer. For now, let me mention a single example of perceptual constancy to give you an idea of my position. In the chambered eye, the retinal image of a luminous object is inversely proportional to the distance of regard. The illuminance of the eye's pupil by the luminous object in question is also inversely proportional to the distance of regard. It is the same inverse relationship. As a consequence of this coincidence (genetically developed and not characteristic of compound eyes), the density of luminous flux comprising the retinal image is independent of the distance of regard. This is why we "see" the luminance of surfaces as fixed as we move about. I suspect it is also why we see surfaces per se.

Rick Marken (CSGnet 910508)

I researched the perception of rotation in depth for many years while waiting for the zeitgeist to change (i.e., waiting to be able to publish control theory work). If I understood exactly what you were trying to do with the Ames window, perhaps I could be of help. For instance, it is possible to simulate clockwise rotation (in the Y dimension) and counterclockwise rotation (in the X dimension), simultaneously: Hershberger, W. A., Stewart, M., & Laughlin, N. K. (1976). Conflicting motion perspective simulating simultaneous clockwise and counterclockwise rotation

in depth. Journal of Experimental Psychology: Human Perception and Performance, 2, 174-178.

I have a number of reprints of the "cute study" you mentioned: Hershberger, W.(1986) An approach through the looking glass. Animal Learning and Behavior, 14(4), 443-451. I would be delighted to send them to interested parties. But I am going to be out of touch for a month, so perhaps requests should be sent by US mail.

Gary Cziko (CSGnet 910509)

I have a problem with Ken's label, perceptual control theory. It has apparent advantages for some of us, which Itzhak recognized immediately--putting some distance between himself and the rest of us. But such an estrangement is no true advantage for any of us. We CSGers are NOT in disagreement with control engineers who insist that control systems control output; we would prefer that they modernize their terminology (see above), but there is no fundamental difference between us. Control is control, harken to Marken (what a mellifluous alliteration). The theory of control is control theory, whether in the animal or in the machine. Furthermore, perceptual control theory suggests that perceptions do the controlling--a la Skinner.

Gary, I am certain that Rick would regard your amendment to his observation not only as a friendly one [I see he said as much in a later post], but one that bears repeating. So I'm going to repeat it. Gary, you said:

"Shouldn't we say something like "controlling means causing a variable to stay at a specified value or take on a specified pattern of values over time"? Yes, I realize that the repetitive pattern of perceptions I get as I jog are represented by a single higher-order value for jogging which we assume is constant (until a still higher system tells me to run faster), but can't we say that at a lower level control need not be limited to a single value? Otherwise it starts sounding a bit too homeostatic to me."

It is customary to "improve" on each others comments Gary, but you leave me with nothing more to say.

Cliff Joslyn (CSGnet 910509)

Cliff wrote "Control Theory, like all the Systems Sciences, must be INCLUSIVE OF and ORTHOGONAL TO academic discipline and system composition and level. Control Theory is not JUST Perceptual Control Theory. Control Theory is NOT a branch of Psychology."

Amen. Listen to Joslyn.

Bill Powers (CSGnet 910509)

Good to hear you're still in touch--from limbo yet.

Gary Cziko (CSGnet 910511)

Since you are responding to Bill's (910509) comment, I'll let him reply to your post--but your attempt to describe the parts of the loop in this way is a splendid pedagogical exercise--as you will soon discover.

By the way, I still consider myself hardcore. I trust you

will send me Closed Loop #2.

Warm regards to all, Wayne

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Label Problems

[from Gary Cziko]

As much as I originally liked and advocated the use of Kent McClelland's term "Perceptual Control Theory" to describe our field, I now see a problem with this label.

When people on this network see Perceptual Control Theory, they naturally see it as meaning that perception is controlled (i.e., perception is the controllee with behavior as the controller). But in fact the term is ambiguous since it could just as easily be taken to mean that perception is the controller and behavior the controllee (which is exactly the opposite of what we want to convey). Consider the term "government control" and you will probably immediately think of government as the controller, not the controllee.

Even the gerund form is ambiguous. "Controlling perceptions is what we study" can be taken either way (although only one meaning is true), in the same way as Chomsky's famous "visiting relatives can be a nuisance" (although in this case both meanings are true).

The only way to disambiguate this appears to say something like "the control of perception" (is it a coincidence that this turns out to be the second part of the title of Bill Powers's 1973 book?) but "The Control of Perception Theory" seems a bit awkward (Note: I had trouble spelling that last word; I got it right the first time, but the "KW" sequence caused me to doubt it was right. I doubt there is another English world with "KW", certainly not one with "WKW".)

William Glasser used the full title of Bill's book at one point calling it BCP theory (Behavior, the control of perception). Should we use BCPT?

I agree with Cliff Joslyn that what we call this thing is very important. We are lucky that control theory is still young enough that we still may have some say in this matter. What will it be? Are we stuck with control theory?--Gary

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Date: Sun, 12 May 91 19:30:25 -0600
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: terminology

[From Bill Powers]

Now my link is through CU in Boulder, thanks to my son Denny's setting up a more or less automatic way of reading, compressing, and sending the mail. Gary, I think you can now Unsubscribe me from the Chicago address, as I am already subscribed from the CU location. My new direct address for the time being is powersd@tramp (.bitnet). I am writing this with my computer on the floor while I sit on a crate with the keyboard on my creaky knees. The furniture arrives tomorrow or Tuesday, so we're camping.

The topic in today's mail seems to be terminology, so I will hold forth on that. I think "topic" may be at the principle level.

"Perceptual Control Theory" has advantages and drawbacks, as everyone has noted. One drawback due to the use of the word "control" is inevitable. People often react strongly against associating this word with theories of behavior, almost always because they don't like the thought of controlling people (or more to the point, being controlled by other people). This reaction is justified, but it shouldn't make us shy away from the word. When people try to control other people, the results are usually some form of immediate or delayed disaster, for reasons we all know well. These disasters are the reasons for the unpleasant connotations of "control." Instead of finding some other terminology, however, I think we will get farthest in the long run by continuing to get across the fact that control theory explains why trying to control other people is a mistake -- not a moral mistake, but a practical, pragmatic one. We should explain that human beings must control in order to continue being human, but that they must learn how to live together without attempting the impossible: controlling each other. We should teach people to identify human problems that stem from the fact that human beings are control systems. In other words, control theory is a guide that can show us how to live in peace in spite of the fact that we must control to live.

Cliff Joslyn objects to the first word, "perceptual," on the grounds that it will be misinterpreted by conventional psychologists. I hope that we don't abandon our current generic usage of "perception." One reason for my proposing all those levels of perception was precisely that the conventional distinction between "sensations" and "perceptions" was far too crude, as well as implying that there was some fundamental difference in kind between these two broad classes of experience. In my model, every level of perception is derived from levels below by computing processes. The function of a perceptual signal in the process of control is the same no matter what level you talk about. By using the single common term perception to mean all stages of afferent processes, we can emphasize the sameness of the basic functional arrangements. By using more specific

terms like intensity, sensation, configuration, and so on, we can make much finer distinctions in the hierarchy of input processes, and at the same time be much more explicit about how different classes of perception differ from each other and depend on each other. I think that the old distinctions were pretty much stabs in the dark, and we should do away with them. If psychologists experience a hangup over the difference in terminology, I would say good, maybe that will make them think about the subject a little more deeply. Why should we be the ones who have to adapt?

As to "input" and "output", input is what goes in and output is what comes out. This means that input and output are terms related to S-R devices. Control systems are made of such devices. An input quantity comes into a perceptual function and a perceptual signal comes out. p and r come into a comparator and e comes out. e comes into an output function and the output signal comes out. The output signal comes into the environment and the state of the controlled quantity comes out.

When you hook up all these little input-output devices with the polarities correct for negative feedback, you get a control system, and you stop talking about the input and output of the whole system because there's a closed loop. Whenever you use either "input" or "output" in connection with a control system, you're automatically talking about just part of the system, a component or a string of components that doesn't include a closed loop. If you always distinguish clearly between the component level of description and the whole-system level, there is never a problem. A lot of the time we don't bother to be explicit because context takes care of it, but when language starts leading us into tangles (one of its main functions) we need to make the distinction.

Tom Bourbon says, in relation to some posts I missed,

>Immediately upon setting
>the reference of writing that book, the author creates error
>that will persist until the book is completed, or the author
>gives up and eliminates the reference. But that is not to say
>that the author will feel chronic bodily sensations of "stress"
>or anxiety: it is simply the case that error will exist until
>the project ends.

This relates to my claim that we don't consciously experience error signals -- only the efforts that they call forth. Whether we use error-like terms to designate those efforts depends on whether the levels of effort are normal or unusual. Control theory tells us about error signals, but direct experience doesn't. If you try to push open a door that is locked, you may feel a very large peak of effort, and say "Oops, I thought it was unlocked," but that's not a description of an error signal in the technical sense. It's a description of a relationship between an imagined situation and a perceived situation: another perception.

If we could experience every part of every process that goes on, we wouldn't need a theory. Theory fills in the blanks between perceptions, once we realize that the blanks exist. Control theory fills in the blanks between the experiences of desiring or intending and the perceived

actions and consequences that seem to occur as a result of desiring or intending.

Tom Bourbon --

Authors who adopt the reference condition "This wonderful book is written" are bound to experience stress and inner conflict, because until the last word is written and edited, the book is NOT written. A student of control theory will select goals more wisely: "I'm writing a book." That goal can be satisfied instantly and maintained in a match with its reference signal from the moment the "on" switch is thrown until the galley proofs are returned to the publisher.

Gary Cziko --

I like your ringing of the changes on influence, determine, and control. If you do a search on "control" in most scientific articles, you will find that the author really should have used influence or determine. And I could add a fourth term: comparison. A "control" experiment is done to establish a value of the dependent variable for comparison -- not for control. If there is a difference between the blank run and the real run, does the experimenter take steps to eliminate it?

Martin Taylor --

A few days ago you mentioned the phenomenon of pushing the wrong door first, and then continuing to do that afterward without learning to push the right one first. I think that what happens is that you learn in one trial that there is a sequence that will get you through the door: push left, then right. This works, the error is corrected every time, so there's no need to reorganize at the sequence level or above. The transient failure to get through the door is immediately corrected, but is sufficient to start the verbal/cognitive system going, trying to correct the error at that level. However, all the mental chatter that accompanies this process (second-guessing, and so on) all happens too late because the error's already been corrected. It's accomplishing nothing more than satisfying a reference condition that says "I have to make verbal sense of this." You're already through the door, and the monkey-chatter dies away again. If you really wanted a cognitive way to choose the correct door, you'd stop, go back, and look for some low-level perception that identifies the door that opens. Then it would stop being a cognitive problem. But who takes the time to do that?

Wayne Hershberger --

>In the chambered eye, the
>retinal image of a luminous object is inversely proportional to
>the distance of regard. The illuminance of the eye's pupil by
>the luminous object in question is also inversely proportional to
>the distance of regard. It is the same inverse relationship. As
>a consequence of this coincidence (genetically developed and not
>characteristic of compound eyes), the density of luminous flux
>comprising the retinal image is independent of the distance of
>regard. This is why we "see" the luminance of surfaces as fixed

>as we move about. I suspect it is also why we see surfaces per
>se.

It's also possible that an object at a constant distance of regard simultaneously gets brighter and larger. In this case the interpretation is of an approaching object of constant brightness. I ran across this phenomenon in a UFO investigation some years back, sparked by one of those barium-cloud rocket experiments. In real experience, I doubt whether many objects remain for long at constant perceived density of luminous flux as they approach and recede, or even when they stand still. I think you're explaining a perceptual phenomenon by using a physical theory -- and both the phenomenon and the theory lie on the same side of the sensory receptors: inside (according to neural theory).

Couldn't resist that.

Wayne plus Gary --

As to your remarks on perceptual control theory: people are going to see or hear whatever you say as fitting what they believe. I have received more than a few letters in which the writer evidently read "controlled variable" as "control variable," meaning the variable that controls behavior. I have also run across this in the literature. So I don't think that precision of terminology is going to help much in convincing the unconvinced -- they don't know enough yet to realize that you're being precise. There's no way around the need for patient and extended explanation, one-on-one, saying the same thing as many different ways as you can think of. Only in that way will you eventually get the concepts across without having them tied to particular verbalizations. If you settle on just one "best" way of explaining anything, the explanation will turn into a slogan and you'll have even less success in getting the real idea across.

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Date: Mon, 13 May 91 04:01:17 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject: Naming the Field

Perhaps a neophyte's opinion might might be informative.

I have been listening for a couple of months and am starting to catch on and like it.

When I think of the topics that this group discusses I use the term Hierarchical Control Theory. Although I very much like "Perceptual Control Theory," I am sensing a growing dissatisfaction. To me "Hierarchical Control Theory" does the following:

(i) states the two fundamental constructs which define the theory: hierarchies and control systems,
(ii) defines, from these two constructs, a relatively new beast, the hierarchical control system, which distinguishes the theory (without too tightly circumscribing it as "PCT" was criticized as doing).

Bat that around.

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Date: Mon, 13 May 91 14:13:07 +0200
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Oded Maler <Oded.Maler@IRISA.FR>
Subject: The game of the name

[From: Oded Maler]

Concerning the title of your theory I have the following remarks:

1) CT alone is a very bad choice, unless you live in a complete isolation from thousands of engineers and mathematicians practicing what they call Control Theory.

2) The main message of the theory as I understand it (without reading the bible, though) is that folk psychological explanation of human and animal behavior (e.g. X does Y because he wants Z instead of the current situation W) which ignore the perceptual aspect's of X's knowing W etc., are non-sense, and an organism can only try to control/influence the picture of the world that he/she/it perceives. I agree completely with this criticism of Psychology, and in that sense, I think PCT is a good choise.

3) I have a lot of sympathy for 'Cybernetics in the Large' but the failure of the General System theory of the '50s, should teach us that trying to build a too general model, not concentrating around a set of concrete and tractable problems, does not lead to good science (this does not imply that the current practice of over-specialization produces good science either). The problem of trying to explain at some level the behavior of animals having a nervous system, and the problem of building artificial creatures that can operate in rich and non-trivial environments seems to me hard enough. Not that I underestimate Enzyme soups, energy clouds, or changes in stock market prices, but I I can't see how a unifying principle can be found.

4) At a more philosophical level, when you look at questions of meaning, knowledge and representation, the main theme of this theory (as I understand it) becomes a tautology. Because finally, there is a mechanism that relates pereception to action, no matter what structure and levles we can attribute to this mechanism. We (theorist) look at some "percept", that is, a collection of input patterns and say that the prganism's behavior is an attempt to keep this percept within a certain range of values. But, in fact, there is just that underlying *mechanism* which causes the behavior, which looks to us *as-if* it is an attepmt to control some percept. [There is a nice article by Searle about the role of consciousness in psychological explanation, somewhere in BBS '90 or '91.]

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From: BARKANA@DUPR.OCS.DREXEL.EDU
Subject: Tom, Rick, Wayne, Input, Output, Perception

[From Izhak Bar-Kana]

The Hungarian scientists discovered a skeleton, and they were very excited, because they assumed it was the very Attilla, The King of Huns. To get a secong opinion, they sent it to their Russian colleagues. After a while, they got it back all smashed, and with a note: "Yes, it is Attilla. He admitted!"

After all these eloquent proofs I can only give up, admit, or whatever... More seriously, I think most of you ignore some of my words, like:

>Besides, the most intelligent system I may dream to design, does not come
>even close to the simplest organism. In my humble opinion, again, one of
>the reasons for the huge and not always motivated (apparently) redundance in
>the organisms is intended to prevent an ocasionally wrong measurement (or
>input feedback) from replacing the correct output that the control system is
>meant to control.

One problem, for example, (I hope it is clear that we don't talk about train engineer's problem) is that we here must control the position of a satellite up there. So, the position of the satellite is THE object of the control. I have no direct means to measure it, as I have no direct means to affect it. Yet, everything in my control problem revolves around this control object. I can only use some remote measuring instrument, but I have no intention to control its output. Actually, one must learn how to ignore some of those input signals, because they are disturbances, or noise. No one can convince an engineer to accept the idea that he and his control system control an input. You all seem to accept the idea that there is no control unless there is intention to control. Therefore, the position of the satellite will be monitored my multiple sensors, well filtered and processed (to estimate some other of plant states, such as velocities, etc.) in such a way that the controlled variable remains the value I am interested in, namely the position, or more general, the motion of the satellite.
If the result of the multiple measuring, filtration and processing is what you would call perception, I have no argument, and, since I don't want to change your group, only to learn from... whatever you call yourselves, I can go on and follow your discussions.

Tom Bourbon:
Many thanks, but please, do not RESPECT, or TRUST me. The only principles I may trust are: 1) Never believe in principles, 2) No one here is God, 3) No one is really dumb.

After all this time that I (try to) follow your discussions and our arguments, I sort of start understanding your problem. And sometimes I am

afraid I got caught in the middle of your fire addressed to those behaviorists. In spite of the fact that you, Rick and Wayne seem to agree about the contents of your messages, and I think I have no problem with most of them, their is a difference in the tone between your explanation and Rick's.

Rick Marken:

There is a big difference between Rick Marken when he clearly presents an idea, and The Rick Marken in an argument. Sometimes, it seems that the second has had THE REVELATION, or even touched GOD, and to hell with the others. I am afraid that, when you talk about control, you have a steady state image at the back of your mind. I MUST keep a dynamic image in my mind, because some of our sad experiences show that the steady state may be beautiful, but never reached. I must use some mathematics, because many great, ingenious, and intuitive ideas just proved wrong. It is easy to show that "if this is so, the gain must be so and so, and if the error is so, that let us adapt the gain to be so and so." These arguments convince, and engineers like them. Hovever, after a plane crashes, and one analyses (very difficult math, in particular in nonlinear systems) the aftermath, one discovers that things become unstable, just because one use non-constant gains, even very carefully, namely within the "admissible" bounds which were tested with constant gains.

When my organism control the motion of my hand, this is the intention of the control, the object of the control, and I would thinks that the corresponding control system controls the position of the motion of the hand. If you agree that there is intention in control, this is the only intention. The fact that I must MEASURE, SENSE, OBSERVE, this motion is problem, TSORES, not a principle. As you have observed lately, a closed loop system is so built (if correctly built), that the gain reference-to-output is almost ONE, while the gain disturbance-to-output is almost zero. We can show that an integration in the forward path makes the corresponding ratios actually 1 and 0, at least for constant reference inputs.

I try to stay aside when psychology is what is talked here, and if you consider that the reference inside the organism is a great idea, showing that life affects the environment, and not vice versa, I am excited by the idea. Unfortunately, I cannot claim the same thing about the artificial control loops, I mean environment makes a lot of troubles. I only have my own organism to observe, and personally, I think that I can decide to drive here or there only after a long period of learning and the reference points within are a good mapping of the reference points out there. I might decide just to follow an internal reference with no relation to the outside word, but usually I stop after the right number of glasses. Furthermore, what reference command is to be followed, I would rather call DECISION than CONTROL.

If I want to move my hand, or a robot arm, I apply a force. If I meet resistance, I use more force. If there is an egg there, I must behave in a different way, and this is first of all a decision problem, or a detection problem. I don't call everything control theory. Many control people (engineers or not) do not know detection or decision theory, and they have to rediscover it again and again when it is needed, and not the best way.

By the way, this argument does not prevent me from enjoying this intelligent discussion. As an illustration, please read again Ed Ford's message on competition and religion. Religious or not, see if you can disagree with his message. It is a good example how some terms one uses may become dominant to the reader and obscure the message itself.

Wayne A. Hershberger:

Time flies like an arrow,

Fruit flies like a banana

I admit that this is not related to our topics, and I admit that what you call gibberish is gibberish indeed, but I cannot see how you relate it to ANY engineering.

I am still happy you do not blame on me the Original Sin. I will never tell anyone he is wrong because his arguments remind me.... I can argue about right or wrong, understand or don't understand, and so on. Too many arguments here blame me for talking like the behaviorists, like Wiener, calibrationists, and who knows what. I think you have better arguments for your position, then calling the control of plants a simple misuse of words, even if control of perception is needed and correct under your paradigm.

What an engineer means is that his system controls the position of the plane, and he calls it an output. It has nothing to do with any old fashioned calibration, as it has nothing to do with Middle ages. He will do anything that is need, and possible, to make this plane follow the desired path. It has nothing to do with grammar, and with the fact than any input is an output of something else, and vice-versa. But I think we rotate now in a closed-loop with no reference whatsoever.

I thought it was worth understanding your terminology and bring it to some common denominator with the large family of control theory, but it is not very important. By the way, when I say "Control" I have a plane or a robot in my mind, not a differential equation. My only problem is that this robot must be at a given position at a given time, no matter how I monitor (sense) its motion.

When you see these lines on the display, the desired output of so many control loops, all designed to satisfy your finest perceptions, even if they cannot control (or because they cannot control) your perceptions, if you can claim that an engineer does not care about what is input or output, I can only ask: Who you call engineer?

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Date: Mon, 13 May 91 08:50:34 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Control

[From Rick Marken]

Ok, here are my system equations.

p = k1(o + d)
o = k2(r-p+de)

where p is the perceptual signal, r is reference signal, o is output variable, d is environmental disturbance and de is "neural" disturbance. I assume that k1*k2 is large and positive and that k2>>k1. k2 converts neural variables into physical variables. It is an amplification factor and has the dimension of physical units/neural units. It's probably on the order of 10000. K1 converts a physical variable into a neural variable so it's dimension is neural units/ physical units. It might be on the order of 0.1. So k1*k2 might be on the

order of 1000 (dimensionless loop gain). Note that o and d are assumed to be in the same physical units; what is perceived is the combined effect of o+d -- as in a compensatory tracking task, where cursor position is the sum of handle (o) and disturbance (d). The perception is assumed to be a linear function of this sum. If it's a log or exponential function probably won't change the basic conclusion. Note also that o is proportional to error (r-p) plus the additive effects of neural noise,de. This equation for output assumes that the noise is added to the efferent neuron carring the error signal to the output transducer.

Solving the equations simultaneously for output, I get:

o = k2r/(1+k1k2)- k1k2d/(1+k1k2) + k2de/(1+k1k2)

since k1*k2 is large, (1+k1k2) is approx. equal to k1k2 so:

o = r/k1 - d + de/k1

For a system with a fixed reference at 0 (r=0) we get

o = de/k1-d

So the steady state output of a control system (that is stable and has very large loop gain = k1*k2) is equal and opposite to the effect of the disturbance on the perceptual input. If there is a neural noise disturbance (de) than the output will also be directly proportional to this disturbance. Since de can be viewed as a disturbance to the output, it is clear that the output does nothing to counteract that disturbance. Thus, the output variable, o, is NOT controlled (given Bill's definition of control).

Now, solving thhe same equations for the peceptual signal, p, and making the same assumptions, we get:

p = r + de + d/k2

Since k2 is large, the effect of the environmental disturbance on the perception is attenuated; the greater the output amplification the greater the attenuation. So with very large k2 we get approximately

p = r+de.

So an efferent neural disturbance functions as a virtual reference signal!!! The closed loop does not remove its effect. This means that there could be problems if the variance of de is large relative to r. This could explain why higher level systems somtimes "intend" to get the "wrong" perception. What de does is make it look (to the higher level system that is setting r) that the lower level system is trying to get p to r + de. Since the variance of de is uncorrelated with the variance of r (probabaly) there is probably no problem as long as the variance of de is small relative to the variance of r. But there is probably an interesting relationship between the loop gain of control systems and the amount of neural noise that can exist and have them still function correctly. I don't think I'm up to solving this problem analytically -- but it might be interesting to people who study neural diseases. There may be predictable consequences of having certain types of neural instability. I will try to incorporate such a noise term into a hierarchical control model and see what it does.

Have any of the control engineers out there (such as Itzak) ever delt with the problem of noise in the control loop? Are there any theorms that might be useful to living systems modelers?

Regards

Rick M.

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Date: Mon, 13 May 91 13:31:08 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: BARKANA@DUPR.OCS.DREXEL.EDU
Subject: re: Control

Rick Marken (910513):
Damn be the engineer who would ever forget the noise, disturbances, or uncertainties. Otherwise, who needs any closed loop?

Izhak

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Date: Mon, 13 May 91 15:10:43 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: BARKANA@DUPR.OCS.DREXEL.EDU
Subject: Re: Control

Rick Marken (910513):
Now seriously, I would like to understand: What do you call handle (o), disturbance (d) and perceptual signal (p)? It is not the English that I don't understand. Is 'd' a measurement disturbance that you intentionally add so the subject cannot see the position of the handle? Why is 'd' some constant, while 'de' is noise? A system with so high gains and no integrations or filters (time-constants) would blow up at almost any level of noise, which amplified would bring anything to saturation.
Why does 'de' enter the same point with e=r-p? In a good control loop, some gain would be used, if possible, before the external noise enters. Ideally, o=k2(r-p)+de, which would give in the output de/k1k2.
Now may be you see why it is difficult for me to accept your model and nomenclature. My models have complex dynamics, noise (I usually call noise the signal that affects the sensor) and disturbances (that would affect the ideal output, sort of your 'de' if it was, for example, wind gust that affects the position), and steady state is a good point to start with, but again, it may never be reached.
I think that the main point here is: why does a constant disturbance affect the measurement 'p'? How does it appear in a biological system? I have been busy al my life trying to filter out and avoid those signal that may affect the desired output from reaching (close to) the reference input. If you have a constant output and a constant disturbances, and you

measure only the sum, whithout having any idea about it, then I must admit that this is what you control, but in my case I cannot call it control, just a mistake in design. Multiple redundant sensors would, and some processing would eliminate or at least reduce the effect of d on the output.

Now less seriously, my colleagues in adaptive control know that if they use a first order system to demonstrate some successful control, they get what some people call "The first Bar-Kana's principle":

'In order to control a first order system, one needs:
-No theory
-No adaptive control
-No principles.'

And if they use a second-order example, they get the second Bar-Kana's principle:

'On a first or second order example, one can prove.... everything'

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Date: Mon, 13 May 91 13:28:30 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Control

Izhak Bar-Kana (910513) asks:

>Now seriously, I would like to understand: What do you call handle (o),
>disturbance (d) and perceptual signal (p)? It is not the English that I
>don't understand. Is 'd' a measurement disturbance that you intentionally
>add so the subject cannot see the position of the handle? Why is 'd'
>some constant, while 'de' is noise? A system with so high gains and no
>integrations or filters (time-constants) would blow up at almost any
>level of noise, which amplified would bring anything to saturation.
> Why does 'de' enter the same point with e=r-p? In a good control loop,
>some gain would be used, if possible, before the external noise enters.
>Ideally, o=k2(r-p)+de, which would give in the output de/k1k2.

First, all the variables in the equations I gave can vary over time.

The output variable, o, is like the horizontal position of a handle (I use a mouse). The position of the mouse is determined by forces exerted by muscles on the skeleton. Neural impulses determine (influence, really) the amount of force. So, untimately, the position of the mouse, o, is determined by the size of the efferent neural signal arriving at the muscles. The whole, complex transformation from efferent neural signal into mouse position is linearized and embodied in k2. The neural signal going to the muscle is the error signal (r-p) plus any noise added by neural instability, de. Again, remember that I assume (r-p) and de are varying over time. So

(r-p)+de is the neural signal magnitude (at any instant) and k2 transforms this magnitude (via the physics of the muscle/skeletal system) into a mouse position, o. So I write the output function o = k2(r-p+de) because de is part of the neural signal. de is inside the organism -- indeed, I imagine that it is part of the charateristics of axonal firing. So any contribution of de to the neural signal gets transformed, via k2, into mouse position.

Yes, this is very simplified. If there are important consequences of this simplification on the conclusions I'd like to know -- really. I'm not really that great at math -- why do think I'm a psychologist anyway?

I have assumed that mouse position, o, is added to an externally varying disturbance, d. I am assuming that both mouse position and disturbance are measured in the same units. In our compensatory tracking experiments, o and d are both measured in pixals. d is a slowly varying (about .2 Hz average) quasi-random function of time. All the subject (controller) sees is the sum o+d (again, a time varying function). Actually, the sum is represented to the subject as the position of a line on the computer screen. I have left out the intermediate step of representing this line (which is actually the controlled variable in the environment) as q = o+d, where q is the position of the line on the screen. I just skipped the step that says p = k1q and wrote p = k1(o+d) instead. k1 is then a simplified model of the function that transforms the line position variable (o+d) into a perceptual variable. k1 is called the "psychophysical function" in psychology and, as I mentioned, most evidence points to k1 being logarithmic or exponential (with a fractional exponent) -- but I am assuming linearity for simplicity.

Finally, p, r, (r-p) and de are all neural variables. I assume that they are all on the same scale so there are no other constants.

> Now may be you see why it is difficult for me to accept your model and
>nomenclature. My models have complex dynamics, noise (I usually call noise
>the signal that affects the sensor) and disturbances (that would affect the
>ideal output, sort of your 'de' if it was, for example, wind gust that
>affects the position), and steady state is a good point to start with, but
>again, it may never be reached.

Yes, my algebra gives the steady state solutions. It assumes that they describe a system that works. Since I have built systems like this, that work with the disturbances I use (and, as you say, this is a pretty simple control system) then I trust that these steady state soluitons describe what I will find when I run the dynamic simulations (I will, using my spreadsheet model eventually).

> I think that the main point here is: why does a constant disturbance
>affect the measurement 'p'? How does it appear in a biological system? I

d is not constant. It is OUTSIDE of the biological system - it is an independent environmental effect on the controlled variable,(o+d). de is INSIDE the system. We usually ignore it, assuming that it is approximately 0. I just threw it into these equations to see how such a disturbance would be delt with in a negative feedback loop. It is treated like part of the refernce input (r). It is not cancelled out.

>have been busy al my life trying to filter out and avoid those signal
>that may affect the desired output from reaching (close to) the reference
>input. If you have a constant output and a constant disturbances, and you

>measure only the sum, whithout having any idea about it, then I must
>admit that this is what you control, but in my case I cannot call it
>control, just a mistake in design. Multiple redundant sensors would, and
>some processing would eliminate or at least reduce the effect of d on the
>output.

The output, o, varies inversely with respect to d (if d varies). de is assumed to be an inherent part of the neural system -- its like electrical noise in the thermostat. It is orders of magnitude smaller that the external disturbance. It is of interest only if variations in de are large relative to possible variations in r. I'm sure that in real nervous systems, de is negligible relative to the size of variations in r. However, it is certainly possible that the nervous system has control systems that are designed to eliminate de type noise. This is something a physiologist might know about. And it might be fun to design a circuit to handle this problem. But I only mentioned de at all as an approach to showing that variable o is not a controlled variable inasmuch as the other influence on o, which is the error signal, (r-p), does not act to compensate for de.

Anyway, I hope this clarifies the equations a bit. You are one tough cookie. I love having you on the net.

Regards

Rick M.

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Date: Mon, 13 May 91 14:28:55 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: Closed loop #2

From Ed Ford

Gary - Am sending a copy of Closed Loop #2 in the CSG newsletter on or about May 25th to all PAID UP members of the CSG.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, Arizona 85253 (Newsletter address)

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Date: Mon, 13 May 91 16:43:26 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: terminology

[from Joel Judd]

Bill commenting the 'wrong door' phenomenon:

>The
>transient failure to get through the door is immediately corrected, but
>is sufficient to start the verbal/cognitive system going, trying to
>correct the error at that level. However, all the mental chatter that
>accompanies this process (second-guessing, and so on) all happens too
>late because the error's already been corrected. It's accomplishing
>nothing more than satisfying a reference condition that says "I have to
>make verbal sense of this." If you really wanted a cognitive way to
>choose the correct door, you'd stop, go back, and look for some low-level
>perception that identifies the door that opens. Then it would stop being
>a cognitive problem. But who takes the time to do that?

A-ha! This is a mature linguistic system in action--making sense of anamolies, explaining irregular behavior, providing excuses, etc. This is the way I interpret Gazziniga and more recently Bruner. But how do you examine the "low-level perception" to take care of the problem?

Joel Judd

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Date: Mon, 13 May 91 23:58:03 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Lost Mail; CSG Meeting

From Ton Bourbon --
LOST MAIL: All incoming mail here was lost between 11:00 am, Sat., 11 May, and now. If anyone posted directly to me during that time, please try again. Gary, is there any way I can get the CSG-L items that were posted then?

CSG MEETING: The call for the next meeting will come out in a couple of weeks. For anyone new to CSG-L, the meeting will be in Durango, Colorado, 14-18 August 1991. Anyone who wants the previous CSG newsletter, with early information about the meeting can contact Ed Ford at: atedf@asuvm.inre.asu.edu

If you want to be listed in the call as a presenter, contact me directly. Also, contact me if you need written confirmation of your place on the program.

Presentations at CSG range from individual talks, to group workshops, to demonstrations of computer programs, to long discussions anywhere two or more gather. We ask that everyone bring multiple copies (20 or so) of anything they plan to present or discuss. Others try to read that material before a presentation. Some people simply bring handouts and talk to anyone who is interested. A few people have asked about poster sessions. Anyone who wants to bring posters should do so. We will find places for them and let discussions form pretty much as they do around the computers and rubberbands.

I havd record of the following people asking for a place on the program: Dick Robertson, Joel Judd, Clark McPhail, Joseph Lubin, Rick Marken, Chuck Tucker, Wayne Hershberger, and Ed Ford. Bill Powers says he does not want a spot, but we know better! Dick and Ed have asked for time to do a clinical workshop. Several of us will bring computers,

complete with joysticks and mice, just to offend those
who are tired of stick wiggling.
    I know many others will ask for time, after they receive
the formal call. This promises to be a good one.
    Best wishes,

Tom Bourbon <TBourbon@SFAustin.BitNet>
Dept. of Psychology
Stephen F. Austin State Univ.
Nacogdoches, TX 75962 Ph. (409)568-4402

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Date: Tue, 14 May 91 09:54:36 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: Lost Mail; CSG Meeting

Tom:

>All incoming mail here was lost between 11:00 am, Sat.,
>11 May, and now. If anyone posted directly to me during that time,
>please try again. Gary, is there any way I can get the CSG-L items
>that were posted then?

I will send you what you missed.

>If you want to be listed in the call as a presenter, contact me
>directly.

I would like some time to talk about CSGnet, how to access, the newsgroup
issue, software, tricks, etc.--Gary

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Date: Tue, 14 May 91 12:29:02 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: BARKANA@DUPR.OCS.DREXEL.EDU
Subject: Rick, Wayne, Control

Aha, so you know which output is THE output!!!
Many thanks, Rick, for your patience and detailed explanations. At least,
I think we start talking in English. I am very busy with some routine things
like classes, etc., and I cannot reply right away. And then, I must think how
to introduce math without leting you think that I play Jewish poker (I ask you
to select a number. You select 5, I say 7: I win. Then you want me to select
first, I select 9, you select 12, but then I say "double," and so on.
Everytime a new rule. When theere is danger that you have learned all rules,
I say "Karma" because Karma takes everything and the game is over).

And then, Wayne may call me again mathematician....

By the way, Wayne, as a psychologist you should beware from calling people names. I have heard that a psychologist was shot recently because he had called his neighbor "polyglot."

Rick, the simple illustrations are best to describe your idea. They do not necessarily PROVE much in an argument, if the behavior of a complex system is concerned. Here, we must treat the details carefully. I don't try to fail you, on the contrary. But nothing can change the fact that I must control the handle or the spot, and everything rotates around this aim. If you claim that a biological system can only control its perception, any one may accept, after some fight. But, if you tell an engineer that Control Systems control their input, it sounds funny, and the discussion is discontinued immediately.

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Date: Tue, 14 May 91 22:16:30 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Control vs. Regulation

[from Gary Cziko]

Yesterday I met with Evelyn Satinoff, a psychologist on my campus, who studies thermal regulation in animals. We had an interesting time comparing her physiological perspective on control and what I know of the CSG approach.

One stumbling block we had was the difference between control and regulation. Since regulation sounded like a synonym for control to me, she had me take home and read the following chapter of which the key paragraph is included below.

"The word _control_ is sometimes used interchangeably with regulation, but there are good reasons for distinguishing between the two terms. Control describes management. For a physiological exchange it is management of rate of functioning. With reference to blood pressure, a control of heart rate and stroke volume and a control of peripheral blood flow are required for regulation of the amount of blood within the arterial system and thus for regulation of blood pressure. Partial pressure of respiratory gases within body fluids is regulated; rate of oxygen consumption and of pulmonary ventilation are controlled. Body temperature is regulated--or at least, the temperature of some idealized and possibly theoretical mechanism in the body is regulated; rate of heat production in muscle and rate of heat loss from skin and respiratory passages are controlled. Food intake is controlled; but energy exchange may be regulated in that the energy content of the body tends to remain constant. But perhaps this is not a regulation after all, because we do not know of the existence of any specialized cells capable of responding to changes in energy content.

These examples are given to illustrate the differences in usage of the words _regulation_ and _control_; is it apparent that controls are required to achieve regulation." (p. 5)

[Brobeck, John R. (1965). Exchange, control, and regulation. In William S. Yamamoto & John R. Brobeck (Eds.), _Physiological controls and reguations_ (pp. 1-13). Philadelphia: Saunders.]

It seems to me that Brobeck is using control here to refer to the output function of a control system while he uses regulation to refer to the steady state thus achieved. Do others see it this way as well? Are we missing something by not having two words so we can say stuff like "regulation is achieved via control"? Woops, but then it sounds like we'd have to equate behavior with control!

This was an interesting article for another reason. Brobeck gives examples of physiological control/regulation, but they are all cast in input-output molds. So he speaks of adding CO2 to an animal's blood and seeing the effect on ventilation rate. "The slope of this line defines what may be called an "input-output" relation for exchange of carbon dioxide. This relation is important for every type of exchange" (p. 8).

So in control theory terms, he is talking about the relationship between what we call disturbance and behavior. But this seems to miss the point that there is no way that the animal can sense the disturbance independently of the effects of its own ventilation rate. Amount of CO2 influences ventilation rate, but ventilation rate also influences amount of CO2. Am I catching on or what?

A while ago Rick Marken stated that all non-CSG tracking studies looked at tracking as input-output relationships and not as controlled variables. I now wonder if this is the case also for the physiologists. Have they also missed the point that what is detected (input) is controlled and not the output necessary to achieve control?--Gary

P.S. Don't forget my main question about regulation vs. control.

Gary A. Cziko
Associate Professor
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Date: Wed, 15 May 91 00:46:44 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>

[From Bill Powers]

I gave my address wrong -- it's an internet address: powersd@tramp.colorado.edu. But you saw that in my headers, I suppose.

Joseph Lubin (910514) --

Welcome. Hierarchical control theory is pretty good, too.

It's really too bad that the best name of all has been spoiled: cybernetics. It means "steersmanship." The essence of control. Or it did before all the guessing about control theory began, back in the 1950s. Now it means Cyborgs and all that stuff.

Oded Maler (910514) --

We say "control theory" for short, hoping that no engineers are listening. Of course you're right. We have to qualify the term.

>The problem of trying to explain at some level the behavior of
>animals having a nervous system, and the problem of building artificial
>creatures that can operate in rich and non-trivial environments seems to
>me hard enough. Not that I underestimate Enzyme soups, energy clouds,
>or changes in stock market prices, but I I can't see how a unifying
>principle can be found.

I think that control theory is specifically a unifying principle FOR LIVING SYSTEMS. As others have pointed out in this forum, all control systems are either living or products of living systems, as far as we know. There are no known other naturally-ocurring control systems. Control systems are not, for example, part of the physicist's model of the material world (and maybe they should be, but that's another subject).

This is important because control theory is missing from all purely physical accounts of the operation of living systems. Only in biochemistry do we see some attempts to describe chemical feedback systems, and in that field the prejudice against the concept of a reference signal has kept the idea of hierarchical control from being used, as least as we use it -- in fact, "feedback" is hardly ever associated with "control.

If we accept the premise that no behavior of a living system ever occurs except for the purpose of controlling some variable that impinges on the system, we have a principle that applies to ALL behavior of EVERY kind in EVERY living system, whether simple or complex. I think that counts as a unifying principle even if it doesn't rule out other possible unifying principles.

>the main theme of this theory
>(as I understand it) becomes a tautology. Because finally, there is a
>mechanism that relates pereception to action, no matter what structure
>and levles we can attribute to this mechanism.

But in general there is no relationship between perception and action, in control theory. You get such a relationship only when there are no exogenous disturbances acting on the controlled variable and only when the reference signal is constant. In a real universe, even given a temporarily fixed reference signal, the action will depend on disturbances, with the result that perception does NOT depend on disturbances (given perfect control). The perception can remain closely matched to a fixed reference signal while the action goes through its entire possible range. If the reference signal is varied by a higher-

level system, there is not even a reliable relationship between the action and the disturbance, because the definition of "zero error" is continuously changing. The principle difference between control theory and stimulus-response theory is that the latter claims a regular relationship between stimulus and response, while control theory shows that this is either an illusion (the "stimulus" is really a disturbance applied to a controlled variable) or simply doesn't happen. So I don't think we have a tautology here.

>But, in fact, there is just that underlying
>*mechanism* which causes the behavior, which looks to us *as-if* it is
>an attepmt to control some percept. [There is a nice article by Searle
>about the role of consciousness in psychological explanation,
>somewhere in BBS '90 or '91.]

There's no "as-if" about it. The mechanism in question actually does behave in such a way as to control a "percept" relative to a preselected state. This is not a metaphor, but a literal description of how the system works. Searle hasn't a glimmer of understanding of control theory.

But you are right about one thing: control theory does not explain consciousness. The best we can do is to say that purposive systems of this kind sometimes operate in the conscious mode, and sometimes in the automatic mode. I do not understand what makes the difference between those two modes, and I don't think that anyone does. "Purpose" suggests consciousness to most people, but control theory shows how we can explain every functional aspect of purpose without invoking consciousness. So purpose and consciousness are not the same thing (although they are naturally associated when we think of those purposes we are conscious of carrying out).

Izhak Bar-Kana (910514) --

It isn't that we ignore your words: it's that we can accept them as truth, but truth of a kind that leaves out other important truths, particularly the one we have found the most startling and the most informative: the truth that a control system can control only what it senses.

In the world of engineering, the engineer has full knowledge of both the environment of a control system and the internal design of the control system. So he can point to a consequence of the system's actions and say "There, that's the output that I want to be controlled, and here, in the system, is the feedback signal that represents that output." In doing this, he does not have to pay any attention to the fact that he must use his own senses to see that output. Literally, however, FOR THE ENGINEER, the output being controlled is known only in the form of a perception (whether aided by instruments or not). That is a fact, but it is irrelevant in engineering.

It is not irrelevant in trying to understand how the engineer works. When we look at the design of the engineer himself, according to our best neurological and physiological models, we can see that the engineer's entire world must exist in the form of sensory signals and higher-order functions of them, also represented as signals. In a way you have given a nice example of this in talking about controlling a satellite:

> One problem, for example, (I hope it is clear that we don't talk about
>train engineer's problem) is that we here must control the position of a
>satelite up there. So, the position of the satellite is THE object of
>the control. I have no direct means to measure it, as I have no direct
>means to affect it. Yet, everything in my control problem revolves
>around this control object. I can only use some remote measuring
>instrument, but I have no intention to control its output.

So how does the engineer know of the position of the satellite (other than by looking up)? Only, as you say, by using some remote measuring instrument. He has some moderate amount of faith in the instrument, after calibrating it, but that does not change the fact that ALL he knows of the satellite's position is in the form of this instrument's reading (which consists of numerical digits, not positions). He does not, in fact, know the position itself. He knows only the reading, and he has a complex theoretical structure in his head that converts this reading into a concept called "position." He calls this concept the "output" he wants to control, but in strict literal truth it is a perception.

The engineer may have no intention of controlling the output of the measuring instrument, but in fact that is all he can control. He has no other way of knowing the position of the satellite but through the use of earthbound measuring instruments (his eyes among them). He trusts that the instrument-readings correspond in a regular fashion to the "actual position" of the satellite (with all necessary corrections applied, for example the time-lag of light rays and radar pulses and the various motions of the earth itself). This trust is an epistemological statement, but its truth or falsity do no matter here: we are talking about practical requirements. The engineer IMAGINES that he is controlling the position of a real satellite, up there in the sky, and he can produce all sorts of justifications for accepting this imagined correspondence. But he can't know that position without using the instruments, and he can't know the effect of his remote-control actions until he sees what the instrument-readings do. Whatever he believes is actually going on, he is stuck, as a practical matter, with controlling a perceptual representation and not the thing itself. His epistemological beliefs make no practical difference at all.

All animals, and most human beings prior to the age of higher learning, necessarily act from the epistemological position that the perceived world is the world itself. There is, of course, no alternative. Speaking for human beings, the reality we know as solid and real, upon which we act and which we intentionally alter in some regards, is the only world there is. "Perceptions" don't exist except, for some of us, as philosophical abstractions or "signals" in a model. When we forget about models and philosophy and just look around, we see the world, not perceptions. When we look up into the night sky and see that serene and untwinkling point of light moving steadily and silently among the stars, we say "Look at that! There's a satellite!" We don't say, "This is a perception of a satellite, a signal in my brain." When we point at the satellite, we see our own hands with forefinger extended. We don't say that there is a perception of a forefinger, nor do we pause to wonder about the relationship we call "extended." The relationship is just as much "out there" as the finger is. It would never occur to us to wonder what sort of thing it is, out there, that we call a "relationship."

This simple and self-evident world has conceptual holes in it. The

biggest hole lies between the intention of pointing at the satellite and the immediately-experienced actuality of pointing at the satellite. How is it that a mere intention, a figment of the mind, actually causes this pointing to occur? Control theory provides a plausible way to fill in this gap, a way that is as self-consistent and as consistent with observation as any finding of physics. In a manner of speaking, it IS a finding of physics. To construct this model, however, we are forced to readjust our conception of this whole apparent reality, because the control model can work only if the satellite and the finger exist for the controlling system as signals produced by sensory inputs and subsequent computing functions in the brain.

In your objections to the concept of control of input, you have consistently assumed that the engineer can know the actual state of the output. Within the boundaries of the usual world of engineering, observing is not a brain process: it simply consists of noting what is there, while the role of the engineer's brain in making this possible remains silently in the background. In our explorations of control theory, however, we make this brain-in-the-background explicit. Even in talking about artificial control systems, we habitually take the point of view of the control system, a thing that few engineers would see any reason to do. We say, "If I were that control system, what world would I be experiencing?" And the answer, of course, is that the world would consist completely and exclusively of the signals coming out of the sensors. We could not know what is causing those signals; we could not even know whether they represented light or magnetism or sound. They are just signals. They get fancier labels only in the context of other signals that are also just signals -- or in the mind of the Engineer, who occupies an omniscient position in relation to this tiny control system and its surroundings.

When I speak of what "we" think on this subject, I am speaking of those who have internalized this model to the extent of relabeling their own ordinary experiences as "perceptions," at least when thinking in the modeling mode. This relabeling has come to most of "us" in a moment of sudden illumination that forever alters how we understand nature and ourselves. Nothing is changed in ordinary experience: "out there" still seems to be where it has always been. What changes is its meaning in relation to how we intepret the behavior of others and ourselves. This threshold of understanding is either passed or it is not. Once it is passed, the world of experience not only contains new implications, but IT MAKES A GREAT DEAL MORE SENSE THAN IT MADE BEFORE. This is what has attracted so many people to the CSG version of control theory in the context of living systems. So many questions are answered, even those we hadn't thought of asking. So many holes are plugged that we haden't even recognized as holes.

I think that Rick Marken was talking about a noisy comparator, which creates an irreducible random variation in the effective reference signal. Nice analysis, Rick.

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Date: Wed, 15 May 91 13:42:02 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>

Subject: Back on and file to follow

I just got back on to find 82 posts on my files. i have not read them yet but will and post when energy returns. I am posting a file which refers to history on the NET.

Regards, Chuck

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Date: Wed, 15 May 91 13:44:35 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Comments on several threads

EXCUSE ERRORS PLEASE

[From Chuck Tucker 910515]

I have been away from the NET for about 10 days but did have a chance to read the posts through 910504 and found them very interesting and thought provoking. Here are some of my thoughts.

Comments of Statistics and Competition

I was wondering if all of us who are critical of the use of aggregate statistical analyses being applied to individuals and at thae same believe that extreme competition leads to many of the problems we have amongst people have abandoned the use of "curving" or distributions for deciding what grades that your students will receive. I believe that one of the most serious problems of our public education system is the use of "curves" to determine a student's grade rather than the use of a standard set by the instructor/teacher and understood by the student. When the "standard" is merely doing better in a statistical distribution that some others then students only have a minimal notion of what is "excellent work". When you have raised a generation of parents and teachers who have experienced such procedures and continue to pass them on then we should expect a continually lowering of the statistical standards (by the way, this would be an excellent experiment to be done by those in education - does it lower standards?). The point - to be consistent with control theory a teacher should set a standard, encourage a students to use that standard and judge the student's performance by the standard set without regard to any statistical distribution of an aggregate (a college class is not a group either!). When this is done all can get high, medium or low grades - students can study together and there is less conflict between them and between students and a teacher (even though I do get complaints when they don't get high grades but I am the only class that approaches grading in this way and a less than high grade is a disturbance). I hope all of you do it is a manner consistent wtih control theory.

An ethical standard of control theory (thru 910504)

I have found the discussion begun by Ford's (910430) answer to Bill's discussion of competition (910427) to be very useful and I

think that an ethical standard can be constructed from the exchange of posts and some reference to previous writings. I will briefly support my suggestion with comments from the posts.

It was Ford who suggested the the CSG gets along so weel because "we repect each other and what each one of us has to offer. In short, our values are very much the same". But notice that rather than focusing solely on this aspect of Ford's post, Rick (910501) mentioned a "higher power" and the phrase "verbalisms rather that phenomena" and he disagreed with Ed's suggestion that we need more faith. Then Judd (910501) brings the convesation back to Ford's original point by saying the "faith" and "values" rather that a particular religion can be used as higher level concepts to bring about cooperation. Bill (910501) made this comments on "higher power" demonstrating that a "higher power" can be part of a control system and used cooperatively as a phenomena. Rick followed (910502) with a discusson of his view of religion and while noting that was not taking an "anti-religious" view he did end his post by writing "Well, enough ranting for tonight" followed by a recognition that he was wrong in noting that religion was just verbalisms. On the next day Rick (910503), after notiong he may have offended someone, suggested that the subject be changed but Bill returned to the discussion (910504) by making the concepts of religion, science and logic almost on the same level then Mary (910504) notes how control theory can use higher level concepts like "higher power" without a particular religious organization's "spin" on it. She also mentions that it is "To one's higher power or certainly to a higher level in oneself" that one goes to resolve a conflict. Now, what I make of these exchanges is an ethical standard at the highest level that is used by those who use and believe in control thoery. This higher level standard is: all human beings are self-regulating control systems and should be respected as such. Rick is correct when he says that most religious leaders (and their religous doctrines) do not respect humans as self-regulating control systems and do try (rather unsuccessfully in most instances) to coerce/force/bribe others to follow their rules (which many do not follow themselves). But I would claim (see Bill's Chapter 17 in BEHAVIOR:...) that control theory contains this ethical standard and that religions would do much better if they would use this standard also. Thus, we get along so well because we place a value on and find as important that each of us is a self-regulating system.

Some references on religion

Those of you who may be interested in some different ideas from those in a "mainline" religion I would suggest:

James R. Adams (1989) So you think you're not Religious.
Cambridge, Ma. Crowley IBSN 0-936384-69-7

John Shelby Spong (1988) Living in Sin. Harper and Row
IBSN 0-06-067505-5

John Shelby Spong (1991) Rescuing the Bible from Fundamentalism.
HarperSanFrancisco ISBN 0-06-067509-8

Adams is an Episcopalian Priest in Washington, DC and Spong is the Episcopal Bishop of New Jersey.

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Date: Wed, 15 May 91 11:10:52 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: understanding the model

From Ed Ford May 15, 1991

Tom - your explanation of what I had in mind hits the nail on the head.

Bill - "concerning internalizing the model" - The struggle I had to understand CT was that I was going into uncharted territory, trying to understand concepts which depended on an experience I had never had (like men trying to understand what it is like to give birth to a baby). The illusion in all this is that when we read an explanation of the model, we think we understand the concepts, but we don't. The model must be tied to an analogous experience. Once I had experienced (read internalized) this model (with the help of your belief in my ability to achieve that goal), then my whole understanding of human relationships and the world of counseling changed. I am reminded of my son, Joseph, when he was nine. His teacher strongly recommended glasses. We had him test and ordered the glasses. We picked them up, then picked up Joseph after school. He got in the car, settled down, then put on his new pair of glasses. Then came a loud "WOW, MAN, LOOK AT THE TREES, LOOK AT THAT HOUSE, MAN, THIS IS GREAT!!! I think all of us who really understand CT have had a similiar experience of discovery. I know I have.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, Arizona 85253 (Newsletter address)

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Date: Wed, 15 May 91 16:28:14 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "W.B. CUNNINGHAM - ATCD-GI AV 6 by way of Gary A. Czikog-cziko@uiuc.edu" <CUNNINGB%MON1@LEAV-EMH.ARMY.MIL>
Subject: Bill Cunningham

Preliminary Note from Gary Cziko:

Bill Cunningham is now on CSGnet. He has also sent me a paper entitled "Cybernetics for Complex Decision Aids" which he would like to share with interested individuals via the net.

Please send a personal message to me if you would like a copy.--Gary

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FROM: W.B. CUNNINGHAM - ATCD-GI AV 680-3441/3273

I have been trying to make contact with Bill Powers for several days. I have his new PO Box address & phone number, but understand he is suffering through a major move. Hope this makes makes it to him.

Also hope to make contact with CSGNET.

I work for the Army in an office charged with defining what things will be like 10-30 years out. My particular function deals with command and control, and with military intelligence at the upper tactical levels. Both topics deal with distillation of massive amounts of information, developing a mosaic from many tiny sensings. Our computerized approaches to this have been classic deterministic machines with input. I'm trying to get beyond that, at least in my own mind.

I am another control systems EE who has strayed from the fold. I took a cybernetics course from Larry Richards at Old Dominion Univ this past semester, finishing with a term paper that describes the intelligence processing business in terms of the hierarchical model of perception. There is virtually a one for one correspondence. This may seem like old business to you, but it represents an exciting breakthrough at this end. The model provides some real insights, and some clues on how we should change our automation approach. Regardless of how we automate, intelligence processing is very much a matter of perception, which is why the model is so relevant. That's why I'm trying to open a dialog.

Hope this reaches a sympathetic ear at the right address.

Bill Cunningham
(804) 727-3441
Internet CUNNINGB%Mon1@Leav-EMH.Army.Mil

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Date: Wed, 15 May 91 20:43:22 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>

[From Bill Powers]

Gary Cziko (910514) --

Yes, Gary, you are catching on or what. Your analysis of Brobeck's piece is essentially the same as mine. Brobeck says "control" or "manage" where we would say "vary" or "influence"; he would say "regulate" where we would say "control."

Others have distinguished between regulation and control on a different basis (we would see it as constancy of reference signal). "Control," to some, gives more of a feel of keeping something from changing, while "regulate" connotes more of a wiggly variable sort of thing, CAUSING something to change (just the opposite of Brobeck). But the difference has nothing to do with the kind of system that is involved; it's just whether the reference signal is constant or varying (or, in Brobeck's case, whether you're talking about the output effector or the controlled variable). Because we have access to the correct technical description of control processes, we don't need to make distinctions based on the sounds of words and their private connotations. I don't see the need for two different words to denote the same process, so I choose "control" as the generic term. "Regulate" means the same thing. So does "manage." Just try controlling, regulating, or managing something without being able to perceive the results of acting and without any concept of the desired state of the regulated, controlled, or managed variable.

If Brobeck understood hierarchical control theory his treatment of blood pressure wouldn't be so confused. Blood pressure per se is not a controlled ("regulated") variable; the organism is not set up to maintain any particular blood pressure. The point is not pressure, but delivery of blood where it is needed, and the controlled variables will be defined by the kind of need (effects of nutrients, energy concentration, temperature, waste concentration). There must be interactions among the control systems, in that demands due to errors of one kind affect other controlled variables which must be independently controlled if they are not to be substantially disturbed. There must be several layers of control, for changes in overall demand deplete the supplies for all control systems, so the supplies (total blood volume, total volume flow rate) must be maintained by separate control systems. When peripheral capillaries constrict, the blood volume flow rate would decrease unless the rate and stroke of heartbeats increased. The stroke of the heart must decrease when the rate goes up if peripheral resistance remains the same and total flow must be maintained constant. These control systems share many mechanisms of variation. For example, temperature control and control of delivery of blood nutrients both make use of vasodilation and vasoconstriction. An analysis of any one control system as if it were isolated is inadequate. You can't understand the operation of any one of these systems until you have a model of all of them, connected into a hierarchical parallel system. This sort of model isn't going to be constructed by playing with words.

>A while ago Rick Marken stated that all non-CSG tracking studies looked
>at tracking as input-output relationships and not as controlled
>variables. I now wonder if this is the case also for the physiologists.
>Have they also missed the point that what is detected (input) is
>controlled and not the output necessary to achieve control?

I don't know about ALL non-CSG tracking studies and ALL physiologists, but what you say is certainly true of many of them, probably the majority. But I've been dripping water on this stone for a whole generation, and others have been, also. I think a small dent is developing.

I have never understood why people think that the analysis of a control process in a real system is just a matter of how you look at it. If you represent the parts of the system as accurately as you can and connect them together in the most realistic way you know about, the model can behave in only one way, the way it behaves. With apologies to Izhak, who has not yet relented but will do so (I predict), a proper analysis of any control system shows that the effector output is NOT controlled except in the rare case where it is also the controlled variable. So no proper analysis of a tracking study could possibly result in the conclusion that it's an input-output phenomenon. I think that the secret explanation is that most people who dabble in these matters don't know what they're doing -- at least in the soft sciences. They think it's all a matter of vocabulary.

A note to friends. Mary is in Mercy Hospital in Durango, recovering nicely from surgery to correct an abdominal blockage, the result of adhesions from last Fall's surgery. The crisis started the day the moving

van arrived. Life is scarey and inconvenient.

Best regards to all -- Bill Powers

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Date: Thu, 16 May 91 09:35:15 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: heredity/environment/experience

[from Joel Judd]

STARTING TO CATCH ON DEPT:

The following excerpts from a summary in the Chicago Tribune of an article in _Health Magazine_:
"... according to Robert Plomin and Judy Dunn, Penn State U. Child Psychologists...(personality is) not all genetic, they say, and it's not all environmental.

"According to Plomin and Dunn, your siblings are no more like you than if they'd switched homes their first day of life and grown up in, say, your boss's or dentist's family.

" 'It's startling at first,' Plomin says. 'But all the evidence points to the same conclusion: What we've thought of all along as 'shared family environment' doesn't exist.'

"From our first day of life, and perhaps even before, we perceive everything that happens to us through a unique filter, every skewed event changing us in a way that affects how we'll experience the next event. In fact, Plomin says, it looks as though growing up in the same familial world actually works to make siblings different."

It goes on to mention some of the authors' conclusions about important factors in growing up, a big one being siblings' "perceptions" of the amount and kind of attention each one receives from parents. Pre-linguistic infants (14 mos.) appear to be aware of how much attention their siblings are receiving and develop ways to get "back the spotlight" as they put it. The article concludes:

"For parents who still cling to the notion that they can mold their children's temperaments and future, the bottom line may seem like the paralyzing punch line to a cosmic joke: Yes, what you do to and for your offspring matters very much, but only in ways you can't control or even foresee.

"The filter through which your child perceives you and the world is evolving constantly and is partly (?) of his or her own making."

Hmmm. It would be interesting to see the research paradigm these two have been using.

Joel Judd

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Date: Thu, 16 May 91 09:32:58 -0700

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Control, Perception

[From Rick Marken]

Bill, my best regards to Mary. I hope she is up and around soon. I will hold off on my continuation of the religion discussion (brought back by Chuck Tucker [910515]) until she can participate.

First, I did try out the addition of a neural disturbance, de, to my spreadsheet control model. Sure enough, the control system brings the perception, p, swiftly and accurately to r+de rather than r. I tried this in a two level hierarchical model (two systems at each level) with de added (as a constant) to the error signals of each lower order system (a different value of de for each system). This had no effect on the ability of the higher level system to get their perceptions matching their references. If the higher level systems set one lower level r to 10, the lower level system would make its perception equal to r+de. The higher level systems needed the lower level perception to be at r in order to achieve their goal. So they automatically adapt by changing r appropriately -- to r-de. I don't know if this is a general property of hierarchical control or whether it only happens in the specific situation I used (constant de and higher order systems controlling variables that are linear functions of the lower order perceptions). This would require some linear algebra -- any math wizzes want to tackle it? At least the spreadsheet model confirms the conclusions of my analysis (the spreadsheet model is a dynamic simuation of a hierarchy of control systems, the variables change values iteritively).

Next, some thoughts on perception:

Bill Powers (910515) writes, in a passage that again brings tears to my eyes:

>All animals, and most human beings prior to the age of higher learning,
>necessarily act from the epistemological position that the perceived
>world is the world itself. There is, of course, no alternative. Speaking
>for human beings, the reality we know as solid and real, upon which we
>act and which we intentionally alter in some regards, is the only world
>there is. "Perceptions" don't exist except, for some of us, as
>philosophical abstractions or "signals" in a model. When we forget about
>models and philosophy and just look around, we see the world, not
>perceptions. When we look up into the night sky and see that serene and
>untwinkling point of light moving steadily and silently among the stars,
>we say "Look at that! There's a satellite!" We don't say, "This is a
>perception of a satellite, a signal in my brain." When we point at the
>satellite, we see our own hands with forefinger extended. We don't say
>that there is a perception of a forefinger, nor do we pause to wonder
>about the relationship we call "extended." The relationship is just as
>much "out there" as the finger is. It would never occur to us to wonder
>what sort of thing it is, out there, that we call a "relationship."

This is a topic that is dear to my heart because it caused me some grief at the last CSG meeting. I was trying to make the following point: when teaching people about control theory, why not start by saying that people control things in the world (even the word variables might require

more explanation than you want to go into). Forget about the fact that it is perceptions they are controlling -- at least at first. I was making this point because I think it is hard enough to get people to understand that their behavior is a process of control without also adding that it is perceptions that are controlled -- since what we means by perceptions (as Bill noted above) is what most everybody in the world thinks of as objective reality. I think, pedagogically, that it is hard enough for people to understand that they lift a glass by controlling the height of the glass and that this is control because the same result is produced in the context of disturbances -- other things, besides yourself, that also (and unpredictably) influence the height of the glass. I think it just makes things more difficult, at first, to add that "lifting a glass involves control of your perception of the glass". This requires a whole epistimological discussion (which Bill's paragraph above would be just the start of) and I doubt that many people are up to it.

Yes, I know that we control perception. My latest paper is a paen to the fact that behavior itself is a preceptual phenomenon -- from both actor's and observer's perspective. But this is a very hard concept to get -- as most CSGer's will testify. As Bill notes in a subsequent paragraph, we all (CSGers) see the world as "out there" -- there are things out there like hands and keyboards and monitors and colors and so on. I KNOW (given my understanding of many different, interrelated models) that these are perceptions. But most people (and Wayne went along with me on this -- thanks Wayne) think of perception as a bit of an illusion -- probably because, if they took a course in perception at all it was the perceptual illusions that were most memorable. Moreover, pereption is taught as though it were the study of the relationship between our experience and REALITY. But it can't be that. Nobody knows what is really "out there" because nobody can see past their senses. Perception is really the study of the relationship between our experience and our current MODELS of what is going on on the "other side"-- because that's all we have is models. These models are built from comparison and test and reasoning about PECEPTIONS. It can be no other way -- unless someone has a private pipeline to the outside world -- and most people assume that they do (after all, you can just open your eyes and see what's out there). Thus, it is very difficult, I think, to understand the most important concept in control theory right off the bat -- the concept that we control perceptions, not reality.

Besides the difficulty problem, there is another problem that Wayne and I also agreed on. When you say people control perception you give the impression that problems can be solved by simply perceiving things in a new way. Well, this is actually true, but, to some, it could sound (I think) like the solution to personal problems is to discover a new, illusory way of experiencing the world. Of course, this is not so. People can learn to perceive things in new ways -- but it is not arbitrary. Perceptions are constrained by whatever it is that is out there as well as by the processing capabilities that are probably built into the nervous system. Changing perception is not easy -- look at the trouble people had perceiving the sun as stationary and the world moving or that a control system controls its perception of a controlled variable -- not the external variable itself. Changing perception is not easy -- even when you want to.

Anyway, it is important for those unfamiliar with CSG to understand what we mean by "perception". It's really not a mysterious, ethereal thing.

You experience perception all the time -- we all do -- we just call it "the real world". When you control the position of your hand, you are controlling the perception of that position (and hand). Some things (like relationships and systems concepts) seem more like perceptions than other things (like hands and keyboards). A relationship like "next to" seems like a subjective interpretation, as does the perception of someone as "religious". But they are the same in the control model -- just at different levels in the hierarchy. Indeed, the difference between "subjective" and "objective" in the control model (as, I think, most people use those terms) are just two ends of a continuum defined by the levels of perception in the control model. Objective is usually used to describe lower order perceptions (intensities, sensations ("that's really red") configurations ('That's really Bill") while subjective describes higher order perceptions (relationships like love, principles like "do unto others", system concepts like the Dodgers (well, they seem real enough to me -- oops).

Anyway, I hope that my "hierarchical behavior of pereption" paper redeems me in the eyes of those who saw me as someone who was trying to minimize the importance of perception in control theory. I was trying to make a pedagogical point -- but never mind (a la Rosanne Rosanadana).

Regards

****

Richard S. Marken USMail: 10459 Holman Ave
The Aerospace Corporation Los Angeles, CA 90024
Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)

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Date: Thu, 16 May 91 12:55:10 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Religion, Name and Kent' MS

[FROM CHUCK TUCKER 910516]

The posts from 910504 through 910515 were of the same thought provoking quality as has been found on the NET since its existence. It is so wonderful to be able to read such clear and well formulated statements. The contrast with what I find in most journals and books is just startling. Keep up the good work everyone.

ON RELIGION

I was so pleased to read that my statement on this topic (910515) was put forth by several before I wrote it. I was especially pleased with the fact that I was able to discern what Ford (910508) was controlling for with his initial statement on the topic. Knowing the theory or formulation that another is using gives great assistance in figuring out their goals. This is one of the reasons that being able to model the higher level perceptions is crucial for the advancement of our own

understanding as well as being able to show other the practical aspects of PCT.

ON A NAME

I agree with Bill (910515) that it is unfortunate that 'cybernetics' is not as useful a name as it once was. I have in various places called the theory 'sociocybernetics' and as you know Maltz presented 'psychocybernetics'; both of these, as proposed by others, have a different basis from Powers' Control Theory. Remember that Kent proposed this label to avoid the identification of the formulation with other 'control' theories that are numerous in the field (as reviewed by Gibbs) and to note that the theory takes a different epistemological approach (most recently described by posts of Marken, Bourbon and Powers in answer to the queries of Izhak) than all other formulations (what I call 'constructionist' but I don't want to open that can of worms again).

The word 'perception' does have the difficultly of being contrasted by many with the word 'reality' yet I also hear the statement: "Perception is reality." (Tom Peters makes this point is his "excellence" book) but if we can make the point that 'behavior" = the control of perception without getting into the reality argument (this is not very likely) then I think we have accomplished our purpose, which is, to give a name to this formulation which is consistent with its epistemological assumptions but marks it as different from others who who claim to have the same interest but in our view do not, yet is not an odd label. We may want to come up with a statement which we all can use to describe the formulation by indicating its main similarities and differences with other formulations that also contain the word 'control.' This would be especially useful if we go on to, in the vain of Gibbs, establish 'control' as the central notion of the life sciences. I think Bill's statement back in November (?) might be a good place to begin.

ON KENT'S MS

As I have said before on the NET, I think that Kent's MS is very important and I have encouraged him to continue to work on it. I have send him some suggestions by snail mail since I wanted to mail him some other articles that may be useful to him but there is one issue in his MS that I would like to see discussed on the NET which involves his Figure 1. Schematic Diagram of a PCT Control Loop which follows Powers 1973 Figure 5.2. In the "environment" he has one box labelled "controlled quanity in environment" and he notes (p. 7) "In ordinary circumstances controlling a perception also brings about control of some physical aspect of the environment, the controlled quanity, to use Powers's teminology. When a mechanical or human cruise control system is working properly and nothing get in the way, the car in fact moves down the highway at the desired speed of sixty-five miles an hour with only slight variation. Thus, perceptual control may also produce effective environmental control." Now this may be technically correct but I think it may lead to the idea that we control the environment NOT perceptions

and lead away from the epistemological and practical basis of the formulation. If this is a problem I think others may make it also (even some of us) thus I would like to have some discussion of it (I may be off base here) to help Kent as well as the rest of us on this issue.

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Date: Thu, 16 May 91 19:37:21 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Wayne's Paper

[From Rick Marken]

Wayne Hershberger -- could you please send me a reprint of the JEP:HPP paper you mentioned on ambiguous motion. I probably won't get to a library soon and I do think it mighty help with my "control of perception" project.

Sorry to use the net for this but I don't have a copy of your e-mail handy.

Thanks much

Rick Marken
10459 Holman Ave
LA, CA 90024
marken@aerospace.aero.org

==========

Date: Fri, 17 May 91 09:48:30 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPROBER@BOGECNVE.BITNET
Subject: mtaylor re learning errors

To: Martin Taylor Re - Error Production, Disciplined Imagination
[From Dick Robertson]
You said >There is a fairly common real-life parallel to this effect that has always interested me...< in reference to Rick Marken's (910509):

>>The closest thing I have seen to what looks like intentional production of
>>(technical) error is found in my article with Bill Powers in Hershberger's
>>Volitional Action book. In the polarity reversal experiment there is a
>>1/2 second period where the subject actually makes things WORSE -- increasing
>>the discrepancy between target and cursor in an accelerated, positive feedback
>>sort of way. When you are a subject in this experiment you can actually feel
>>it happening "against your will"....But this happens only because the higher
>>level systems cannot correct things fast enough. It is explained just fine as
>>the behavior of a two level negative feedback control system that is trying
>>(as always) to minimize (technical) error.

>...I tried to get the people in this human-factors institute
>to explain it to me when I was a student here 30+ years ago, and I still
>haven't found anyone with an answer. Maybe CSG people have an answer.
>Here's an example situation; there are parallel situations where the same
>thing happens, but I think one example is enough:

>In a certain corridor there is a doorway with two swinging doors (i.e. when
>both are open it's a very wide doorway, but opening one is all one needs
>to do to pass through.) Usually, one of the two doors is locked shut,
>and people pass through the other. It is always the same one that is
>locked. The first time someone encounters this doorway, they may go through
>the openable door, and if so, everything is OK thereafter. But if on
>this first encounter they try the locked door, and then move to the
>unlocked one after failing to get through the locked one, there is trouble.
>For a long time thereafter, even if they use the doorway several times
>a day, they are liable to try the wrong door first. The subjective
>impression (it has happened to me a few times) is that one mentally
>oscillates "I know it isn't the one I first thought it was, which means
>it is not that one, because I think it is that one, so it must be the
>other one....BANG!" It seems that the more times one goes through this
>routine, at least for perhaps tens of experiences, the more likely it is
>that one will eventually choose the wrong door. It's a very frustrating
>thing, very common (other people confirm it happens to them, and it can
>be observed casually), and without any explanation that has satisfied me yet.
>Is there a CT explanation? It sounds a bit like Rick's experimental
>situation, though that corrects itself more quickly.

This brought to my mind Edwin R. Guthrie's theory of learning (that we learn what we do)- which was the only learning theory I ever got very excited about eons ago when I took my first course in it. Guthrie's wasn't much of a theory; it consisted mainly of "explaining" learning by postulating that what we observe to be the facts are the facts (like most psychological "theories"), but the particular observations that he described were very concrete and very practical. I still use them when I want to teach somebody anything that we call "skill." When I want to teach any of my students word processing (e.g.), I don't bother showing him or her anything. I have her/him sit at the keyboard and I give verbal instructions as to what to punch. Providing that I don't screw up the instructions (and the subject doesn't innovate) s/he never makes any move or per illustrations by which I've been persuaded that old Guthrie was right: we construct memory (of the skill we want to acquire) out of the perceptual signals we have to produce in order to satisfy our references.

You might see how we could begin to forge an answer to your question about what CT might have to say about this as follows (this is a first approximation):
Goal (RS)
/ (get where you are going)
Input (perceive 2 doors)--> Program
/ (choose one)

(Now here we come to an interesting situation; you described an intricate set of cognitions that you go through - I, on the other hand often find that I have no conscious awareness at all until the moment the BANG occurs, I have been thinking of whatever I am doing and my actions would seem to imply an assumption that my body "knows" which door to head for (or whatever other similar situation applies). In either case, though, I think it's just a matter of what level of variables each of us typically monitors in such situations, so continuing the Program)
/
RS--> Sequence level
(pull out of memory the last Seq.-RS stored)
As you say, if you lucked out the first time you seem to make the same move agai and never get the Program enlarged by some decision-making components, but if yo didn't luck out (and you're like me) you repeat the previous movement and THEN

get hung up and incur the conscious decision-making stuff.

I think Guthrie would have been an early CSG convert if CT had been around at th
time he was.

This doesn't seem to me to apply directly to Rick's report that you quoted,
because in his case the subject is reorganizing at the point referred to. In th
case of your illustration I think reorganization hasn't begun until several more
or less automatic attempts to run through the program have gotten stymied.
Dick Robertson Dept of psychology Northeastern Il U
5712 Harper Ave. Chicago, IL 60637 (312) 643 8686 uprober@bogecnve
==========
Date: Fri, 17 May 91 09:57:50 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPROBER@BOGECNVE.BITNET
Subject: clinical symposium II

[From Dick Robertson]
I'm hoping all the clinicians in CSG will get together for another symposium
at this years conference on using CT in our clinical work. You don't have to h
have a formal paper, but I would be glad to get one, or at least your title, to
put it in some order. We shuould have a lot to follow up with after the dis-
cussion here on the net a month or two back. I've already asked Tom to give
us slot on the program.
Dick Robertson Dept of psychology Northeastern Il U
5712 Harper Ave. Chicago, IL 60637 (312) 643 8686 uprober@bogecnve
==========
Date: Fri, 17 May 91 10:31:49 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPROBER@BOGECNVE.BITNET
Subject: m TAYLOR'S QUEST. ABOUT LEARNING ERRORS & ct

To: Martin Taylor Re - Error Production, Disciplined Imagination
[From Dick Robertson]
You said >There is a fairly common real-life parallel to this effect that has
always interested me...< in reference to Rick Marken's (910509):

>>The closest thing I have seen to what looks like intentional production of
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>>it happening "against your will"....But this happens only because the higher
>>level systems cannot correct things fast enough. It is explained just fine as
>>the behavior of a two level negative feedback control system that is trying
>>(as always) to minimize (technical) error.

>...I tried to get the people in this human-factors institute
>to explain it to me when I was a student here 30+ years ago, and I still
>haven't found anyone with an answer. Maybe CSG people have an answer.
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>For a long time thereafter, even if they use the doorway several times
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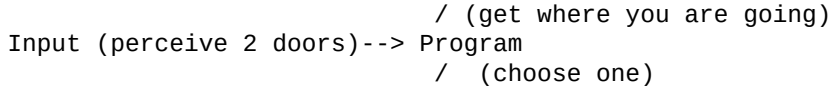
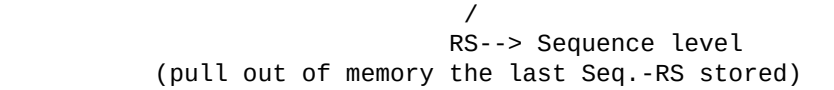
instructions (and the subject doesn't innovate) s/he never makes any move or per
illustrations by which I've been persuaded that old Guthrie was right: we
construct memory (of the skill we want to acquire) out of the perceptual signals
we have to produce in order to satisfy our references.

You might see how we could begin to forge an answer to your question about
what CT might have to say about this as follows (this is a first approximation):
Goal (RS)



(Now here we come to an interesting situation; you described an intricate set of
cognitions that you go through - I, on the other hand often find that I have no
conscious awareness at all until the moment the BANG occurs, I have been thinkin
of whatever I am doing and my actions would seem to imply an assumption that my
body "knows" which door to head for (or whatever other similar situation
applies). In either case, though, I think it's just a matter of what level of
variables each of us typically monitors in such situations, so continuing the
Program)



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and never get the Program enlarged by some decision-making components, but if yo
didn't luck out (and you're like me) you repeat the previous movement and THEN
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because in his case the subject is reorganizing at the point referred to. In th
case of your illustration I think reorganization hasn't begun until several more
or less automatic attempts to run through the program have gotten stymied.
Dick Robertson Dept of psychology Northeastern Il U
5712 Harper Ave. Chicago, IL 60637 (312) 643 8686 uprober@bogecnve
==========

Date: Fri, 17 May 91 12:44:03 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Consciousness

[From Rick Marken]

Well, it's pretty quiet out there again. Ah well, here are some more
thoughts on something else Bill mentioned in a previous post
[Powers (910515)] in reply to Oded Mahler:

>But you are right about one thing: control theory does not explain
>consciousness. The best we can do is to say that purposive systems of
>this kind sometimes operate in the conscious mode, and sometimes in the
>automatic mode. I do not understand what makes the difference between
>those two modes, and I don't think that anyone does. "Purpose" suggests
>consciousness to most people, but control theory shows how we can explain
>every functional aspect of purpose without invoking consciousness. So
>purpose and consciousness are not the same thing (although they are
>naturally associated when we think of those purposes we are conscious of
>carrying out).

This is a very important point to keep in mind. I think the concepts
of intentionality and consciousness are often conflated in philosophical
and "cognitive science" discussions. The control model shows that these two
phenomena are, indeed, quite distinct. The model can carry out many intentions
simultaneously with no consciousness whatsoever. Although the control model
has no answers to the "what is consciousness?" question, it does make some
rather plausible suggestions about where it might "fit in". In "Behavior:
The control of perception" for example, Bill describes consciousness as
something that operates on the control hierarchy. Consiousness is
itself a control system -- but it is the control hierarchy (rather than exter-
nal reality) that is the object of its control. The consciousness system
perceives what the control hierarchy is perceiving. This aspect of conscious-
[Aness is perception of perception -- what is ordinarily called "awareness".
Awareness can apparently be directed to different levels of the hierarchy
"at will" . How or why this happens, we don't know. Bill calls it "point
of view". My awareness can be directed at sensations (the colors on the
monior) configuations (objects) etc. This point of view seems to be limited;
I can only be aware of certain perceptions at any one time. This, I believe,
is what conventional psychologists refer to as "attention".

The other side of consiousness has to do with actions taken on the control
hierarchy. The consciousness system can apparently inject signals into the
control hierarchy -- probably as reference signals. This causes the hierarchy

to produce an intended perception for no reason other than that "I" (consciousness) wanted to. We experience this as "willing".
For example, I can consciously (but for no higher order reason having to do with my control hierarchy) will my left hand to rise. I am arbitrarily injecting a reference into a transition perception system to produce a temporal change (movement) of a visual configuration (my hand).

I think it is important to keep the distinction between intention and consciousness clear. It is a distinction, I admit, that it easier to make in the model than in the observation of behavior. While we do have ways to show that a behavior involves control (the test for the controlled variable) we do not (that I know of) have good ways to show that a behavior involves consciousness. Robertson and Glines (in Perceptual and Motor Skills,51,55-64, 1985) may have made a start at this since consciousness is likely to be involved in the process fo reorganization. They got some beautiful data that show people moving from one steady state (controlled) solution of a problem to another (better) solution. The periods between steady state periods show evidence of reorganization -- changing control systems. This probably involed some attention (awareness) of different perceptual variables and willfulling trying some new references for some variables. Figuring out a way to put a microscope to the Robertson/Glines reorganization periods might be a step toward taking a behavioral look at consciousness.

Hasta Luego

Rick M.

****

Richard S. Marken USMail: 10459 Holman Ave
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Internet:marken@aerospace.aero.org
213 336-6214 (day)
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==========

Date: Fri, 17 May 91 16:03:01 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Greg Wms. on Copyrights

FROM GREG WILLIAMS, 5-11-91: (via Gary Cziko)

Gary, in reply to Cliff Joslyn's post of 5-3-91, I'm no expert on copyright, but this should give you some idea of the complexities involved. The following shouldn't be taken as legal gospel, but rather as an approximation to a messy bit of bureaucratese. Putting it on the Net for all to see what they're getting into (all the time, whenever they put pen to paper or finger to keyboard) might be useful. At any rate, because the situation is so convoluted, I still want to cover myself by getting explicit permission from Netters before publishing excerpts from their posts in CLOSED LOOP, unless they take the trouble to include a statement in each post to the effect that, being of sound mind and body and all that, they are "deliberately" (see below!) not

claiming copyright and thus are allowing the post to enter the public domain.

PARTS OF THE 1976 COPYRIGHT LAW, PARAPRASED:

Copyright protection is available for any original literary creation, "fixed" (by any method) in a tangible medium from which the work can be communicated directly or indirectly or with the aid of a machine or device.

Rights are automatically acquired by creating such material, without it being registered with the U.S. Copyright Office, and without affixing a copyright notice to the material.

If an copyright infringement occurs and there is no copyright notice (the encircled C or the word "Copyright" or "Copr." and the year of first publication and the name of the copyright owner) on publicly distributed copies of the work, the copyright owner might not be able to avail himself or herself of all remedies otherwise available, but there is not necessarily a loss of copyright. (Note that "All rights reserved" constitutes a copyright notice in certain Latin American countries.)

If a copyright notice has been omitted on "a relatively small number" of copies distributed to the public, protection might not be invalidated. Protection will not be immediately forfeited even if more than "a relatively small number" of copies have been distributed, provided that official registration of the work is made before or within five years following publication without notice, and that a "reasonable effort" is made to add the notice to copies distributed to the public after the omission is discovered.

If the would-be copyright owner "deliberately" [sounds like a great thing to determine in a court of law! "Prove your reference signals on November 16, 1987" and so forth...] fails to put a notice on publicly distributed copies of a work, the work passes into the public domain.

==========

Note from Gary Cziko:

If you wish to be included in Closed Loop, fill out the following form and send to Greg:

==========

TO GREG WILLIAMS:

YOU HAVE MY PERMISSION TO USE EXCERPTS FROM MY POSTS ON CSGNET IN "CLOSED LOOP." I RETAIN ALL COPYRIGHTS TO MY POSTS, AND YOU WILL INDICATE THAT FACT BY INCLUDING A LEGAL COPYRIGHT NOTICE IN "CLOSED LOOP" FOR EACH EXCERPT FROM MY POSTS. I MAY CANCEL PERMISSION (NON-RETROACTIVELY) WITH REGARD TO ANY PORTION OF MY POSTS BY GIVING YOU SIX WEEKS' NOTICE.

SIGNED _____
DATE _____
NAME _____
ADDRESS _____

Send to: Greg Williams, Route 1, Box 302, Gravel Switch, KY 40328 USA

==========

Date: Fri, 17 May 91 19:13:13 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: psy_delprato@EMUNIX.EMICH.EDU
Subject: Label Problems, Behavior, Perception

[FROM Dennis Delprato]
1. "Behavior controls perception."
2. "Behavior _is_ the control of perception."

1 seems to separate behavioral events from perceptual events.
2 seems to say that behavioral events are perceptual events and perceptual events are behavioral events. That is, behavior = perception.

Could 2 be more in accord with the model? Apart from my attempt here to keep verbal descriptions consistent with the mathematical ones, I am also thinking that behaviorism does not own the concept of behavior. By this I mean that when we treat behavior as separate from perceptual events, we seem to be treating behavior in the old sense of muscular activity and glandular secretions. Alternatively, one may take the entire loop as behavior. This could take one to "behavioral control system."

Dennis Delprato
Department of Psychology
Eastern Michigan University
Ypsilanti, MI 48197
Psy_Delprato@emunix.emich.edu

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Date: Fri, 17 May 91 21:21:39 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: miscellaneous comments

[From Bill Powers]

Chuck Tucker, Rick Marken (direct)

Yes, Chuck and Rick, my address works. Thanks for good wishes. Mary is still fretting in the hospital -- may be there 2-3 more days. I'm not sending direct replies yet because it's clumsy to do.

Rick Marken (910517) --

I like your way of getting the subject of consciousness into some semblance of order. About all we can do with it right now is to note the phenomena as they relate to the behavioral model, and wait for a bright idea concerning theory to occur to someone. At the moment, consciousness looks pretty magical to me. Where the heck does it live?

Greg Williams (910517) --

Money is a great source of conflict. I'd like to reserve any income-producing use of my work, but the reason is in order not to have to worry about money. Paradoxes are conflicts, aren't they? Although not all

conflicts are paradoxes. Anyway, I hereby deliberately declare that all my works on this net are copyrighted, so feel free to use them with due credit and for any purpose you please except making money. I get the money. Please send list of bidders soonest.

Dennis Delprato (910517) --

>1. "Behavior controls perception."
>2. "Behavior _is_ the control of perception."
>
>1 seems to separate behavioral events from perceptual events.
>2 seems to say that behavioral events are perceptual events and
> perceptual events are behavioral events. That is, behavior =
> perception.

>Could 2 be more in accord with the model?

As you may have noticed, whenever I mean to be talking about the outputs coming from an organism into its environment, I try to use a word like "action" and avoid "behavior." The reason is the very question you raise. There are two difficulties here: one is whether you mean behavior as viewed by the behaving organism or by someone else, and the other has to do with hierarchical control. Let's take up the first difficulty.

When I look at your behavior, I don't see the perceptions that are being controlled because they are inside you. I just see an organism in an environment, from MY point of view. The behavior of drinking a glass of water, from the external point of view, leaves out what you, the drinker, experience: the side of the glass with Yogi Bear on it, the weight of the glass, the feel of the glass on your lips, the sensations of flowing water in the mouth and swallowing efforts and noises, and the foreshortening of the image of the glass right under your nose. Similarly, the experimenter looking down on a rat negotiating a maze is certainly not experiencing the maze as the rat does; the teacher watching a student answering test questions (and knowing what they are supposed to mean) does not experience the answering process as the student does.

When we see the behavior of another person, we're definitely NOT seeing controlled perceptions, are we? We're seeing movements and postures and hearing noises and seeing facial expressions, some of which may be intended effects of the other person's motor actions and some of which may be irrelevant side effects -- and none of which we see from the right point of view or in the right context. We can't know which is which until we figure out what the other person is perceiving and controlling, from the other person's point of view, in relation to other perceptions and intentions of the other person.

I would conclude, therefore, that (1) above applies when we are looking at another person behaving; in that case we hypothesize that some aspect of the behavior we see is controlling a perception or set of perceptions in the other person, which we can't observe directly. It follows that (2) applies when we observe OUR OWN behavior, for all we can know of our own behavior is its perceptual representation.

That's not quite the end of the story, because whether we call a given perception of our own actions a "behavior" or a "controlled consequence of behavior" depends on the level in our own hierarchy from which we view

it.

Dusting off our trusty car-driver example: suppose it occurs to you while you're driving to notice how you make the steering wheel move. At that point you notice the muscle efforts, and you can see that in order to make the wheel move (a consequence of behavior) you have to vary the feeling of effort (the perceived behavior itself).

If you go up a level or two, you may attend instead to how you maneuver the car on the road. Then you see that in order to achieve a particular perceived position of the car in its lane (a consequence of behavior), you have to move the steering wheel (the perceived behavior itself). Now the variable that was formerly a controlled consequence of behavior (the steering wheel movement) becomes redefined as your own behavior (because it is controlled and obeys your wishes), and a consequence of that consequence is now the controlled consequence, if you follow me.

We can go another step: now you notice that in order to drive around another car (a relationship-consequence of behavior) while passing, you have to alter the controlled lateral position of the car (the perceived behavior itself). Once again, the controlled perception at one level becomes the behavior from the viewpoint of the next level up.

Maybe somebody would be amused by carrying this level-raising a few steps more.

From the point of view of the actor, therefore, behavior = perception, but we classify some perceptions as being our means of acting, while we classify others as controlled consequences of those means. In speaking of ourselves, we tend, I think, to use the term "behavior" to mean WHAT WE ARE "DOING" IN ORDER TO ACCOMPLISH SOMETHING ELSE, where "doing" = "controlling without thinking about it." We take for granted that the next system down will make its perception match the reference level we are willing; as the perception obediently changes, we experience that as our own willed action. At the same time, through the external loop, the change in the "action" perception entails effects on perceptions at the level from which we are viewing: that effect is seen as the consequence of the action, and is experienced explicitly as being under control. Actually, there are perceptions under control at many levels at once, because there are disturbances peculiar to each level that must be counteracted and anyway I don't think that nature ever does anything open-loop in an organism if there is a way to close the loop. If this principle had been applied to the Three-Mile-Island reactor-control design, that water valve would have been a servo, and when it stuck the abnormal error signal would have been sensed to set off an alarm that yelled over the intercom, "Reorganize! Reorganize!"

Too many subjects trying to get into this conversation, so I'll quit here.

This is how it feels to be a hierarchical control system.

Dennis, I hope that you can see why I used a semicolon in the title of my book instead of "is" and instead of a simple declarative sentence.

Best --- Bill Powers

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Date: Sat, 18 May 91 12:24:13 cdt
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "McClelland,Kent" <MCCLEL@GRIN1.BITNET>
Subject: Hunter and Prey

[From Kent McClelland]

Tom Bourbon sent me a copy of a draft of a paper he has been working on with his student Eric Chong, which contains quite an interesting demonstration of two control systems engaged in a cooperative task. Although the paper begins with a vivid description of a hunting scene in which a hunting party (in this case hawks) surround and pounce on their prey (in this case a rabbit), the cooperative task actually modeled in the paper is a somewhat less exciting one: two people working together to line up three cursors on a computer screen. After reading this, I thought back on Clark McPhail's demonstration of the nifty "crowd behavior" simulations he and Chuck Tucker have been developing, based on programs written, I believe, by Bill Powers, and wondered whether it would be possible to produce a demo in the style of the crowd-behavior series which simulates the hunter-and-prey situation.

Something like this might even have commercial applications as a computer game. The PAC-MAN games follow a similar principle, and the successful board game, SCOTLAND YARD, in which five detectives move around the streets of London to close in on and capture a fugitive, shows that the hunter-and-prey format has some market appeal. A real-time cooperative chase game with good graphics would be far more exciting, and the verisimilitude of the control-system algorithm should make it a winner. (By the way, does anyone know what algorithm the PAC-MAN type games use?) A commercial game might allow for the player to take the role of either hunter or prey, might allow two or more players to work together as a hunting team, and might supply extra control-system hawks or rabbits (at various levels of speed and agility) to complete the scenario. All in all, the hunter-prey game strikes me as having the macho appeal necessary for considerable market success.

In this capitalistic society, what better way to demonstrate an idea works than by making money from it?

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Date: Sun, 19 May 91 09:50:04 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Clark

[From Bill Powers]

My formal exposure to psychology (including a year of graduate school) took place at Northwestern University in a Hull/Spence environment. This environment caused me to acquire Clark Hull's book, *Principles of behavior* (New York: Appleton-Century-Crofts, 1943). It showed up in a box this morning, and this is what I found in it on page 26.

[I use *...* to designate italics. I'm also not unpacking very fast.]

"Perhaps the very natural and economical mode of communication whereby we speak of the terminal or goal phases of action, largely regardless of the antecedent movements involved, predisposes us to a belief in *teleology*. In its extreme form teleology is the name of the belief that the *terminal* stage of certain environmental-organismic interaction cycles somehow is at the same time one of the *antecedent* determining conditions which bring the behavior cycle about. This approach, in the case of a purposive behavior situation not hitherto known to the theorist, involves a kind of logical circularity: to deduce the outcome of any behavioral situation in the sense of the deductive predictions here under consideration, it is necessary to know all the relevant antecedent conditions, but these cannot be determined until the behavioral outcome has been deduced. In effect this means that the task of deduction cannot begin until after it is completed! Naturally this leaves the theorist completely helpless. It is not surprising that the doctrine of teleology leads to a theoretical despair and to such pseudo-remedies as vitalism and *emergentism*.

"Emergentism, as applied to organismic behavior, is the name for the view that in the process of evolution there has 'emerged' a form of behavior which is ultimately unanalyzable into logically more primitive elements -- behavior which cannot possibly be deduced from any logically prior principles whatever. In particular it is held that what is called goal or purposive behavior is of such a nature, that it cannot be derived from any conceivable set of postulates involving mere stimuli and mere movement [and here he cites Tolman, *Purposive behavior in animals and man*, 1932]."

This book appeared in the same year that Wiener, Rosenbleuth, and Bigelow published their debate with [Taylor?] on teleological systems [I haven't unearthed Buckley yet]. It is almost certain that Hull knew of these debates or at least of the widespread discussions of teleological systems that were going on at the time. So I suspect that the passages above were a response to this growing Zeitgeist (else why include the subject?). I have seen this sort of averted-vision dismissal of an opposing view many times, in connection with control theory. Instead of presenting the position that is to be criticized as accurately as possible and then showing where and how it fails (or accepting it), the author knocks down a pseudo-presentation so constructed as to sound patently false.

The first paragraph contains this: "... the *terminal* stage of certain environmental-organismic interaction cycles somehow is at the same time one of the *antecedent* determining conditions which bring the behavior cycle about." Note how attention is focussed entirely on the "cycle," as if the explanation of teleology, if it exists, is to be found somewhere in a circular chain of events considered one at a time. Note, too, how the time dimension comes in: "terminal" and "antecedent" conditions. The idea that a CONTINUOUS specification of the terminal condition EXISTS INSIDE THE ORGANISM never even occurs to Hull (or to any of his contemporaries who used essentially this same argument). The variables in the circle of causation have to provide the purpose themselves, which of course is impossible. Knowing what we know, we see the missing concepts standing out unmistakeably: without the idea of

a comparator and a reference signal, one simply can't comprehend what is going on in purposive behavior.

In the passage on emergentism above, note how Hull converts "cannot be derived from any conceivable set of postulates involving mere stimuli and mere movement" into a generalization, given first in order to make the specific "cannot" into an instance of the more general "cannot possibly be deduced from any logically prior principles whatever." This is a way of asserting that the only possible prior principles are those under which movement depends on stimuli. It is also a statement that there can't possibly be any other valid principles that Clark Hull doesn't know about. In 1943, that would have been true only if Hull were ignorant of the rise of control theory, an excuse I would doubt. As many others have done, Hull was writing about control phenomena defensively: it was inconceivable that his profession could have so completely failed to understand a phenomenon of such central importance.

When one of us writes the story of the rise of control theory, one of the main threads has to be the way in which psychologists (and others) kept encountering the problem of purposiveness and dismissing it on spurious grounds. Knowing that purposive systems actually exist and behave in exactly the way deemed impossible, we can easily find the spurious assumptions and missing knowledge that led to the rejection of teleology. But we can also easily see that the arguments don't hold water on their own merits. There is simply no justification for Hull's assertion that if the principle of stimulus and response doesn't hold, no principle whatever could hold. Such an assertion is blatantly self-serving.

I think that the failure of life scientists in general to solve the problem of purpose has led to a distortion of their thought processes. All the proposed debunkings of purposiveness have been based on weak, sloppy, and emotional arguments that are not even logically self-consistent, arguments of a kind that would not be tolerated for a moment if they were employed within a scientific presentation rather than as a defense of mainstream beliefs against outside pressures. It seems that when "everybody knows" the truth, arguments against heresies do not have to be constructed with care, because nobody is going to examine critically the construction of a rebuttal of an idea that everyone knows is wrong, stupid, superstitious, and so on. This toleration of careless reasoning has inevitably crept into mainstream scientific arguments as well.

But a scientist can't afford to indulge in offhanded defenses of central beliefs. When a belief becomes so important that even bad logic and sloppy arguments are acceptable in defending it, the scientist putting up the defense has given up his only claim to integrity. As a result, he or she is going to be less of a scientist.

The true spirit of science is maintained only when alternative proposals are accepted rather than rejected: OK, let's try to make that idea work. This is what is meant by judging ideas on their merits rather than in terms of their implications. A psychologist who says "But if control theory is correct, then I've been wasting my entire career blundering down a blind alley" is merely stating a truth.

Such comments, however, do not normally stop there: they go on:
" ... so control theory can't possibly be correct." The implications
of control theory become a reason for rejecting it even if it is
correct.

If one has wasted a career on a wrong idea, that is no reason to waste
the rest of it the same way. To think that way, however, one must have a
particular system concept of what a scientist is and what science itself
is, as an uplifting endeavor. One must believe that it is more important
to know the truth than to be right. This is a little like giving one's
Self over to a Higher Power: the rewards of maintaining the integrity of
science far outweigh the embarrassment of being wrong. One can then
become willing to explore a new idea without letting its implications
influence the judgement as to whether the ideas explain the observations.
One does the Right Thing and copes with the consequences. That's about as
close as I get to a religion.
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Date: Sun, 19 May 91 22:09:26 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Driesch and Hobhouse

[from Gary Cziko]

Bill Powers (910519):

Bill, I don't suppose you have any old books by Driesch or Hobhouse to
unpack. If so, they might cheer you up a bit afer your second encounter
with Hull.

I've been reading a book (Boakes, Robert. (1984). _From Darwin to
behaviourism: Psychology and the minds of animals_. Cambridge: Cambridge
University Press) which has turned up some interesting stuff.

In chapter 7 "Apes, problem-solving and purpose," Boakes cites the work of
German biologist Driesch and British psychologist Hobhouse.

First, on Driesch (1904):

"Another extension, which is particularly important here, concerned the way
that the actions of living creatures display the characteristics of
'equifinality' and 'self-regulation'. Driesch gave the example off a dog
making its way to a certain place. In one case the dog might be heading
there in a direct line. when a carriage crosses the line, causing him to
run more quickly and make a curve in order to avoid the carriage. In other
case one leg is injured so that the dog has to use three legs to get to his
goal. In both cases the final end-point is reached, even though either a
different route or a different set of movements from the normal ones has
been employed so as to adjust to disturbances in an appropriate way.^11"
(p. 178)

My goodness; Driesch even uses the word disturbance here! The three-legged
example makes me wonder how the behaviorists can even begin to make sense
of such phenomena. Weren't some studies done once with rats who had
learned a maze and then somehow were deprived the use of their legs.? I
have this image of rats rolling through mazes in my head. Maybe it was

just a bad dream. For some reason I feel that Dennis Delprato should know about this.

Now let's look at Driesch's (1894) idea of perception:

"One of Driesch's main preoccupations was with the level at which it is appropriate to describe and analyze some biological phenomenon; a painting such as '_The Madonna of the Chair_, examined with a lens at a distance of 1 cm shows up quite differently than at 5 m away. The first time we see only blotches. Is then the study of blotches really the only task of the biologists?'^12 In psychology the problem of levels first came to prominence in the context illustrated by this example; by the turn of the century many psychologists began to question the value of trying to understand human perception by means of experiments on 'blotches'. The kind of terminology Driesch used in the context of embryology--terms like 'structure', 'organization' and phrases like 'form is not a mere sum of certain elements'--was introduced into the study of perception" (p. 178).

Hm, levels of perception. An interesting idea!

Unfortunately, Driesch was a bit of a weirdo whose vitalis
and interests in Lamarckian inheritance and telepathy cost him the respect of other scientists. But then maybe he was just ahead of his time in these other quirks as well!

Now we turn to Boake's discussion of Hobhouse's 1901 book, _MInd in evolution_.

"Hobhouse appears to have been very familiar with Driesch's ideas and, like Driesch, he believed that an analysis of behaviour must begin by considering a living creature as an _organized_ self-regulating system. 'The normal life of any organism from highest to lowest is a process of unceasing change. It involves a constant interchange of substance with the outer world, and equally constant metabolism or transformation within itself of the substance which it takes up from without and a no less constant transformation of energy. Throughout this unceasing process which differentiates it from inanimate matter, the organism preserves its own identity as clearly as the unchanging rock.'^3 What an animal does is to be seen as part of a general system serving to preserve its identity; the behaviour of an animal is not simply a set of independent reflexes or stimulus-response units, as Thorndike proposed; what is of crucial importance is how various forms of behaviour are integrated ,organized or, to use the term Hobhouse favoured, 'correlated'."" (p. 180).

The idea of unceasing change and activity in order to maintain a constant internal identity sounds kind of familiar around here. Boakes later remarks about Hobhouse:

"Some of Hobhouse's remarks suggest that he would have been pleased by the later application of control theories developed for man-made self-regulating systems" (p. 203).

So perhaps part of the problem with psychology is that people had control theory ideas before they had an explicit model to plug it into. Then by the 1930s it was too late; psychology had already committed itself to the lineal causation model so successful in physics.

So when one of us (why shouldn't it be Bill?) writes that story of the rise of control theory, I hope he or she doesn't overlook these early control theorists.--Gary

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Date: Mon, 20 May 91 08:31:47 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: History of Control

[From Rick Marken]

Gary Cziko (910519) writes:

>My goodness; Driesch even uses the word disturbance here! The three-legged
>example makes me wonder how the behaviorists can even begin to make sense
>of such phenomena. Weren't some studies done once with rats who had
>learned a maze and then somehow were deprived the use of their legs.? I
>have this image of rats rolling through mazes in my head. Maybe it was
>just a bad dream. For some reason I feel that Dennis Delprato should know
>about this.

I would imagine any psychologist would know this -- you are thinking of studies done by Edward Chase Tolman, professor of psychology at Berzerkley (know as Cal at the time -- 1930s), author of a book called "Puposive behavior in animals and man" and, I believe, Clark Hull's main adversary. The psych building at Berkeley is named after Tolman. The experiment you are thinking of is one where Tolman trained rats to run a maze to a goal. Once the maze was learned, Tolman filled it with water so that the rats now had to "swim" the same path that they had learned to run. All the rats who learned to run the maze successfully swam it successfully also -- the first time. The point, of course, is that organisms will use variable means to achieve consistent ends. The rats were not learning behaviors (muscular acts, I believe they were called) but how to produce an end (the food at the end of the maze). They were learning to control a result (getting the goal food) -- and they would vary their actions, as necessary, to produce the intended result. Tolman had no explanation for how this worked (well he did but it had no explanatory power -- he came up with ideas like cognitive maps and "sign stimuli" and whatever).

Tolman, I think, understood the phenomenon that we call control (he, of course, called it purposive behavior) but he had no explanation for it. I don't know the date of Tolman's book but I bet it was about the time of Hull's book. If, as Bill suggests, psychologists had any inkling that control theory had anything to do with purpose, then even those who understood the purposiveness of behavior, like Tolman, had no idea that control theory could help them. Anyway, control theorists should pay some homage to Tolman,

I think. Although he had no idea of how modeling worked and had no model for what he was seeing, he did carry out many demonstrations of the purposeful nature of behavior. I know that behaviorists like Hull tried to deal with Tolman's findings -- apparently the majority of psychologists imagined that a stimulus-response explanation would work since the S-R paradigm certainly prevailed. Since none of the models at the time were much more than verbalisms (there's that word again) it was probably difficult to show that these S-R explanations would not work. So Tolman was probably just out-argued by the folks who said things compatible with the current zeitgeist.

C'est la vie.

Regards

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Richard S. Marken USMail: 10459 Holman Ave
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Date: Mon, 20 May 91 14:15:27 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Higher Power

[From Rick Marken]

Bill Powers (910519) says:

>But a scientist can't afford to indulge in offhanded defenses of
>central beliefs. When a belief becomes so important that even bad
>logic and sloppy arguments are acceptable in defending it, the
>scientist putting up the defense has given up his only claim to
>integrity. As a result, he or she is going to be less of a scientist.

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>proposals are accepted rather than rejected: OK, let's try to
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>entire career blundering down a blind alley" is merely stating a truth.
>Such comments, however, do not normally stop there: they go on:
>" ... so control theory can't possibly be correct." The implications
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>correct.

>If one has wasted a career on a wrong idea, that is no reason to waste
>the rest of it the same way. To think that way, however, one must have a
>particular system concept of what a scientist is and what science itself
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>to know the truth than to be right. This is a little like giving one's
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>science far outweigh the embarrassment of being wrong. One can then

>become willing to explore a new idea without letting its implications
>influence the judgement as to whether the ideas explain the observations.
>One does the Right Thing and copes with the consequences. That's about as
>close as I get to a religion.

Bill, you make it hard for me to resist letting off some steam when you write prose like that. For reasons that I cannot understand, I count myself as one with a belief that makes it far more satisfying to know the truth than to be right. I think the above paragraphs suggest why I tend to distrust and fear control systems that prefer being right to being truthful (or, since we rarely, if ever, get the latter, admitting that their "rightness" is tentative). It seems to me there have been, are, and will certainly continue to be control systems that want only to be recognized as having the right idea -- an idea that we would probably call a system concept. The methods of showing that these system concepts are right have too often included violence.

I argue that there is only one system concept I know of that has,explicitly, included, as one of its working principles, the principle that Bill articulated above -- that it is more important to know the truth than to be right. I think this principle implies a willingness to subject one's beliefs to the test to observation, logic and reasoning -- ie. FALSIFIABILITY. Scientists who act as though this principle is not part of their system concept are no longer -- from my point of view -- scientists (even if they say they are and they do a lot of math and a lot of experiments). They are just ideologues -- religious fanatics like the rest.
I don't think any ideology (religion) other than science contains this principle of "truth over right" as part its system concepts. The very essence of religion is revelation -- "I know what's true no matter what logic or my experience says". What could be more dangerous. When I meet a religious person (or the exponent of any other ideology -- ie a system concept that does not include falsifiability as a central tenet) who says -- gee, I might be right but I'm willing to change based on the evidence -- then I'll be greatly impressed. I might even join the religion.

Regards

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Date: Mon, 20 May 91 12:58:19 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: CSG Meeting:Airline;Participants

From Tom Bourbon --
UNITED AIRLINES has agreed to offer us a group discount for travel to the meeting in Durango. The discount will be 5% off of

any fare already discounted for other reasons and 40% off of unrestricted coach fares. United will allow a four-day window before and after the meeting. Some people might combine the discounted fares with the offer from the college that allows us to arrive as early as 10 August. The result would be a few days in Durango at reduced costs, during the peak of the expensive tourist season.

Inited will give me the final details this week. I will post them on CSG-L and will include them in the written call.

WRITTEN CONFIRMATIONS: Written confirmations for those who need them will be mailed soon -- as soon as I finish grading final exams.

PARTICIPANTS: To the list of participants I posted last week, add Gary Cziko and Kent McClelland. I just learned that Frans Plooij will not attend. My colleague from the medical school in Galveston, Texas, Andy Papanicolaou, will attend. Andy is not on the net and is unknown to most of you, if not all of you. He is an experimental psychologist, psychophysiologist, neuroscientist and iconoclast. His deepest interest always was intentional behavior, now that interest is wedded to an interest in control theory. Andy has been doing interesting work on a CST model of classical conditioning.

Best wishes,

Tom Bourbon <TBourbon@SFAustin.BitNet>
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Stephen F. Austin State Univ.
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Date: Mon, 20 May 91 22:37:47 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Tolman

[from Gary Cziko]

Rick Marken (910520) chides insensitively:

>I would imagine any psychologist would know this -- you are thinking of
>studies done by Edward Chase Tolman, professor of psychology at Berzerkley
>(know as Cal at the time -- 1930s), author of a book called "Puposive
>behavior in animals and man" and, I believe, Clark Hull's main adversary.

I suppose I did know it at one time, but I haven't spent too much time reading about rats over the last 12 years of so (I'm supposed to be an EDUCATIONAL psychologist). It may hurt a bit to admit ignorance, but I've found out that it's worth it in the long run--you learn more that way.

>Tolman trained rats to run a maze to a goal. Once
>the maze was learned, Tolman filled it with water so that the rats now had
>to
>"swim" the same path that they had learned to run.

Ha! I may have you here. In the Boakes book I am reading, it says that Tolman's rats first learned to swim the maze, then had to wade through it. Maybe you're thinking of another experiment. But I still have this image

of rats ROLLING through mazes in my head.--Gary

P.S. to everyone except Rick Marken: Watch how apologetic Rick becomes when he thinks he's hurt someone's feelings. He really IS a nice guy after all.

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Date: Tue, 21 May 91 09:54:30 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: Higher Power

[from Joel Judd]

Rick recently rote (910520):

>For reasons that I cannot understand, I count myself
>as one with a belief that makes it far more satisfying to
>know the truth than to be right. I think the above paragraphs suggest why
I
>tend to distrust and fear control systems that prefer being right to being
>truthful (or, since we rarely, if ever, get the latter, admitting that
their
>"rightness" is tentative). It seems to me there have been, are, and will
>certainly continue to be control systems that want only to be recognized
as
>having the right idea -- an idea that we would probably call a
>system concept. The methods of showing that these system concepts are
>right have too often included violence.

This topic doesn't seem to go away, so I'll add a few comments. I would argue that most serious religionists, or at ones I admire, would argue that the search for meaning, God, etc. is the search to be both true AND right. Again, I don't see the mismanagement and abuse of religion as negating any possibility that there is Truth and Rightness together somewhere. The problem, or paradox, is that I don't believe inquiring minds want to know; rather, there has always been the desire to PROVE God, etc. "scientifically," and I don't see that happening in the near future. That is why scientists argue (against) "religion" using the following type of statement:

>I think this principle implies a willingness to subject one's beliefs
>to the test to observation, logic and reasoning -- ie. FALSIFIABILITY.
>Scientists who act as though this principle is not part of their system
>concept are no longer -- from my point of view -- scientists (even
>if they say they are and they do a lot of math and a lot of ex-
>periments). They are just ideologues -- religious fanatics like the rest.
>I don't think any ideology (religion) other than science contains this
>principle of "truth over right" as part its system

>concepts. The very essence of religion is revelation -- "I know what's
>true no matter what logic or my experience says". What could be more
>dangerous.

Or what could be more sublime? I find it interesting that you use the word "revelation," because in my beliefs that happens to be a key concept. It refers to the idea that God communicates with man (which of course assumes there exists God, etc.). No, it's not amenable to logic, but yes, I do believe experience can bear out one's perceptions of "revelation," if you mean the same thing by experience that I do. Revelation to me might just be "luck," "good fortune," or a "timely decision" to you. There is no way I can "prove" to you it is right, or true.

One last thread which has run unexpressed through most of the "religion" polemic concerns the idea of "selflessness," for lack of a better word. Most major religions include some form of the doctrine that a human being reaches greater heights by thinking less of himself and more of others. In christianity the paradox was expressed by Christ when He spoke of "finding" your life by "losing" it, explaining that serving others was somehow more divine than serving yourself. Included in this self-subjugation was obedience to God with the understanding that He has had more "experience" and is in a position to suggest how we might make the most of being human. I would bet that a lot of the people we admire fall into this characterization, whether or not they believed in a higher power. It's great to recognize your potential as a fully-functioning control system, but I think it's even greater to reign in all that power and place it in the service of others and help them reach their potential. While I'll never be able to "prove" that, that's the interface between science and religion for me.

Joel Judd

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Date: Tue, 21 May 91 07:56:05 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Mindreading, Tolman, Life's Tragedies

Gary -- I am working on a general update of the mindreading programs. I agree, the numbers don't get big enough when they are detected as being controlled. I'll fix that up for sure. I'm glad you like what I call the "find mind" program. It is kind of cute, I must admit.

You're right -- I do apologize for chiding you insensitively about the Tolman experiment. But I am properly chided myself for recalling the experiment incorrectly -- I'm sure the version in the Boakes book is correct. I do humbly apologize (for all to see).

Now for a quick "Life's little tragedies" story. Yesterday, I got a call from a professor at UCSB (my graduate alma mater) who liked my "Degrees of freedom" paper in Psychological Science. The tragedy is that he loved the paper for the wrong reason -- he loved the high correlatoins between human and model behavior (on the order of .95 -- which is actually fairly low by our current standards). So he wanted the data for some high powered statistician to look at. Of course, I will send it. But it's kind of depressing to me that the whole point of the article was really missed -- and all that was tuned into was the least interesting from my perspective. Ah well. I don't know what kind of

statistical evaluation is planned but I doubt that they will see the forest for the trees. I think that Tom Bourbon should also consider sending these folks his data. If they want impressive correlations I know that Tom has them. I've got to rush off but, if you are interested Tom, post me a personal note and I'll let you know what I know about this.

Regards

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Date: Tue, 21 May 91 11:32:18 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Epistemology and Religion

FROM CHUCK TUCKER 910521

ON RELIGION

I did not want to get into a discussion of religion, religions or religious beliefs per se but these topics seem to return to our conversations. My point was that PCT or CT as I understand it has an ethical principle that is on the same level as religions, theories, ideologies or meta-meta-instructions. The principle is: respect each human being as a self-regulating control system. I also tried to make the point that most of those who hold to some religious doctrines that I know of do not use this priciple and it is that occasions much conflict, anger, dispair and other disturbances even more profound.

ON EPISTEMOLOGY

This topic returns again. Yes, you were correct Rick, I did take your remarks at the last CSG meeting as a rejection of the notion of perception as used in CT but now I see your point. I have struggled with the problem of trying to convince students for years that they are self-regulating organisms but they simply refuse to take any responsibility for their own construction of "the world" {actually I refer to "it" as "experience" (primitive undefined term)}. I have even come to the point of telling them that they are responsible for their own learning; if they fail to learn it is their responsibility - I just keep score! They still blame me and others for their lack of information. It is a serious problem and I now think that one way to do it is to find people who are so disturbed by the neorealistic assumptions that they can not longer function and offer them another set (of course, one can set out to disturb another and then be prepared to offer them another view but we wouldn't even do anything so cruel). In this regard I recalled the other day what I wrote in an obituary of one of my students and colleagues about the "reasons" for having such difficulty in moving people from the neorealistic epitemology. I wrote:

> "The combined tenets of the Enlightenment and the "quest for certainty" along with an interest in being "bookkeepers

> of facts" are overwhelming. When this rhetoric is mixed with a consumed indifference for practical social life and the devotion to simple minded psychological "theories", a doctrine is constituted that nothing less than a revolution will change."

Control Theory along with the ideas of Mead, Dewey, James, Pierce, Bentley, and a few others (excluding present company) can serve as the basis of that revolution. That is what I think we are up to folks!

SOCICYBERNETICS

On a separate post I am sending some statements the Bob Stewart developed and that I use as often as I can in my courses which indicate the epistemology that we use. I think that it is consistent with CT but at the minimum I do not find it contrary to its epistemological assumptions. Use them if you care to.

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Date: Tue, 21 May 91 12:10:51 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: EPISTEMOLOGY AGAIN !

STATEMENTS FROM A SOCIOCYBERNETIC PERSPECTIVE OF HUMAN CONDUCT*

Society, social structure, social class, culture, or group pressure do <<not>> make people do anything.

People are responsible for their action, but <<not>> for everything that happens to them.

Personality, socialization, and social background do <<not>> make people do anything. (Rather, these provide resources for action, but determine none of it.)

Scientific facts and theories do <<not>> describe reality.

Social life, by which is meant living and acting together, depends on arrangements people make.

No scientist in any discipline, including physics, has discovered, or can discover, any law or principle of nature, or has or can gain knowledge of reality.

People guide their actions by directions they give themselves.

Problems people have in social life are results of missing, inadequate, or poorly implemented arrangements.

Discovering the laws of social life is <<not>> possible, or even sensible.

Biological agents such as germs or viruses, or chemical agents such as alcohol or cocaine or steriods do <<not>> make people do anything. (Rather these can affect performance levels and the coordination and control of behavior)

We can study arrangements and how they are made, and we can improve upon them, and create new and more useful ones.

Technology does <<not>> make people do anything. (Rather, technology provides resources for action.)

Scientists in all disciplines provide ways of solving problems people are having.

Social norms, rules, values, beliefs, customs, traditions, laws, or social sanctions do <<not>> make people do anything. (Rather, these are devices people use to facilitate living and acting together.)

People can <<not>> be made to do anything, unless they are literally and directly and physically forced to.

Genetic inheritance or any other biological factors do <<not>> make people do anything. (Rather, these permit people to do what they do, and, undoubtly permit them to do much that so far they have not done.)

Without making arrangements people are socially incompetent.

Each person is <<not>> unique, nor have unique ideas, nor have unique perceptions of things.

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*Slight modifications of statements used in the courses of Bob Stewart.

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Date: Tue, 21 May 91 13:31:48 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Higher Power

[from Rick Marken]

Joel Judd (910521) writes, among other beautiful things, this lovely passage that deserves repeating:

>One last thread which has run unexpressed through most of the "religion"
>polemic concerns the idea of "selflessness," for lack of a better word.
>Most major religions include some form of the doctrine that a human being
>reaches greater heights by thinking less of himself and more of others. In
>christianity the paradox was expressed by Christ when He spoke of "finding"
>your life by "losing" it, explaining that serving others was somehow more
>divine than serving yourself. Included in this self-subjugation was
>obedience to God with the understanding that He has had more "experience"
>and is in a position to suggest how we might make the most of being human.
>I would bet that a lot of the people we admire fall into this
>characterization, whether or not they believed in a higher power. It's
>great to recognize your potential as a fully-functioning control system,
>but I think it's even greater to reign in all that power and place it in
>the service of others and help them reach their potential. While I'll never
>be able to "prove" that, that's the interface between science and religion
>for me.

Joel, this was a beutiful post. It made me feel a bit like Scrooge McScientist when I read it. I think my hostility toward some aspects of religion masks my real love of many things that would also be called religious. (In fact, I realize that I keep posting on this topic because I am so drawn to, well, spiritual topics). It's hard for me to have a consistent attitude about a system concept (or set of them) that has brought you everything from witch hunts to nearly everything Bach wrote. As your paragraph shows, there are some beautiful sentiments in the Bible. I am particularly fond of many of the psalms, I love eccelsiastes (by and large) and the stories of the new testement are great. I love the character of Jesus. I love a great deal of western mythology -- greek, norse, etc. I'm not a big fan of the eastern mythologies -- but that is a matter of taste.

The problem with religion -- what spoils it for me -- is what you might call "literalism" or "fundementalism". I kinda think it's what is also called "faith" I'm afraid. It is the part where you have to "worship" something or "believe" that something really happened or that something "really" exists " although there is no evidence of it. There is no faster way to corrupt the sublime, from my point of view, than by making the "rightness" of it mandatory. The problem, I think, comes from the fact that religion (western religion anyway) filled at least three roles, two of which are now handled much better by modern disciplines. One role of religion was explanation of what was observed -- this is what genesis and many mythologies try to do. Now we've got science -- we understand that the wonderful imagination that created the "explanatory" myths is only one halfp of the process of explanaiton -- there must also be the discipline of observation and test. But some people still want the "explanation of phenomena" role for religion -- to give it legitimacy, I suppose. Hence we get creationists, flat earthers and other, basically harmless, crazies.

The other role of religion is to express the unexpressable -- the nature of the human spirit. This is now handled by art -- poetry, music,etc. The bible has some of the best prose and poetry going. It is art -- some of the most inspired art of all time. So the biblical art is a subset of a vast expanse of songs of the human spirit. But it is not special (other than in terms of how well if achieves its artistic goals (of expressing the human spirit). It has no more priveledged place in the art word than Shakespeare or Chaucer (or name your favorite poet). But there are still some who want to maintain that biblical writings are special -- inspired by god. This leads to book burners and banners. These crazies are dangerous and quite unacceptable.

The third role of religion (and there may be more) seems to me to be rather unique to western judeo-christian religion. This is the ethical role. Apparently, at some time, long ago, some Hebrew tribal person realized that there was no obvious reason why he was being a nice person. And if s/he had no reason then nobody else had a good reason so they might just go haywire at any time. So he realized that he needed to tell people that there was a reason why they should continue to be nice to each other -- it's because they have 11th order system concept control systems watching to make sure that they have selected the right references for their principles. S/he just called these 11th references god. Not leaving anything to chance, s/he made sure that everyone knew that if they didn't set their principles appropriately they would suffer an error signal -- eternal damnation in the fires of hell

(catchy new name for an 11th level error signal).

I suppose civil law could be considered a replacment for the written ethical standards (backed by threat of coersion) that had been provided by religion;
but I don't quite think they capture it. I think what Hugh Gibbon is doing in trying to analyse the system concepts that underly the law and our sense of justice is the start at a rational approach to understanding the ethical basis of our behavior. Chuck Tucker suggests that there might be an ethical principle that is part of control theory itself -- but I don't think so. I think control theory can explain why we do (and don't) behave ethically -- but it boasts no ethics of its own.

Because there is no real convincing modern discipline to replace the ethical role of religion (although I do believe that control theory might start to help -- but don't expect anything interesting for a few decades) the crazies in this area of religion have been particularly prevalent and destructive. Nowhere else has religion caused more misery to innocent people than in the ethical bullshit it has imposed based on the "wisdom" in ancient texts. I think the creationists are amusing and the book burners are annoying but the one's bringing "god's rules" are just flat out evil. I have had many homosexual friends whose lives I've seen made miserable and difficult because of the religious prejudice against this practice -- because god says it's wrong. We have a massive overpopulation problem in the world, partly due to the fact that some nut cakes have divined that god doesn't like anything to come between seman and ovum (this one, alone, will probably be sufficient to end any hopes of a civilized society). From what I read, it seems to me that Jesus was the kind of guy who wanted people to find their highest degree of personal human fulfillment. He didn't get made at prostitutes (who hurt no one, save possibly themselves) or fags (again, who hurt no one except, possible themselves) or masturbaters or birth controllers. Not even an adulteress. I think Jesus new the difference between helping people achieve their own personal goals and helping people achieve his goals. I love selfless giving -- but remember, that's SELF LESS. If christians were really christian, they would be out there trying to help homosexuals find the mates they want -- not the mates that the christian wants. By the way, I'm not a homosexual myself -- but I don't bungi jump either. Both are OK with me though (as long as no one is being made to do anything against their will). Of course, these values of mine must be all wrong because they are not written down on an ancient parchment. Ah well.

Anyway, when it comes to religion, I think the aspects of it that really are wonderful can only be kept wonderful if they are brought back into the bosom of art where they belong where they will not corrupted by the ugly drive for "rightness" that taints discussions of ethics.

Hasta Luego

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Date: Tue, 21 May 91 15:57:38 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Why religion?

[From Rick Marken]

I'm having so much fun that I thought I'd try to get a quick post off before I leave work. I just want to suggest another reason (other than the one suggested by Bill Powers a couple weks ago) why the subject of religion is not irrelevent to CSGNet.

Bill noted that discussions about religion, and our reactions to them, constitute hints about the nature of our own system level reference signals. If you can get past the fact that the substance of these beliefs are considered "true" you will notice that they are perceptions that you are trying to defend at particular references. Thus, our arguments, if analyzed properly (I bet Bill could help) are themselves a laboratory for study of control of the higest level perceptions in the control hierarchy -- definitly more interesting than watching control of the position of a cursor on a screen.

The other reason that religion is relevant to CSG, I would suggest, is for the same reason that it is hard to keep it out of discussions of the origin of life. Control theory, like evolutionary theory, is trying to deal with aspects of human existence that were once the sole perview of religion; with evolution it is the origin of people; with control theory it is the nature of the soul. Of course, regular old psychology treads on religious issues too. But control theory really get's to the "soulful" aspects in a particularly deep way. Control theory explains (rather than explaining away) one aspect of people that most deeply defines our human nature -- our purposefulness. Suddenly, teleology is no longer a spiritual mystery but an understandable characteristic of closed loop, negative feedback organizations of matter. Most importantly, religion itself is an understandable part of the control model -- it is a system level purpose; an intention to perceive certain principles, relationships, categories, etc. This doesn't make god or religion go away (just as evolution did not make god and religion go away) but, like evolution, control theory certainly requires a thoughtful reevaluation of this system concept. There is just no getting around it. I can help but feel that, to the extent that control theory is an improved model of human nature, reevaluating one of the most important aspects of human nature in the context of this model cannot help but be for the best.

Hasta Luego

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Date: Tue, 21 May 91 21:15:24 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: The theory of 11th order

[From Bill Powers]

Joel Judd (910521) --

This is the point where in ordinary conversations I would say "Oh, sorry, I didn't mean to tread on your beliefs." This isn't an ordinary conversation. It's a scientific conversation, meaning that the participants are assumed to be more interested in improving their explanations of natural phenomena than in defending them. So when you say

>I find it interesting that you use the word "revelation," because in my >beliefs that happens to be a key concept. It refers to the idea that God >communicates with man (which of course assumes there exists God, etc.).

... I can only take this to be a scientific report. You are reporting a phenomenon (and in conversations of this sort, one main ground rule is that all reports are honest and taken to be honest). The phenomenon is:

> ... experience can bear out one's perceptions of "revelation," if you >mean the same thing by experience that I do. Revelation to me might just >be "luck," "good fortune," or a "timely decision" to you.

The theory I propose to account for the phenomena of revelation, taking it as given that revelations do occur, is that (1) higher-order systems in the brain, operating at a level higher than the normal level that is conscious (whatever that means), can inject reference signals that appear arbitrary and sourceless to the conscious systems; and/or (2) the process of reorganization can alter (at random) the way the conscious systems operate, including the way they perceive, so that sudden new understandings and new methods of acting appear, as if from nowhere. I would argue that there is no reason to think that such changes in the conscious world are due to any factor outside the brain -- i.e., a supernatural being. On the other hand, there is no evidence that such supernatural intervention does not occur; we do not have the ability, now, to tell the difference between supra-conscious processes originating inside the brain and supernatural processes originating outside the brain -- as long as our only evidence is the experienced result.

Now, you go on to say,

>There is no way I can "prove" to you it is right, or true.

... referring, I take it, to the proposition that such revelations originate outside the brain. I agree; I see no way to construct a compelling argument that would persuade any reasonable person of the truth or falsity of your proposition, or of mine. So in terms of scientific knowing, we would have to agree that we do not know which is the correct proposition, if either.

In such cases we have to choose something as a provisional belief, to take the place of knowledge. The question then is which belief to choose, not on grounds that it is "right" (because we do not know which is right), but on whatever practical grounds we can find.

One possibility that we have to entertain is that sudden changes in the conscious world may sometimes be due to normal reorganization or to the action of higher-order systems in the brain, and sometimes may be revelations from a Higher Power outside (or larger than) the brain. If that possibility exists, then we must ask about the consequences of making a mistake: of mistaking a brain process for a revelation from God.

Suppose you suddenly get the thought, crystal-clear and compelling and as if from a higher source, "All of your troubles are being caused by the Jews. You must therefore kill all the Jews, and purify the land." If you are convinced that this thought is a product of your own reorganizing processes, you will evaluate it in terms of all your other concepts and understandings and goals, and quite probably dismiss it as just another of those bright ideas that would not work out very well. But if you decide that this sudden idea is a revelation from God, you have no choice but to obey. The theory of God does not allow for ignoring God's word, or re-evaluating it.

I think we have to accept that thousands upon thousands of people have received sudden thoughts that they attributed to God, and as a result have committed what I at least consider to be unspeakable evils, thinking that they were acting under Divine Orders. In many theologies, the answer to this problem is not to say that such sudden thoughts arose from internal reorganizations and were simply not evaluated appropriately, but that they originated in ANOTHER supernatural power: Satan, the god of evil. The theory of God, in combination with observations that seem to attribute unacceptable characteristics to God, requires introducing the theory of Satan, who is reponsible for the unacceptable "Divine" orders.

The Koran states quite plainly that God commands loyal Muslims to convert the infidels, and if they will not convert, to destroy them as the forces of Satan. I should imagine that there have been many faithful Muslims who have undergone a crisis of the spirit over this teaching: God says you must kill these innocent people, while reason and compassion say that to do so would be evil. The power of faith, however, can overcome mere human reason and feeling. The good Muslim would subjugate his personal thoughts and feelings to the commands of God, and do what the Divine Word says he must do. I'm no expert on the Mulsim faith, but I think that this is not a grossly unrealistic scenario.

In this country, of course, our God (of Christianity or Judaism, to speak only of the majority beliefs) does not command us to kill the infidels (although not everyone would agree with that). So we have the case where in one part of the world, divine revelation contradicts what divine revelation says in another part. A crisis of the spirit in a soldier from the USA in the Persian Gulf War might lead him to decide not to kill an Iraqi soldier in his sights, while another crisis of the spirit in an Iraqi soldier might lead him to decide to kill the American who is in an equally helpless position. Both reject what personal inclination demands, and submit eventually to the Word of God -- with opposite results.

The theory of God keeps getting more complicated as problems like this arise. This theory, to say the least, lacks universality. It must be clear to the adherents of different faiths that their beliefs differ radically from those of others who also lay claim to belief in God. The only solution that does not lead to God contradicting Himself is to decide that one's own faith is the RIGHT one, while the others are in

error on the points of dispute -- they have mistaken their own thoughts for revelations from God. In countries where freedom of religious belief and expression are considered extremely important, this leads to the odd situation in which a constitutional edict requires distortions of the True Word of God to be tolerated. In other words, one must figure how how it is all right for other people to go against the word of God, while it is NOT all right for oneself to do the same thing.

All in all, I think that my theory makes more sense. It allows us to understand the experience of revelation in a way that does not require all people to experience the same, or even consistent, revelations. It does not in any way deny the reality of the experience of relevation: it merely explains it in a different way. In a context that allows equal consideration to all varieties and details of religious belief, I think that my proposition remains free of contradictions and entails the postulation of the fewest entities, whereas the theory of God requires the multiplication of entities and the maintainance of a set of principles that differ from believer to believer -- all of them True.

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Date: Wed, 22 May 91 09:06:55 -0400
Reply-To: coombs@cs.rochester.edu
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: David Coombs <coombs@CS.ROCHESTER.EDU>
Subject: so long and thanks for all the fish

It's been great fun, but I need to get my thesis together.

cheers,
dave

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Date: Wed, 22 May 91 08:59:36 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: 11th Order, Nice work Clark, Hi Dag

[From Rick Marken]

Bill Powers (910521) -- what can I say? What a beautiful post. But pretty strong stuff -- a theory of 11th order. The net sure quiet's down when you get to that level. The "god theory" of revelation is just not going to go away. In fact, it looks like the "god theory" forces just advanced their theory another notch yesterday by blowing away Rajiv Ghandi. I think your point about constitutionally mandated religious tolerance was great -- I've always wondered how it could really work since it does require (if you believe in the "god theory") that you allow other people to go against the word of god while you don't. I think it is becoming clear that it can't work. It's not going to work in India any more. It's barely holding on in the US. It seems to me there are only two possible solutions -- one (which I think Ed suggested) is to accept the god theory and hope (or require) that everyone agrees on just which god is really out there or give up the god theory and try an alternative -- possibly brain theory; the theory of 11th order control systems. I think that the latter is quite unlikely, ever. Pretty depressing. My rule of thumb, however, is to always try to live in the society that has the least institutionalized commitment

to a particular version of the god theory. I hope America can hold out for a while longer -- but it looks like, after a brief period of enlightenment, the world is prepared to dip into another millennium of testing for the correct god theory. Oy vay.

To Clark McPhail- I'm sending you some programs. Thanks for the copy of the paper you did with Bill Powers and Chuck Tucker on the computer simulation of collective action. GREAT paper. It's damn near as good as one of mine. Damn it, it's even better. I'm jealous. Very nice work; well written and very interesting. I also got a copy of your book Clark - it looks very good. I havn't read it yet but my wife love's the title. An english major, you know.

Dag Forsell -- Welcome to the net! I hereby publicly commit myself to reviewing the training materials you left with me. I'll post a review tomorrow (or on the weekend).

Regards to all.

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Date: Wed, 22 May 91 13:46:13 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: The theory of 11th order

[from Joel Judd]

Rick Marken (910521) and Bill Powers (910521):

"I wanted out, but they keep pulling me back in." (Al Pacino in Godfather III)

Well, this discussion is a nice distraction from my dissertation proposal (which the committee's finally going to hear about on Friday), so as long as it's interesting to someone, I'll offer up questions.

Bill mentions regarding behavior resulting from "revelation":
>so that sudden new
>understandings and new methods of acting appear, as if from nowhere. I
>would argue that there is no reason to think that such changes in the
>conscious world are due to any factor outside the brain -- i.e., a
>supernatural being. On the other hand, there is no evidence that such
>supernatural intervention does not occur; we do not have the ability,
>now, to tell the difference between supra-conscious processes originating
>inside the brain and supernatural processes originating outside the brain
>-- as long as our only evidence is the experienced result.
> So in terms of
>scientific knowing, we would have to agree that we do not know which is

>the correct proposition, if either.

This is why I have been reluctant to pursue the discussion too far, since one comes to this conclusion based on the evidence we have now. I'm glad, though, that the way I presonally interpreted CT on this point seemed appropriate.

At the risk of turning this into a forum for personal beliefs, I would mention some fundamental notions in order to respond to the other comments received from Rick and Bill. Assume (and I know this is a big assumption) the following scenario: there exists a couple of Gods (it takes two to have kids, you know) who have some offsrping and want to offer a physical/mortal existence to them (for reasons I won't go into fully). This existence requires a place to live and the niceities of mortality--birth and death. Part of the reason for sending the children away is to let them learn to make choices concerning--that's right--Good and Bad, Right and Wrong. Following the mortal part of this plan, the children would continue on immortally in different states of "maturity" and "knowledge" according to their actions on earth. Now as soon as this plan was presented, two people offered to help carry it out--right again--Lucifer and Christ (both sons of God, by the way). [In case you think I'm making this up, check out Isaiah and Revelations, among other sources] However, they quibbled over an important issue: Free Agency. You see, Lucifer, being a good guy and a little bit selFISH, offered to make sure that ALL God's children would make it back safe and sound--by forcing them to make good choices. Christ, on the other hand, said he would let everyone have a say in the matter, allowing them choices and, knowing that children inevitably goof sometimes, would do his best to allow everyone to make up for their mistakes, and show them how to do so. Well, we can find out how this (mythical) story turns out by looking at christian theology. Lucifer becomes the bad guy by resenting God's rejection of his offer, and he and his followers leave without tasting mortality.

Returning to science, I repeat I try not to get worked up about science/religion (dare I say S-R?) arguments because of conclusions like the following:

>In such cases we have to choose something as a provisional belief, to
>take the place of knowledge. The question then is which belief to choose,
>not on grounds that it is "right" (because we do not know which is
>right), but on whatever practical grounds we can find.

I believe that the crowning principle of mortality is freedom (as do you all, but perhaps for different reasons), and from my point of view part of the reason for being here is to see what we'll do without that convincing certainty that "Dad" is always looking over our shoulder. However:

>If that possibility [revelation] exists, then we must ask about the
consequences of
>making a mistake: of mistaking a brain process for a revelation from God.
>
>Suppose you suddenly get the thought, crystal-clear and compelling and as
>if from a higher source, "All of your troubles are being caused by the
>Jews. You must therefore kill all the Jews, and purify the land." But if
you
>decide that this sudden idea is a revelation from God, you have no choice
>but to obey. The theory of God does not allow for ignoring God's word, or

>re-evaluating it.

This and Rick's comments along the same lines point out many people's worst fears about religions. However, religion can suffer from the same confusion as science. For example, the characterization of the "theory of God" given above assumes that anyone is justified in professing revelation and recruiting others to help. This is not the pattern in christianity, where one person is called at a time to speak for God ("Prophet"). Nor can a prophet say whatever he wants to say and get away with it. There are any number of checks and balances on people's behavior by which we can judge--"by their fruits ye shall know them," "do unto others..." etc. We can all think of worst case scenarios where God, Christ, and others have been invoked in the name of genocide, purification, education, and other causes. But I don't think any of those crusades spread peace, goodwill, and cooperation, the hallmarks of God-like behavior. We can judge religion and religionists with a few almost common-sensical standards, like the couple just mentioned.

>The theory of God keeps getting more complicated as problems like this
>arise. This theory, to say the least, lacks universality. It must be
>clear to the adherents of different faiths that their beliefs differ
>radically from those of others who also lay claim to belief in God.
>...whereas the theory of God requires
>the multiplication of entities and the maintainance of a set of
>principles that differ from believer to believer -- all of them True.

Unfortunately, this is one of the best ways to turn people off of something--provide too many contradictory choices. Returning to the scenario laid out above, and assuming it were correct, wouldn't this be a great way to turn people off of religion/God?

There are two other issues I'll dangle. One concerns the idea of Spirit/Body (the soul). That revelatory communication (if it occurs) would take place at a level we generally talk about at LOWER levels I find intriguing. I tend to wonder about the Spirit/Body interface and how these higher levels might relate to/communicate with things "spiritual" as opposed to the more physiological functions of lower levels of the hierarchy. Of course if you don't entertain notions of immortality then such issues are not interesting.

The last concerns the perspective on life obtained from belief in God and belief in Man. I almost never bring this issue up, because it directly addresses the worst-fears examples which always come up in discussions of religion. If one is focussed entirely on mortality and birth and death as the bookends of one's existence, then life often becomes overwhelmingly precious and something to be maintained at all costs. If, on the other hand, one believe's that "life" began long before birth, and extends long after death, then the mortal part of this picture becomes almost a "drop in the bucket," as it were. That DOES NOT MEAN that life is valueless or worthless, only that it is not EVERYTHING. When someone whips out an Old Testament "myth" and shows how this beneficent God drowned thousands of Egyptians in the Red Sea, or murdered thousands of Sumarians in the Middle East, I tend to look at the context of the story (what we DON'T know about the situation as well as what we do), and consider the Big Picture. And when a child dies of malnutrition and disease in Bangladesh, or a family is wiped out in a Kansas tornado, I don't curse God, or complain that if God existed He certainly wouldn't let such things happen. Instead, I try to do

my part to see that the corner of the world I can influence is made better. God is not around to babysit us every second anymore than most parents are around their 50 year-old children--but they certainly are available to give advice and offer solutions, IF THE CHILDREN ASK (and sometimes when they don't).

Now none of this is very scientific, or concvincing experiementally. But it's how I make sense of the world, and my life in it. THAT can be explained by CT, as Bill and Rick and others have pointed out. But it probably can't be proved. Back to more mundane matters.

Joel Judd

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Date: Wed, 22 May 91 19:07:57 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: teaching perceptions

From Ed Ford May 22, 1991

Rick - I think it is possible to teach how we control perceptions. I find that if you begin with the fact that we deal with people according to how we perceive them, there is a universal understanding of this experience, especially if you follow that with several examples (you're more sympathetic to a blind person who bumps you than a sighted person, or it depends on how we perceive people as to whether we'll take abuse from them, such as special ed teachers taking abuse from their students that they wouldn't take from their spouse). The next step is for them to understand how they try to change a perception of those things over which they have control versus those things over which they don't have control. There I use the well-known speedometer example. They watch the speedometer change to correlate to the speed they want. Then I have them reflect on an area where they have little or no control. I ask them "Can I change you or make you do something you don't want to do?" The answer I always get is "no". Then I ask, "What do you have control over?" and they always say themselves. Then I teach them how they can control their perception of being close with their spouse through spending quality time with them. Since this sense of being close is never understood by anyone unless they have had a prior experience of this phenomenon, I teach them how to develop this loving perception of their spouse. Once they've attained this goal and experienced a close relationship with their spouse, they then begin to sense some control over their sense of closeness with their spouse. Teaching that we control a perception can also be accomplished if the client has had the experience of changing a perception that can be identified. I find you have to search for an area where they have had the experience, then on reflection of the experience, they'll understand the concept. Otherwise, you have to teach them how to set up this experience, and through it understand the concept of controlling a perception. The best example I can think of is this: I always perceive myself as a lot closer to my wife after we have taken a long walk. I then sense control over that perception of closeness through asking my wife to take a walk and then actually walking with her. That's really what controlling a perception is

all about.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, Arizona 85253 (Newsletter address)

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Date: Wed, 22 May 91 23:16:13 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: change of viewpoint on religion issue

[From Bill Powers]

Rick Marken (910522) --

Before we get any farther into showing the defects of various god-theories, let's pause and figure out what we're doing. Control theory is not going to settle the question of the existence, nature, or purposes of God. That question isn't even interesting from the CT point of view. What is interesting is the fact that people support such beliefs and that the beliefs play some role in determining their principles, strategies, procedures, categories, and so on. If we wanted to play games at the system-concept level, we could make up our own stories about why we're here and what it's all about. We could seek converts, start a church or a political party, and go around claiming that our system concept is better than anyone else's. We could even have our own war once we got the hang of it. It's been done lots of times before.

Speaking strictly as a control theorist, a position from which I've been straying lately, what I'm interested in are the system concepts underlying the various god-theories. I want to know if there are sets of principles from which they are drawn; if the principles guide logic and reasoning; if logic and reasoning select sequences of actions; if the sequences are indeed composed of symbols (category-perceptions) -- and so on. In other words, I want to know if the hierarchical control theory model actually works as an explanation of human experience and behavior. As a control theorist, it isn't my business to offer free advice concerning which system concepts are the best. (MINE are, of course, and they're copyrighted -- all rights reserved in South America).

As I said, I've been straying from this course. Straying from it involves saying things like "How can your system concept be the only True one when I know of many people who believe in a different and even contradictory one?" That amounts to trying to tell someone his system concept is no good, or that someone's is no good. If people are control systems, and if they all have 11th-level (system concept) organizations, and if they each develop in a fundamentally autonomous way, then of course they are going to end up with different system concepts, even when they think they have the same system concepts as others do.

In fact it is very hard for people to agree on system concepts even when they try. It isn't so much that they resist having their system concepts modified to fit the group, but they really have only a foggy idea of what the "group system concept" is supposed to be. Perceptions of this level are extremely hard to communicate. Religious and political groups keep forming and fragmenting for this very reason: the people develop divergent perceptions and goals, get into conflicts, and split up into

smaller groups to eliminate the conflict. This happens in EVERY case where people try to share important system concepts, not just in religion. If anyone gets fanatical or fundamentalist about control theory, it will happen here, too. The more important the goal (meaning, the smaller the error that is tolerable), the less difference in interpretation is required to create a significant conflict.

There are many things we can say as control theorists about system concepts without getting into judging their substance. The point of a hierarchical control model is to account for all the levels of human functioning that we can identify. We certainly have to consider an important subject like religious belief, because it is a phenomenon of human experience. We are even interested in the content of those beliefs. But the interest does not have to do with the correctness of the content; only with its relationship to lower levels of control.

So if I say to Joel Judd (910522), as I'm inclined to do, "Joel, I don't believe the story you tell," I am not speaking as a control theorist but only as a human being who prefers his own stories. I'm willing to argue on this subject as long as anyone feels like participating, especially if there are things I really should be doing but don't want to do, but if I do so I won't be talking about control theory. I'll just be telling you how William T. Powers is organized -- one five billionth of the human race. Maybe I'm doing that when I talk about control theory, too, but I'm a heck of a lot better organized in that field than I am in the field of spiritual subjects.

So, Joel, it's quite unimportant whether I believe your story or not -- as long as we agree that we're here to talk about hierarchical control theory. If you could analyze the story into system concepts, principles, programs, sequences, and so on, we could talk about how well the hierarchical model fits the way these perceptions work together and the way a person might behave to maintain them at their respective reference levels. Then we might come to understand something about belief itself, instead of trying to decide which beliefs are correct. I understand that from your standpoint your beliefs are true and right. From my standpoint, so are mine. With that settled, I think we can talk about belief as a phenomenon of human nature, and return to our original subject.

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Date: Thu, 23 May 91 12:23:24 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Why religion?

Rick Marken (910521) says:

> But control theory really get's to the "soulful" aspects
>in a particularly deep way. Control theory explains (rather than explaining
>away) one aspect of people that most deeply defines our human nature --
>our purposefulness. Suddenly, teleology is no longer a spiritual mystery but
>an understandable characteristic of closed loop, negative feedback organiza-
>tions of matter.

I don't follow that. I can see all the lower level control systems as being imbued with purpose by the reference signals that direct them, but I can't see an infinite regress, which would be required if there is a "deep" purposefulness. Either one has a simple closed dynamical

system at the top level -- "simple" in that it is not a control system with an externally supplied reference -- or the top level has a magically supplied external reference -- God-supplied. I prefer the former view, but both seem to be outside the purview of CT.

Evolutionarily, it is almost trivial to say that strutures that behave in such a way as to remain stable or to be replicated are those we will observe. A dynamical system that has evolved control systems as parts of itself will be particularly stable, and such a system that also replicates will look like -- what else? -- life. There's no teleology in that, and one doesn't need to invoke the control systems that aid the stability in order to account for the fact of stability. If there is a "soul" in such a system, it will be found in the dynamics that provide the reference signals for the highest level control systems. They see "purpose," but the sole (soul?) purpose that exists at the top is a post-hoc reflection of the fact that the organism is the product of successful evolution; it is constructed to survive long enough to reproduce.

Martin Taylor

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Date: Thu, 23 May 91 11:05:36 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: A history of control andanother reason for religion

FROM CHUCK TUCKER 23 MAY 1991

ON A HISTORY OF CONTROL

I finally located a copy of James R. Beniger's book THE CONTROL REVOLUTION Cambridge, MA: Harvard University Press, 1986 HM 258 .B459 which was recommended by Don Ploch a sociologist at Tennessee who does work with computer simulation programs. The book is about the deveolpment of the "information society" so a great deal of it is not relevant to the history of control as we see it but the book does provide documentation for the centrality of the idea of self-regulation in the development of a modern society and points out all of the innovations that relied on the understanding of control. There are a number of useful statements in the book one I found rather nice was:

> "The more general point is that symbols do not effect control by causing but by meaning. Cause is effected by individuals who recognize the symbol *qua* symbol and hence its meaning. Meaning might be defined - folowing Pierce and the pragmatists- as the output that results from that meaning as input. Such definition involves a black box, but not one that contains a cause: relations to an action that are internal to an actor = like intention or motive - are not considered causes rather part of the action. Language, law, and other symbolic cultural systems do not control individuals through causation but through meaning. Although human being do not differ from other species in that their worlds have meaning for them, in this narrow definition, we are unique in that our meaning systems are artifactural (cultural), not entirely learned, infinite in some capacities, and otherwise reflective of the special generative

structure of our brains that is language." (p. 95)

In the PCT vocabulary perceptual signals are meanings for the person (as Bill pointed out in this recent post in answer to Gary on Behavior = perception). The Beniger book offers some some evidence that the issue of control has a long and glorious history largely ignored by behavioral scientists.

ANOTHER REASON FOR RELIGION (actually two)

Of course, it was Max Weber who argued (and apparently supported it quite well) that the protestant ethic was necessary for the development of capitalism. Now some who say, yes, I know, that it the problem with it but many of the ideas of capitalism are quite useful.

Another reason I found in a recent article by Ken Boulding "The Concept of the World Order" in the May/June 1991 issue of ABS (34:5:581-593). He suggests that modern science (remember that stuff) relied on Christianity for its development by asking why modern science did not develop in China, India or the Islamic world as it has in the Western world. He speculates:

> "Christianity, which was the dominant ideology in Europe (all early scientists, for instance, were Christians) was a working-class religion, founded by a carpenter, propagated by a tent maker and fisherman, and so legitimated the reality of the material world. It was hard for modern science to come out of Buddhism or Hinduism, simply because the intellectuals in a sense denied the ultimate reality of the material world, even though Budda's concept of detachment was an important part of the ethos of modern science. It was hard to get chemistry out of a philosophy that regarded the material world as an illusion. Furthermore, Christianity has the Bible, which was contantly available to challenge the corruption of the organized Chuch and had a lot to do with the Reformation. . . . The Reformation in its many forms challenged authority and in a sense legitimated novelty, without which, of course, science was impossible. After Galileo (1564-1642), science flourished mainly in Protentant Europe..." (pp. 585-586)

By why not the Islamic world? He says:

> "Perhaps again, the problem was that the Ottoman Empire was too well organized. The Koran also is the work of a single person and its potential for change seems to have been exhausted by the 1400's. The Bible is the record of a very large number of persons and human experiences and hence its potential is harder to exhaust." (p. 586)

So, there might be another reason that religion is important in addition to those already mentioned on the NET.

CODA

By the way, my copy of "Behavior: The control of Perception" has a colon not semicolon in the title; is mine the only misprint?

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Date: Thu, 23 May 91 10:31:16 -0700

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Theory of 11th order, Perceptual Control, "reality"therapy

[from Rick Marken]

Bill Powers (910522) --

>Before we get any farther into showing the defects of various god-
>theories, let's pause and figure out what we're doing. Control theory is
>not going to settle the question of the existence, nature, or purposes of
>God. That question isn't even interesting from the CT point of view. What
>is interesting is the fact that people support such beliefs and that the
>beliefs play some role in determining their principles, strategies,
>procedures, categories, and so on. If we wanted to play games at the

OK, speaking as a control theorist, I think I am theorizing that religious phenomena (among others -- such as ideological phenomena of variuous flavors, ethnic phenomena -- that is any experiences that seem to be based on a set of principles) are, in the model, 11th order control systems. I believe the control model would say that different people want to perceive themselves as "christians" or "jews" or "nazis" or "communists" or "pacifists" because of differences between these people in terms of 11th order reference signals. One intersting thing about the 11th level (that Bill brought up) is that the reference levels for these perceptions seem to come from "outside" of the person. I imagine that a person whose reference for "religiousness" has them controlling for "Chistianity" (as they understand it) experiences the source of this reference as being outside -- the higher power that is above him. This is certainly the way I experience my own reference for religiousness (which is obviously set at "atheism"). It feels less like something I chose than like something I am.

For some reason it is difficult to become conscious of the fact that the reference for a system concept is selected by you -- not something that is "out there" that imposes itself on you. Actually, system level references are sort of imposed on you (from the model's point of view) by reorganization. But it is hard to see that the reference for a religion is something that your brain came up with in the same way that you brain comes up with a reference for a particular sitting position. For some reason, it is possible (though not necessarily easy) to learn that you are the one who selects the references for a particular configuration (like the sitting position) but it is nearly impossible for people to realize that it is they who have selected the reference for a particular system concept -- though not to satisfy a higher level goal but as a result of fairly random reorganization to satisfy intrinsic needs. This may be an important point for therapists. The 11th order may be the "id" of control theory -- the source of one's desires (references) for particular system concepts may be very difficult (if not impossible) to make accessible to consciousness. My hypothesis is that consiousness (whatever that is) can only become aware of the source of a reference signal if it can take a point of view from a level of the control hierarchy that is at the level from which that reference is sent. Thus, it is possible to become conscious of the source of the reference for the sitting position configuration when you can look at configuration perceptions as a means of perceiving a higher level perception -- such as a particular

relationship between your line of sight and the computer monitor. I suspect that it is difficult (or impossible) to look at system concepts from the point of view of what ever it is that wants to use system concept perceptions to achieve their goals. Anyway, to the extent that it is possible, the hierarchical control model gives the term "consciousness raising" a whole new, drug free meaning.

The bottom line is that, from the control theory point of view, system concepts (like the ones Joel and Ed and Bill and I are discussing) are perceptions that are maintained at particular references levels that have been set there for reasons that are not that well understood (in terms of the model or in terms of one's own consciousness). I think a person who understands the control model would have to accept this fact about the nature of their own system concepts.

Problems arise at the system concept level , not because some system concepts are bad while others are good but because (according to my understanding of the control model) people tend to assume that the references for their system concepts come from "out there". THAT IS THE PROBLEM. It leads to the conclusion that the level at which you want to keep a particular system concept is the truly right level -- forgetting to add that it is just "the right level for you" -- because it is YOUR REFERENCE SIGNAL. There is nothing wrong, really, with any system concept as long as you can remember that it is just right for you -- not necessarily for anyone else. This is the message of control theory about all levels of perception. The "right" level of a perception is the level that matches YOUR OWN reference for that perception. The only caveat is that, in controlling your perception, you should do so without interferning witht the ability of another person to control their own perceptions. This interference is called conflict and control theorists generally want to find ways to avoid it. Thus, system concepts like "kill the xxx" can be considered bad if you agree with this principle -- of conflict avoidance. Obviously, killing is the ultimate way to prevent people from controlling their own perceptions.

If people could just be happy controlling their own system concepts and let others control their system concepts then all would be fine. I could care less what a person believes. My problem comes from the fact that most system concepts have principles that involve other people-- like the Moslim principle (and christian too) of converting the infidel. This stuff scares me; I think principles like that come about because peopel don't understand that system reference level (the "right" way to be) are not "out there" they are "in the individual". Why system concepts seem to include edicts about how other people should behave is an interesting question -- one that social psychologists, especially those interested in collective behavior, should look at very carefully. I'll leave that discussion for later.

It's easy to see when people are confusing internal references for external references. People who say "we have to do it right" obviously believe that their reference for whatever perception they are controlling is "out there"-- so that anyone can control relative to it. My mother does this all the time and it drives me nuts (well, not any more -- I just look at it scientifically). But it shows that, even references for lower level perceptions (like the reference for a particular arrangement of furniture) can seem like it comes from "out there" when it is really coming from a higher (and not consciously available at the moment) system in you.

On a related note, Ed Ford (910522) writes:

>Rick - I think it is possible to teach how we control perceptions.

I never said it wasn't. I just taught my kid to drive a car.
I just never told him he was learning to control perceptions --
I just referred to things like pedals and curbs, which he thought of as
objects out there in the real world. He learned to control those perceptions
anyway. What I said is it's hard to teach people that what they experience
as "the real world" is a representation of the real world -- a percpetion.

>this phenomenon, I teach them how to develop this loving perception
>of their spouse. Once they've attained this goal and experienced a

I presume you are teaching a reference perception here.

>close relationship with their spouse, they then begin to sense some

and now they have achieved it? Did the spouse participate? Isn't the
spouse a control system? Suppose s/he doesn't want to participate in
the development of the other spouses perception of closeness? Does the
spurned spouse just control a perception and ignore the fact that the
other spouse isn't contributing to the perception?

>control over their sense of closeness with their spouse. Teaching
>that we control a perception can also be accomplished if the client
>has had the experience of changing a perception that can be
>identified. I find you have to search for an area where they have
>had the experience, then on reflection of the experience, they'll
>understand the concept. Otherwise, you have to teach them how to
>set up this experience, and through it understand the concept of
>controlling a perception.

Nearly every experience I have of controlling (the position of my car,
the words on the screen -- and they are sure not hard to find) I would
describe the experience as controlling something. Adding the "perception"
just seems pedantic and gratuitous. Tonight, I am going to control
the position of my car across LA until it arrives precisely in my driveway.
How does changing that sentence to "tonight, I am going to control the
pereption of the position of my car across my perception of LA until I
arrive at my perception of my driveway and produce the perception of my car
being in my perception of my driveway" help things?

Again, I KNOW that control systems control perception. I also think I
know why that is important. But nothing you have said above makes it
clear to me why it helps to teach someone that it is perceptions (rather than
reality) that they are controlling. How does it help! Would my kid be a
better driver (he's a great driver) if I had said "keep in mind that it is
your perception of the car with respect to the other cars that you are
actually controlling here; just pick a controllable perception and you'll
be OK".

> The best example I can think of is this:
>I always perceive myself as a lot closer to my wife after we have
>taken a long walk. I then sense control over that perception of

>closeness through asking my wife to take a walk and then actually
>walking with her. That's really what controlling a perception is
>all about.

Again, I don't get it. I know that you are controlling your perception of closeness (though again, the means you use assume that another control system will reliably behave in a way that allows you to bring your perception to the desired level) -- but why does it matter that you call this relationship a perception rather than a real aspect of the world (like the relationship of my car to the road).

I think, by the way, that the importance of the fact that we control perceptions rather than reality is related to the fact that there are levels of perception. The same experience can be controlled at many different levels. It is possible to select ways of perceiving things at certain levels that create CONFLICT. So the way we perceive does matter--and the reason is CONFLICT. When there is conflict, you can either try to pereive things in a new, non-conflicting way or change the way the higher order system that is setting the conflicting goals tries to schieve its goals.

Unless this is made clear, I think "control of perception" can sound like pedantry.

Finally, Here is a brief comment to Dag Forsell:

I have your "Alighment/Mission Statement" and "Discussion of issues and control theory". The alignment/mission statement seems to be a template for a statement of agreed higher-order goals for two control systems (people) working as partners in an engineering firm. It looks OK to me. I have a bit of trouble with terms like "accept responsibility for our lives" and "efficient perception of reality". I also think that the statement that control theory views people as controlling themselves misses the point by enough to be misleading.

The "efficient perception of reality" statement makes me wonder -- what did a guy who developed something called "REALITY therapy" see in a model of behavior as the control of PERCEPTION. Is the idea of reality therapy that REALITY is PERCEPTION? If so, why use the term reality? It suggests a therapy that helps you get in touch with reality which suggests that the therapist knows what reality is and you (the therapee) should too. If I understood behavior as the control of perception and problems requireing therapy as the result of perceiving things in a way that prevented non-conflicting control of those results I would never have thought of calling my therapy "reality therapy". Maybe, "control therapy" or "perceptual reconciliation therapy" or, the best, "conflict resolution therapy". But "reality therapy"? What could be more misleading. If you clinicians are still around, could you tell me why the hell William Glasser, who claims to have understood control theory before he even discovered it, called his approach to therapy "reality therapy". I smell condescention here.

Your discussion of control theory seems reasonable. It does emphasize the control of perception. Again, I would suggest (as I did to Ed) that you make clear the relevence of perceptual control to the problem of conflict and how to resolve it. After all, I think that's what the value of control theory is for effective management -- finding ways to perceive the production

process so that there will be minimal or no conflict between the cooperating contributors to the process.

Speaking of which, I'd better get back to the process of producing some briefing charts.

Hasta Luego

****

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213 336-6214 (day)
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==========

Date: Thu, 23 May 91 13:02:13 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: change of viewpoint on religion issue

[from Joel Judd]

Bill says (910522),

>Religious and political groups keep
>forming and fragmenting for this very reason: the people develop
>divergent perceptions and goals, get into conflicts, and split up into
>smaller groups to eliminate the conflict.

This made me think of a couple of things: 1) the adoption of conquerors' religions in history, eg. the indians' "acceptance" of catholicism in Peru. Many of their beliefs were tolerated by priests and have become part of the ritual worship for andean people, a mixture of pagan and christian. A catholic from New York visiting a chapel in Peru might be astonished or even shocked at the differences in what ostensibly is the same religion. 2) the problems caused by church clergy adopting political stances (eg. Archbishop Romero. Either one of these would make very interesting CT theses for some student of political science, anthropology, etc. Unfortunately they have nothing to do with my own...

> "Joel, I don't
>believe the story you tell," I am not speaking as a control theorist but
>only as a human being who prefers his own stories. I'm willing to argue
>on this subject as long as anyone feels like participating, especially if
>there are THINGS I REALLY SHOULD BE DOING but don't want to do, but if I
>do so I won't be talking about control theory.

I appreciate the clause (which I emphasized).

>So, Joel, it's quite unimportant whether I believe your story or not --
>as long as we agree that we're here to talk about hierarchical control
>theory. If you could analyze the story into system concepts, principles,
>programs, sequences, and so on, we could talk about how well the
>hierarchical model fits the way these perceptions work together and the

>way a person might behave to maintain them at their respective reference
>levels.

Agreed. But see clause above!

Joel Judd

Date: Thu, 23 May 91 13:32:19 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Competence and performance

[from Joel Judd]

I continue to run up against a dichotomy in SLA that I am going to have explain probably as often as I will the CS hierarchy. It's not a dichotomy unique to language learning, but it plays an important role in virtually every extant theory of it. It is the competence/performance problem. In general, it says that we can often (perhaps always) perceive aspects of the L2 BEFORE we can utilize (produce) them. Chomsky speaks of grammatical competence; the Silent Way advocates X number of hours language listening before the student tries to produce anything.

Lately I've been reading articles by Gerald Neufeld who is sort of an iconoclast in SLA in that he continues to poke at the assumption that adults cannot achieve native-like fluency in L2 phonology, providing evidence that at least under certain conditions they can. The article I read yesterday though maintains this dichotomy in discussing three groups of French-English bilinguals. These were native French speakers (NF), advanced bilinguals (AB), and beginning bilinguals (BB). He subjected these groups to a number of perception and production tests. The perception tests consisted of rating recordings of bilinguals considered native-like in their speech. The production tests consisted of imitated and free speech of the subjects.

Ignoring for a moment the fact that he used the old relative frequency methods of group comparisons, these were the general findings. The AB group often (more than 80% of the time) agreed with NFs in judging phonetic components (eg. nasal vowel difficult for non-natives to master) of native-like speech as correct or incorrect, while BBs were abysmal at doing so. However, in imitating utterances and answering open questions, the AB group performed markedly more like the BB group in the phonemes of interest.

One of the author's questions, then, is why experienced bilinguals can make quite acute judgments concerning L2 speech, but cannot themselves produce that kind of speech. If I can end this post here for now, I'd like some initial reponses before I go into this more.

Joel Judd

Date: Thu, 23 May 91 13:36:54 cdt
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "McClelland,Kent" <MCCLEL@GRIN1.BITNET>
Subject: Personal uniqueness

[From Kent McClelland]

Between grading exams, I have been trying to catch up on what's happening on the net and have finally taken a close look at Chuck Tucker's (910521b) list of epistemological "Statements from a Sociocybernetic Perspective of Human Conduct."

I think I understand why most of these statements are made, but the last one has me puzzled:

> Each person is <<not>> unique, nor have unique ideas, nor have
> unique perceptions of things.

How does that assertion follow from a control-systems view of things? What exactly does it mean?

Kent

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Date: Thu, 23 May 91 14:22:08 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: Theory of 11th order, Perceptual Control, "reality"therapy

[from Joel Judd]

Well now this showed up as I was logging off and it raised a couple of questions.

Rick mentioned among other things (910523),

>OK, speaking as a control theorist, I think I am theorizing that religious
>phenomena (among others -- such as ideological phenomena of variuous flavors,
>ethnic phenomena -- that is any experiences that seem to be based on a set
>of principles) are, in the model, 11th order control systems... One intersting thing about the
>11th level (that Bill brought up) is that the reference levels for these
>perceptions seem to come from "outside" of the person. I imagine that a
>person whose reference for "religiousness" has them controlling for
>"Chistianity" (as they understand it) experiences the source of this reference
>as being outside -- the higher power that is above him. This is certainly
>the way I experience my own reference for religiousness (which is obviously
>set at "atheism"). It feels less like something I chose than like something
>I am.
>
> For some reason it is difficult to become conscious of the fact that the
>reference for a system concept is selected by you -- not something that
>is "out there" that imposes itself on you. Actually, system level references
>are sort of imposed on you (from the model's point of view) by
>reorganization.

Several times in the last couple of days the 11th level has been spoken of in sort of an offhand way, as if christian/atheist etc. sort of can be there one day and gone another. However, I have perceptions of higher levels as possibly having rather long time frames--ditto the reorganization which may involve them. "Christian" may be a perception built up over 10 years, 40 years, or a lifetime. We probably would not consider a newborn a Protestant, the newborn itself almost certainly doesn't either. At what point do we say he/she is? When their behavior fits our perception of "acting like a Prostestant?" Maybe this was all assumed in the discussion, but I wanted to make sure. The same thing would hold for other concepts like "language", which develop over years of experience with language. (the quote from Beniger's book Chuck Tucker sent was great).

Wouldn't some of the mysterious nature of the origin of higher reference levels be explained if we admit these longer time frames in their development? It then wouldn't be possible to point to a discrete experience and say, "That's when I developed a reference level for 'family'." That would address the following comment:

>But it is hard to see that the reference for a religion is
>something that your brain came up with in the same way that you brain comes
>up with a reference for a particular sitting position.

On other hand, if reorganization commences to address intrinsic needs, and so much of peoples' reorganizations end up working with religious ideas/God, what does that suggest about the source/purpose whatever of intrinsic needs?:

> though not to satisfy a higher level goal but as a
>result of fairly random reorganization to satisfy intrinsic needs.
>This may be an important point for therapists. The 11th order may be the
>"id" of control theory -- the source of one's desires (references) for
>particular system concepts may be very difficult (if not impossible) to
>make accessible to consciousness.

The final thing I'm not clear on:

>THAT IS THE PROBLEM. It leads to the conclusion that the level at which you want
>to keep a particular system concept is the truly right level -- forgetting
>to add that it is just "the right level for you" -- because it is YOUR
>REFERENCE SIGNAL. There is nothing wrong, really, with any system concept
>as long as you can remember that it is just right for you -- not necessarily
>for anyone else.
>
> It's easy to see when people are confusing internal references for external
>references. People who say "we have to do it right" obviously believe that
>their reference for whatever perception they are controlling is "out there"--
>so that anyone can control relative to it. My mother does this all the time
>and it drives me nuts (well, not any more -- I just look at it scientifically).

I can understand that the theory itself makes no judgments about rightness/wrongness--it is an explanatory tool. That can be as true for development as it is for the description of a mature CSH. But I'm not sure about the idea of negating "right things out there." I've been at the keyboard too long, but I'll ask this then quit. Isn't there a "right way" of driving a car? That's not the same as saying that there is a right way to do EVERY LITTLE THING EVERY TIME I get in the car. Rather there is a system concept for "right way to drive" that drivers share. I don't think it's so different for everyone (well maybe men/women--oops). We don't sit in the backseat to drive, we don't use our hands to manipulate the pedals, we don't go down the road backwards, though we can (and usually do in High School) do these things. There's a right way to do a lot of things: use the language, pay taxes, get a Ph.D., worship God. For some things, though, there is more than one right way...uh, I just lost my train of thought.

Anyway developmentally we have models for developing concepts: parents, God, Michael Jackson. In the case of children we ACT as if there is a right way (OURS) and expect them to adopt it. So how do we teach one another system concepts that we can agree on even though each is an individual?

Joel Judd

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Date: Thu, 23 May 91 13:18:25 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: 11th order, etc

[from Rick Marken]

Damn-- I'll never get any work done with this thing in front of me.

Here's a quick response to Joel Judd's (910523) great questions:

>Several times in the last couple of days the 11th level has been spoken of
>in sort of an offhand way, as if christian/atheist etc. sort of can be
>there one day and gone another. However, I have perceptions of higher
>levels as possibly having rather long time frames--ditto the reorganization

Unquestionably

>years, 40 years, or a lifetime. We probably would not consider a newborn a
>Protestant, the newborn itself almost certainly doesn't either. At what
>point do we say he/she is? When their behavior fits our perception of

When you test for evidence that the person is controlling that variable. Just apply disturbaces and watch for resistence. Acting "like a this or that" is not enough to show that there is control; for example, I can get you to write out a profanity as you move a mouse to counter a two dimensional disturbance to the position of a cursor. You are producing a profanity -- but you are not controlling it -- ie, you will do nothing to resist my attempts to make your hand write a non-profanity. System concepts are probably not controlled until a person is well into their teens. Lower level perceptual abilities also develop over time -- you must be able to control configuration before you can control transitions. Plooij found clear evidence

of this in chimps (who probably cannot perceive and hence control system concepts).

>Wouldn't some of the mysterious nature of the origin of higher reference
>levels be explained if we admit these longer time frames in their

The origin of the higher refernce levels is no more mysterious than the origin of lower level references. They are equally mysterious. The model accounts for the origin of higher order references differently that lower order references -- but there is NO mystery about how it is done in the model.

>I can understand that the theory itself makes no judgments about
>rightness/wrongness--it is an explanatory tool. That can be as true for
>development as it is for the description of a mature CSH. But I'm not sure
>about the idea of negating "right things out there." I've been at the
>keyboard too long, but I'll ask this then quit. Isn't there a "right way"
>of driving a car? That's not the same as saying that there is a right way
>to do EVERY LITTLE THING EVERY TIME I get in the car. Rather there is a
>system concept for "right way to drive" that drivers share. I don't think

Bingo -- I think we have here a place where the content of your personal system concepts may come into conflict with the content of the system concept that we call control theory. This might be a job for "Zen and the Art of Motorcycle Maintenance". But I'll just give you the short answer "NO, there is no right way of driving a car". There are just variable (perceptual) aspects of the car's behavior -- some of which you can learn to influence in predictable ways -- and you can bring these variables to reference levels that you specify in order to satisfy other reference levels. But there is no "Right way to drive a car" unless you are talking about the "real world" constaints on the way you can influence what you perceive. In my car, you can only accelerate forward (when sitting in the driver's seat on a level road) by push on a pedal under your right foot (with the ignition on). If you don't do this -- with the car on and all that -- it won't go. Same in our tracking experiments -- there is only one "right way" to influence the cursor -- because we've set up the world that way.

So the real world (the one we know only in terms of our physical models) does impose constraints on how we can influence the perceptual variables that we are controlling -- but the particular values to which we move our perceptions are right or wroing only in terms of whether they bring higher order perceptions to their reference levels.

The term "right" implies a standard for comparison -- a reference. If you believe that there are standards "out there" for how things should be, then I would simply ask "how do you know them when you see them". Control theory explains how to determine when a variable is being controlled and what the standard of referencis for the variable. When we apply this test to objects "out there" we typically find that the are not maintained at a a standard level unless there is a control system around making that happen. I'm afraid that, from a control theory perspective, "right" is defined by the control system -- not by anything outside the control system that is not also a control system. This has got to be very disturbing to certain system concepts -- but not to mine.

Quick note to Martin Taylor (910523) who says:

>I don't follow that. I can see all the lower level control systems as
>being imbued with purpose by the reference signals that direct them, but
>I can't see an infinite regress, which would be required if there is

My "deep" comment only applied to the teleological nature of the behavior of a control system -- which you seem to reject anyway. The control hierarchy is not an infinite regress and "deep" did not refer to the depth of the hierarchy anyway. I don't see the reference signals as "imbuing" any purpose in teh control system; it is the closed loop, negative feedback organization of the control system that gives it's purposeful character -- countering disturbance (Bill's major criteroin for control) to keep the controlled variable at the reference level.

Finally, stability and control are two different things, but that is a good topic for later.

Hasta Luego

****

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Date: Thu, 23 May 91 18:10:37 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Competence and performance

Joel Judd (910523)

>I continue to run up against a dichotomy in SLA that I am going to have
>explain probably as often as I will the CS hierarchy. It's not a dichotomy
>unique to language learning, but it plays an important role in virtually
>every extant theory of it. It is the competence/performance problem. In
>general, it says that we can often (perhaps always) perceive aspects of the
>L2 BEFORE we can utilize (produce) them.

I'm not at all sure this is true, at least if you mean the everyday (conscious) meaning of "perceive." I am acquiring German in a way, by being in Germany for two or three weeks two or three times per year. I am not deliberately trying to "learn" German (i.e. figure out or read about its constructions and so forth). My vocabulary is probably a small number of thousand words by now, and I make sentences that Germans tell me are often well constructed (though often not, as well). Now the point of all this is that I have noticed on quite a few occasions that I have discovered some regularity (rule?) of German grammar by observing that I have spoken a sentence with a structure that seems to me to be quite strange, on consideration. But when I reflect on it further, I realize that my speech was right, and that Germans do use this strange construction. Would you say that my performance precedes my competence? I don't think

you could say that in any meaningful way. But I do utilize aspects of L2 before I (consciously) perceive them.

Martin Taylor

Date: Thu, 23 May 91 17:59:37 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: 11th order, etc

Rick Marken (910523):

>Quick note to Martin Taylor (910523) who says:
>
>>I don't follow that. I can see all the lower level control systems as
>>being imbued with purpose by the reference signals that direct them, but
>>I can't see an infinite regress, which would be required if there is
>
>My "deep" comment only applied to the teleological nature of the behavior
>of a control system -- which you seem to reject anyway. The control
>hierarchy is not an infinite regress and "deep" did not refer to the depth
>of the hierarchy anyway. I don't see the reference signals as "imbuing"
>any purpose in teh control system; it is the closed loop, negative feedback
>organization of the control system that gives it's purposeful character --
>countering disturbance (Bill's major criteroin for control) to keep the
>controlled variable at the reference level.
>
>Finally, stability and control are two different things, but that is a
>good topic for later.
>

I think we agree, for the most part. My reading of the "purpose" is that the control system uses the feedback to keep the input near the reference. That's why I said that the reference signal imbues the control system with purpose. There is certainly an important distinction between stability and control. The main thrust of the note from which the above quote was extracted is that living control systems have evolved as part of a system that maintains stability without itself being a control system. That super-stable system persists over the lifetimes of many individuals, and, just perhaps, it provides some reference signals to indivudals from outside themselves, that might seem "inspired." But there would be no metaphysics involved even if this were so.

The purposefulness of a control system is in its feedback loop, but its purpose is in the reference signal. How's that for a distinction?

Martin Taylor

Date: Fri, 24 May 91 00:55:00 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Dag Forssell <0004742580@MCIMAIL.COM>

Gary Cziko:

Thanks for adding me to the net. I have been downloading for a

week now and am both learning from and enjoying the exchanges.

I am certainly delighted that you have set up and maintain this net. Thank you!

You asked me to report on MCI MAIL. Bill suggested it to me. They accepted $35 by way of VISA for a one year subscription. It took almost two weeks to get the manual and identification. They connect via 800 phone numbers and have helpful people to guide you. I learned to download in part with the help of a report on my software (Procomm Plus) which I also downloaded.

The reciept of mail is free (Thank my 11th reference), but they charge to send (I expect to discover how).

Charges: Up to 500 characters: $0.45
501-2500 characters: $0.75
2501-7500 characters: $1.00
Each add'l 7500: $1.00

It is a mail system, so I don't expect to be able to give commands to the Listserv, but will try to send one as a message of two words: "info getintro" to the address you gave and see if it responds.

This will be my first attempt at uploading. I am writing off line. What program do you guys use to edit and respond? I have a BBS program called "The Silly Little Mail Reader", but have not made it work yet.

Rick Marken:

Thanks for reading my papers. I am glad you did not find any major misstatement on my part. I can not ask for any thorough critique at this stage, since my presentation is not finished. The particular papers were extracts and summaries, respectively. I will be pleased to have an opportunity in Durango. I am trying to introduce CT to industry in a fruitful way.

When you came across the word REALITY, your configuration references immediately associated with THERAPY and GLASSER. Perhaps you remember my mention of Glasser last august. I am VERY glad that I found CSG, even if it was by way of Glasser's writings and significantly Ed Fords book. I am not a student of Glasser any more (I can still see value in many of his writings, both from a medical perspective and the more recent musings on quality, but he is dangerous because he totally misrepresents (GROSSLY oversimplifies because it apparently reduces his error signals) CT and what one can learn from it).

Anyhow, upon closer scrutiny, you will note that the word therapy is nowhere to be found. I made reference to Abraham Maslow's admittedly unscientific observation that the most outstanding common denominator in people of a high level of mental health is "MORE EFFICIENT PERCEPTION OF REALITY AND MORE COMFORTABLE RELATIONS WITH IT"

I am excited about CT precisely because the model offers "a more efficient perception of reality" - the way the world works and we with it. In quoting Maslow, I am not trying to adhere religiously to any standard, rigorously defined CT terminology, if there is one. It seems to me that in addressing a larger public I must find a way to use terms they relate to. So far in my attempts, I try to use as many synonyms and analogies as I can find.

My feelings do not choose to be hurt, but I will accept your apology when it comes. In my very first input from you I think of Truman ..If you can't stand the heat... I am here to stay!

Uprober: [May 17]

The discussion of the two doors made me think of: I before e, except after c. Right???? I think of myself as an excellent speller (not typist, yet) but I can't seem to get ie / ei right, ever! Same phenomenon?

I planned to wait until week after next to learn to upload, but you pulled me in, Rick, so here I am.

See you all in Durango

Dag Forssell

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Date: Thu, 23 May 91 18:36:55 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Reality

[From Rick Marken]

Dag Forssell (910523)

Boy am I sorry. My comments about reality therapy and Glasser were not directed at you at all -- nor were they meant as a criticism of your work. It just jogged a thought in my mind that I just wanted to make public -- about Glasser's interest in control theory. I've wondered why his therapy is called reality therapy if he is such a fan of control theory. I was asking the net at large -- there are a number of therapists out there and some are familiar with reality therapy(I'm not familiar with any therapy, obviously: If I were familiar with some I might not be so rude). So again, accept my deepest apologies. I do request info on this topic from therapists, though. I really am curious about it.

Best regards
Have a great trip and ,again, welcome to the net

Rick M.

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Date: Thu, 23 May 91 22:44:59 -0500

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Rheostasis

For those of on the network interested in moving down from cloud, I mean level, 11, I wanted to say a few things about a book I've been reading recommended by a campus colleague, Evelyn Satinoff.

Mrosovsky, Nicholas. (1990). _Rheostasis: The physiology of change_. New York: Oxford University Press.

What CSGers will like about this book is that it gives lots of wonderful examples of (mostly) physiological control in animals and humans. There are lizards spending more time under the heat lamp when injected with pathogens so that they raise their body temperature. There are birds losing weight as they
incubate eggs, even when food is made available to them at the nest. There is the method of alliesthesia to determine reference levels (called set points) of temperature in human subjects. There are creative solutions to problems (rats have a warm house and frigid restaurant; how do they both eat and stay warm). There is the reorganization of temperature reference levels in paraplegics to account for the fact that they have lost the control of vasodilation and vasoconstriction in their limbs. And many more fascinating examples of control.

However, even though I know next to nothing of this physiological stuff, I still feel (from a hierarchical control theory (HCT) perspective) that there are problems with the theoretical framework presented. The author coins the term rheostasis (in contrast to homeostasis) to describe changing reference levels and makes a big deal out of this. Since changing reference levels is a core component of HCT, it seems odd (to me at least) that Mrosovsky gets so excited about this, but, of course, his examples are great. Also, while Mrosovsky does consider the possibility that in some case there may be "second-order rheostasis," he's not too excited about the idea as he calls it a "unnecessary concept but logical possibility" (I suppose we would call it a logical necessary concept). Instead, he focuses on two types of rheostasis: (a) programmed rheostasis, when "adjustment of regulated levels . . . is . . . built into developmental or cyclical programs by evolutionary pressures; (b) reactive rheostasis, when reference levels change as the result of current stimuli (e.g., pathogens, injury, food deprivation). This gets me to thinking that perhaps Hierarchical Control Theory is indeed the term we should be using. I noticed that Bill Powers has used at least once or twice himself recently.

He also suggests that mechanisms other than negative feedback loops may play major roles in control.

I couldn't resist including this quote: "The study of alliesthesia (Cabanac, 1971) shows that it is a discrepancy between the actual value of a variable and a changed set-point [reference level], rather than the changed set-point in itself, that produces unpleasant feelings" (pp. 48-49). Of course it's nice to know that there is experimental evidence for this, but it seems so odd from a HCT perspective that the other interpretation could even be entertained.--Gary

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Date: Fri, 24 May 91 08:15:00 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Cloud 11 & rheostats

[From Bill Powers]

Gary Cziko (910424) --

Looks as though Mrovosky's book is a must-get. Also Mrovoksy. The name "rheostatis" is sort of OK, although that makes organisms into rheostats. Who turns the knob? I guess that's the problem you note with Mrovosky's conception of hierarchical control, so perhaps he chose a term consistent with the extent of his ideas about control theory.

Here we go again, though. All the concrete illustrations of control theory from life come from the lower levels of organization, where we can see variables being controlled, measure reference levels, and so on -- without fear of being made uncomfortable.

It would be nice to start collecting illustrations of higher-level control systems in the same way. Unfortunately, when we begin looking at behavior from cloud 11, the subject-matter becomes very hard to hold at arm's length: we start talking about topics uncomfortably close to where we live, and it's hard to find a platform from which to view them without identifying with them.

It isn't really hard to find material at the higher levels. What is hard is to stop BEING the control systems long enough to see them AS control systems. Maybe this is just too difficult; maybe we have to take the bottom-up approach, or confine ourselves to talking about other people as if we weren't examples of the same sorts of systems. If this is really true, then a complete control-system psychology is further in the future than I had hoped.

But it will still be interesting to do the possible.

Hello to Evelyn Satinoff.

I missed all of yesterday's posts as of about 7:30 MDT due to a computer glitch, so if anyone said all this already, I won't know it until tonight when Gary kindly sends me the replacements.

We have found a very fine place to live and have made an offer.

Best -- Bill Powers.

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Date: Fri, 24 May 91 10:04:13 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: Competence and performance

[from Joel Judd]

Martin Taylor (910523)

I reread what I wrote to see what I said backwards. I'm not sure but I think I may have emphasized the before/after sequence too much. What you said, however, is EXACTLY the point:

>But when I reflect on it further, I realize
>that my speech was right, and that Germans do use this strange construction.
>Would you say that my performance precedes my competence? I don't think
>you could say that in any meaningful way. But I do utilize aspects of
>L2 before I (consciously) perceive them.

THAT'S the argument, that we have a competence or set of them that underly production. It's like saying that we have a larger READING vocabulary than a WORKING one. And I was using "perception" in a non-CT sense. Your situation would be described by a man named Stephen Krashen as an example of "language acquisition," the unconscious learning of language, as opposed to "language learning," the conscious learning of phonology, syntax, etc. (ie in a classroom). This is another dichotomy I don't buy into (anymore).

Joel Judd
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Date: Fri, 24 May 91 10:04:17 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: 11th order, etc

[from Joel Judd]

Bill (910523)

Sorry to depress you by being so attached to my system concepts. I'm as interested in a CT psychology as anyone.

Rick (910523)

> System concepts are probably
>not controlled until a person is well into their teens.

Thanks for putting a time frame in print. Puberty is of course a magical critical/sensitive period for several disciplines, not the least of which is language.

About mysteriousness and rightness I'll have to think of another way to ask the questions. I understand your replies but my perceptions tell me they're not addressing exactly the things I wanted to know.

Joel Judd
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Date: Fri, 24 May 91 13:00:04 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Re: Personal uniqueness
In-Reply-To: Message of Thu, 23 May 91 13:36:54 cdt from <MCCLEL@GRIN1>

Dear Kent, FROM CHUCK TUCKER 24 May 1991

This statement about uniqueness is there to counter those who claim that each and every human being is so different that only a theory which dwells on that issue can possibly be useful for understanding human conduct. It also leads into a discussion of language and the other ways which humans use to get another to understand them. Finally, we use it to counter all of those who hold to the psychological theories which are so popular today.

It is probably not necessary for the understanding of control theory but it does seem to be a distrubance for most people.

Thanks for asking. Chuck

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Date: Fri, 24 May 91 12:40:25 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: The Defense of Variables

[from Gary Cziko]

Concerning the book I mentioned yesterday [Mrosovsky, Nicholas. (1990). _Rheostasis: The physiology of change_. New York: Oxford University Press.], I forgot to mention that Mrosovsky tends to use the world "defend" where we would say "control." So he talks about, for example, an organism "defending" its temperature against disturbances.

One of the problems with the world control, is that it is often confused by "ordinary" people with words like "influence," "affect," and "determine." I think "defense" avoids this problem, although it may drag along with it other unfavorable connotations.

But then again, if we called this thing "Hierarchical Defense Theory," think of the possibilities for government research funding, perhaps even enough to set up that "Hierarchical Defense Institute" that Bill Powers has dreamed about for so long.--Gary

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Champaign, Illinois 61820-6990
USA

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Date: Fri, 24 May 91 16:00:03 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Mary;Meeting;Mazes

From Tom Bourbon --

I have just emerged from the black hole known as the end of an academic year. And I just finished several hours of catching up with over a week of csg-l. The traffic on the net is heavy.

My enforced absence led to my missing several important items, not the least of which was the news that Mary Powers was in the hospital. Bill, I hope she is out and doing well. Your recent post about finding and making an offer on a nice place encourages me to think that the team is out and about. (Does the place have an out-building or two where you can house the institute?)

MEETING NEWS. The formal announcement and call for the meeting is in the hands of USPS, wending its way to Ed Ford, who will distribute it in the newslatter. It contains an elaborate schedule of fees, covering every major alternative I could imagine from among different arrival times, different sized parties, etc. I am sure someone will come up with a combination not in my schedule. If you are the one, just contact me and ask -- I will make up a solution.

Letters confirming places on the program for those who contacted me before yesterday will go out this weekend.

MAZES. From among the many threads and currents on the net during the past week, one passing remark leaped from the screen and stuck, more than any other. It came from Gary Cziko, who wrote it twice during discussions of Tolman's experiments with rats getting down mazes any way they could. (Actually, Tolman was not the only one to do such studies. You know psychologists, once the idea was out there, people were fine-slicing the possible combinations of maze design and animal handicapping every way imaginable -- and in some cases I do mean SLICING!). Gary, however, seems to have come up with a unique variation: twice he wrote of "having images of rats rolling through mazes in my mind." Now THAT is a novel place to put a maze, and a remarkable place to put a rat!

Best wishes,

Tom Bourbon <TBourbon@SFAustin.BitNet>
Dept. of Psychology
Stephen F. Austin State Univ.
Nacogdoches, TX 75962 Ph. (409)568-4402

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Date: Fri, 24 May 91 15:42:14 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Purpose

[From Rick Marken]

Martin Taylor (910523) writes:

>The purposefulness of a control system is in its feedback loop, but its
>purpose is in the reference signal. How's that for a distinction?

Perfect.

****

Richard S. Marken USMail: 10459 Holman Ave
The Aerospace Corporation Los Angeles, CA 90024
Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)

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Date: Fri, 24 May 91 19:04:31 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Back to cloud 11

[From Rick Marken]

I'm at home now so I can't edit. So just a quick note regarding Bill Powers' (910524) comment about the difficulty of dealing with higher level control from a perspective that let's us see it as control (and not something to protect -- since we are controlling for it). I am beginning to think that this little jaunt into cloud 11 is one of the most important excursions I've ever taken in my time with control theory. I do plenty of the level 5 on down control studies -- and I think we're all convinced that we can demonstate and account for the phenomenon of control at those levels rather well. I has to be considered one of Powers' most important insights that ALL behavior can, in principle, be handled by control theory. As I said in my forward to Bill's book. Bill didn't invent control theory -- but he noticed the appropriate way to apply it to living systems. He also noticed that all behavior -- from tensing muscles to defending principles, is control and, hence, can be accounted for by control theory. What could be more important to promoting the control theory view of human behavior than to show that system concepts, principles, programs, etc are controlled perceptual variables.

So, while I think it is certainly nice to have more and more evidence that variables like temperature, chemical concentration, force, or whatever are controlled -- it seems to me it could be monumentally more important to show that things like "atheism" or "humanitarian" or whatever other system concepts such words only point to, are actually controlled variables -- and show how they are controlled, how disturbances are resisted, etc.

I am happy to volunteer myself as a subject for this investigaiton. Perhaps Bill (or anyone else) could start testing for my controlled principles, programs, etc by introducing carefully selected disturbances.

Hasta Luego

Rick M

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Date: Fri, 24 May 91 22:31:20 -0500

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: Back to cloud 11

[from Gary Cziko]

Rick Marken says (910524):

>So, while I think it is certainly nice to have more and more
>evidence that variables like temperature, chemical concentration,
>force, or whatever are controlled -- it seems to me it could be
>monumentally more important to show that things like "atheism"
>or "humanitarian" or whatever other system concepts such words
>only point to, are actually controlled variables -- and show
>how they are controlled, how disturbances are resisted, etc.

I agree that this would an important advance for control theory, but there seem to be (to me, anyway) so many problems in demonstrating this convincingly.

One problem is that if we disturb your principles enough, you may change (reorganize) them and then we won't see you defending them any more. If we keep telling you how dumb you are, you may at first resist, but after a while you may re-organize your systems concept so that our comments no longer create any error. We can show control at lower levels because we can count on subjects to be nice and adopt the reference levels we give them. But I can't see this working for high-level reference levels, such as belief in God, etc.

There are also serious ethical problems raised by disturbing one's perceptions at the higher levels. Joel Judd had thought of giving students disturbing (inaccurate) test scores to see how they would react. Try getting that one pass the research review committees for human subjects!

But with all the smart people out there in CSGnet land (except for Rick Marken, of course), I suspect someone will come up with solutions to these problems.--Gary

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Date: Fri, 24 May 91 22:14:58 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Misc replies

[From Bill Powers]

Joel Judd, Martin Taylor (910524) --

There is a way in which performance can precede competence. Rick Marken touched on it when he was describing a way to make someone write a profanity during a two-dimensional tracking task, by applying a properly-patterned disturbance.

When a person is attending to a controlled variable and keeping it constant, the actions that counteract disturbances are not at the center of attention, but they can certainly be noticed. So you may find yourself performing some action, producing some pattern of actions, that strikes you as interesting, remember it, and try to produce it intentionally (i.e., when the action isn't being used to control some other variable). This is a neat way to "show someone the moves" for a new skill. Instead of having the person imitate the moves, and instead of trying to describe them in words, set up a controlled variable and apply disturbances such that the person can't control the variable without producing exactly the moves you mean.

This is really worth trying! Could you teach someone to write (the skill, not the understanding of what is written) by telling that person to hold a dot still, then applying a disturbance in the form of a written word upside down and backward? All the learner would have to do at first would be to keep the dot from moving. With the same disturbance applied over and over, as the person's skill in holding the dot still improved the action would come to mirror the disturbance more and more closely. The person would come closer and closer to producing the movements that constitute good handwriting without paying attention to them at all! Of course the final stage would be to tell the person to start paying attention to the hand movements, recording them to serve as reference signals, with the last step being to produce those movements without the disturbance acting.

I once fantasize getting up before the AAAS annual meeting, giving a two-minute summary of control theory, and then calling onstage my assistant, a chimpanzee who picks up a piece of chalk attached to one end of a rubber-band and writes his name on a blackboard. Now I'm wondering if that is really such a fantasy -- maybe you could teach the chimpanzee to write his name without the rubber band.

I don't know how you'd apply this to spoken language, but it's worth some creative thought. I have a feeling that something important is lurking here.

Joel Judd (910524b) --

Not depressed -- just went up a level.

Chuck Tucker (910524) --

Uniqueness: Hierarchical control theory tells us that we are all alike in the sense of having a hierarchical structure; I have proposed some generic classes into which levels in this structure might fall. But we differ from each other when it comes to which perceptions we control within a given class, and even in the lower-order components that make up perceptions that we call by the same names. There's a spectrum of alikeness. We all use essentially the same moves to drive a standard-shift car, but we certainly don't all drive cars the same way when it comes to competitiveness, fuel economy, tire wear, accidents per mile,

and so on. At the lowest levels we are more alike than at the highest.

Repeating the book reference, as well as I can remember it: the title is Feedback Thought in Social Science. Author is George Richardson. Publisher is University of Pennsylvania press. Year is 1991 -- very recent. Rick has a copy -- maybe he can supply the ISBN number.

Rick Marken (910524b) --

>I believe the control model would say that different people want to
>perceive themselves as "christians" or "jews" or "nazis" or "communists"
>or "pacifists" because of differences between these people in terms of
>11th order reference signals.

To be more precise, it's because of differences in 11th order input functions. At any level, it's the input function that determines what function of which lower-level signals is to amount to a perception. The perceptual signals are just signals that get bigger or smaller. If you stuck an electrode on the signal, it would look like any other neural signal, no matter what it means. Same for reference signals: they just say "this much," not this much of what. The "what" is given by the form of the input function and which control system you're talking about. I am not at all satisfied with this aspect of the model, because it doesn't seem to capture the quality of perceptions. On the other hand, when you focus on any one perception very closely, it starts to seem like "just a signal" and to lose a lot of its meaning. Anyway, good or bad, this is how the model is presently designed.

People get a "Christian-ness signal" from all sorts of different lower-order perceptions, don't they? The perceptions that contribute to Jerry Falwell's Christianness are certainly different from those that contribute to the Pope's Christianness. It's very confusing when people use the same words for perceptions that are different. But they have to -- there are more perceptions than words.

Martin Taylor (910524) --

>I can see all the lower level control systems as being imbued with
>purpose by the reference signals that direct them, but I can't see an
>infinite regress, which would be required if there is a "deep"
>purposefulness. Either one has a simple closed dynamical system at the
>top level -- "simple" in that it is not a control system with an
>externally supplied reference -- or the top level has a magically
>supplied external reference -- God-supplied. I prefer the former view,
>but both seem to be outside the purview of CT.

The top level of reference signal is always a problem, even when you're considering development, when the top level can be ANY level, depending on the stage. I'm not sure that development goes strictly bottom-up, but clearly there are times during a child's growth when (for example) principles are few and poorly understood. So the principle level really isn't in shape to provide reference signals for the program level. Where do those signals come from?

One idea: I have proposed that reference signals come from memory, with the addressing being the function of the higher-level output signal. This would probably apply mostly to the higher-level systems. The stored

memories are recordings of past values of the perceptual signal in the same system. With this scheme we can make up some interesting stories. For example, suppose that addressing of memory occurs in the time dimension. The higher system's signal, in effect, points to a time at which a recording was made, and that recording is then replayed as the effective reference signal. A clever designer would arrange the relationship between the address-signal and time so that ZERO signal means "right now." Thus once the recording facilities come into action, the absence of a higher level output signal means that the organism simply wants to create the perception that has just occurred. If the organism has had NO experiences to record, the effective reference signal is zero -- which leads to avoiding a new perception. I suppose that this would fit some kinds of examples.

I think we simply have to leave open the question of where the currently-highest-level reference signals come from. We don't have any data on this subject that we can interpret.

There's a word problem with "stability." When Marken and Bourbon and I and others use it, it means only "dynamic stability" -- not persistence of organization through time. It means that the system has the proper dynamic characteristics so it doesn't burst into spontaneous oscillations and thus destroy its own ability to control. The question of dynamic stability only comes up in connection with pathological situations -- bad organization or conflict. The living systems we are concerned with seldom show any signs of dynamic instability, except under temporary conditions of large disturbance or during reorganization.

>If there is a "soul" in such a system, it will be found in the dynamics
>that provide the reference signals for the highest level control
>systems.

The nearest translation of "soul" that I can come up with is "awareness." The problem with assigning awareness to just the highest-level systems is that observation doesn't bear this proposition out. At least in the hierarchy that I have proposed, the perceptions that occur at any level may be awared or not. We know that if a control process (for example, positional control at the elbow joint) works properly, all the required perceptual signals must be generated by the appropriate input functions at that level. But we can be aware of very carefully putting an egg down on a hard surface, which certainly requires control at the elbow joint, without any awareness of elbow angle. It's easy to think of dozens of examples of this phenomenon: a control action that requires presence of a perceptual signal, without any awareness of that perceptual signal. And of course in all such examples, it is perfectly possible to BECOME aware of the same signal. So awareness is not synonymous with "presence of a perceptual signal." Nor is it tied to just one level.

My own bet is that awareness is connected with the reorganizing system, and is not a function of ANY level of the learned hierarchy of control systems, highest or otherwise. That still doesn't tell me what it is.

> ... the sole (soul?) purpose that exists at the top is a post-hoc
>reflection of the fact that the organism is the product of successful
>evolution; it is constructed to survive long enough to reproduce.

Surviving long enough to reproduce is obviously a requirement on species

survival -- the species must reproduce to survive. But I doubt that it is sufficient to account for all evolutionary changes. If anyone wants to, we can do a reprise on the control-theory additions to the theory of evolution. Maybe somebody else could summarize what we went through in earlier posts (last year) for the sake of those who missed it.

I don't know how to connect "soul" or "awareness" with these ideas on evolution. But it's clear to me as a modeller that I've never succeeded in capturing this whatchamacallit that makes each of us the center of the universe. I have never built a control system with anything in it that could be called awareness. And I don't think that the systems I have built contained anything like what I experience as consciousness or awareness.

This is a case where we have to be very careful to put phenomena before theory. Awareness so universally shared as to amount to a publicly-observable phenomenon. No model ever proposed has been able to imitate this phenomenon. It would be all too easy to say that if the models don't contain it, it doesn't exist -- which would mean that you're not conscious, I'm not conscious, nobody is conscious. And that simply contradicts experience, the certain mark of a wrong proposition.

Chuck Tucker (910524b) --

Re "Another reason for religion:" I think the reason for religion is that people want to explain their experiences to themselves. In many fields, such as explaining how the universe got here, science does a far better job. In other important fields, science has abdicated, leaving religious theories the only ones widely discussed or believed. It's not very convincing to a religious person to be told " It's all in your imagination" by someone who can't explain imagination, either.

Somehow I don't think that Max Weber or Kenneth Boulding got very close to the actual problems involved.

Re the title: It's a colon. Perhaps I accidentally called it a semicolon because of the nature of Mary's recent problems.

Joel Judd (910524b) --

I said
>especially if there are THINGS I REALLY SHOULD BE DOING but don't want
>to do, but if I do so I won't be talking about control theory.

You said

>I appreciate the clause (which I emphasized).

Let me guess: you think that if I really should be unpacking more books, and procrastinate by indulging in arguments like these, I am being nudged by some Power greater than me. I think it's just a conflict caused by wanting to do two incompatible things -- and both of the Powers that are telling me to do these things are named William T. I resolve this conflict, by the way, by using sequencing. First I do one thing, then the other. I thought of that all by myself -- with a little help from my ancestors.
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Date: Fri, 24 May 91 22:36:16 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: The Test

[From bill Powers]

Rick Marken (910525z or so) --

>I am happy to volunteer myself as a subject for this investigaiton.
>Perhaps Bill (or anyone else) could start testing for my controlled
>principles, programs, etc by introducing carefully selected
>disturbances.

What do you think I have been doing?

Gary Cziko (910524b) --

>One problem is that if we disturb your principles enough, you may change
>(reorganize) them and then we won't see you defending them any more. If
>we keep telling you how dumb you are, you may at first resist, but after
>a while you may re-organize your systems concept so that our comments no
>longer create any error. We can show control at lower levels because we
>can count on subjects to be nice and adopt the reference levels we give
>them. But I can't see this working for high-level reference levels,
>such as belief in God, etc.

The test for the controlled variable doesn't require disturbances so large that they destroy control. All you need is a disturbance large enough to call forth an opposing (successful) effort that can be observed. If the opposing effort succeeds, there won't be enough error for long enough to produce significant reorganization. You won't change a person's principles or system concepts by pushing on them a little. But you will find out a lot about what the person will resist and what the person will let pass.

"Disturbing" a controlled variable doesn't mean pushing hard enough to cause it to change. It just means pushing hard enough to elicit an opposing effort that cancels the disturbance as far as the controlling person is concerned. If you use a large enough disturbance to succeed in overcoming the opposition, the result is, as you say, likely to be reorganization, and you won't be observing the same system any more. But that isn't how the Test is used.

There aren't any ethical problems in using the Test correctly. You don't actually change anything that matters to the person. The only cost to the person is a little effort to oppose the disturbance. This means, of course, that you must choose your disturbances so they CAN be resisted successfully.

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Date: Sat, 25 May 91 05:15:00 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Dag Forssell <0004742580@MCIMAIL.COM>
Subject: MCI MAIL

Thanks for two notes, Bill. Both addresses worked.

For specifics on MCI mail, call 1-800-444-MAIL

I am using Procomm Plus 2.0 This is a 1991 release which is much easier to use than the older shareware. I paid $85 for my package.ee

Hope this is helpful.

Dag Forssell

Rich Marken:

Now that I understand your comment as a question, I shall attempt an answer.

Very briefly, Bill Glasser used / developed Reality Therapy 30 years back. His Book by the same name is still available in your local book store. He developed an institute and a large following, numbering in the thousands. He has written many books which show his deep interest in matters human. See Positive Addiction and The Identity Society. He was told about our 1973 bible and attempted to write a version more accessible to the public.

I was fascinated by "Stations of the Mind", but then I am an engineer. The book does a credible job as far as I remember. He gives proper credit to Bill.

Many of Bill Glassers senior faculty still go by that book, which is why some are in CSG

The book was probably not a hit with the public and apparently not with most of Bill's followers. Bill developed a four color chart to teach by, which is simplified but not bad.

Clearly, Reality Therapy came first. Control theory failed to support it as Bill Glasser anticipated, since he could not teach it in a way that his audience accepted. Problems of organization control may have contributed to the break with Powers.

It seems to me that Bill Glasser is smart and has made contrlbutions in many ways. Reality Therapy is his baby and dominant systems concept. It comes first. Intellectual honesty nith regards to CT comes second. The book "Control Theory" provides the following definition of CT: CT contends that every behavior is a persons best attempt to meet his needs" (Approx, from memory). Perception is out the window because it was confusing to his audience.

This is a quick sketch of my perceptions on this.

As is obvious from the 11th level thread, we all have different content in our systems concepts. Glassers priorities are different from ours. He has still brought a number of CT faithfuls to our fold through his promotions efforts. I am glad that I am one of them.

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Date: Sat, 25 May 91 11:28:47 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: none

REVIEW CSG-L

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Date: Sat, 25 May 91 11:52:36 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Reality Therapy

[From Rick Marken]

Dag Forssell (910525)

Thanks for the thumbnail sketch of the history of reality therapy and its relationship to CSG. Actually, I am fairly familiar with that history. I actually went to a Bill Glasser show in about 1981 when I was in Minnesota. When I found out he was interested in Powers I went right up to Bemidji or whereever he was. I even had lunch with him in the regal dining room -- he invited me in when I told him I had been working on control theory for a couple of years already. He struck me as a consummately self-absorbed individual. Not at all intellectually interesting. I still don't really understand the basis for the rift (if that's what it was) between he and Powers though I would be that it had much more to do with Glasser's rather shallow grasp of Powers' model than with any conflict over control of any organization (the very notion of Bill Powers trying to control some organization is pretty silly given when Bill Powers is like -- I think Glasser was trying to get credit and, possibly finacial gain, where none was due, but I'll leave that to Bill to comment on if he wants).

Anyway, I still don't understand what "reality" therpy has to do with control of PERCEPTION. I kind of ragged on Ed Ford for not explaining to my satisfaction why it is important to realize that people are controlling perceptions, but I think I react so strongly because it is so omportant to me that this be made clear. NOBODY has direct access to reality. We control only representations of what reality might be. To the extent that people can see agreement regarding what they perceive then we tend to call those perceptions reality -- but they ain't. They are just shared (somewhat) perceptions. Indeed, I think using the term "reality" in a theraputic situation could actually be dangerous -- giving the therapee the impression that there is a right way to perceive things. My pedagogical point was that, when explaining control, just leave out the term perception AT FIRST. I control the letters on the screen, the position of my hands, etc etc. Once a personn understands that

there are variables "out there" that are controlled (many different letters could be types, many different hand positions are possible) and that these variables are brought to reference levels though the action of lower order acts that may also counter the effects of disturbance to these variables -- then the person understands the phenomenon of control. Then you can explain that it is percetual aspects of experience that are being controlled -- and that there are, therefore, different perceptual aspects of the same experience that can be controlled.

I just did a review of CSGNet and I see that my clinician friends are still listed. So again I ask -- really, just out of curiosity, not hostility -- why would a person who developed "reality therapy" see perceptual control theory (still like that description) as something that would support their theory. What is "reality therapy" anyway.

Bill Powers and Gary Cziko (910524) -- keep testing away. Whn I get the change maybe I'll write a description of my own hierarchy and you can compare it to your test results.

Best regards

Rick

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Date: Sat, 25 May 91 14:32:17 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Powers's Dissatisfaction

[from Gary Cziko]

Bill Powers (910524a):

You surprised me by saying:

>At any level, it's the input function that determines what
>function of which lower-level signals is to amount to a perception. The
>perceptual signals are just signals that get bigger or smaller. If you
>stuck an electrode on the signal, it would look like any other neural
>signal, no matter what it means. Same for reference signals: they just
>say "this much," not this much of what. The "what" is given by the form
>of the input function and which control system you're talking about. I am
>not at all satisfied with this aspect of the model, because it doesn't
>seem to capture the quality of perceptions.

But I had thought that the the whole purpose of a hierarchical perceptual system was to make just such reductions to a single signal possible. This was, for me, one of the hardest parts of the theory to understand. But now that I think I understood it, you say you don't think quality can be turned into quantity!

Is there some way that hierarchical control theory (HCT) can be made to work without reducing complex perceptions into single signals somewhere in the hierarchy?

Perhaps this would be a good time for someone on the network to give us a little lesson on what the neurophysiology says about this issue. I know a bit about the functioning of complex and hypercomplex cells in visual perception, but is there other evidence that other types of complex perceptions correspond somewhere to a single neural signal?--Gary

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Date: Sat, 25 May 91 15:59:20 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: The Test; Quality of perception

[From Bill Powers]

Gary Cziko (910525) --

>But I had thought that the the whole purpose of a hierarchical
>perceptual system was to make just such reductions to a single signal
>possible. This was, for me, one of the hardest parts of the theory to
>understand. But now that I think I understood it, you say you don't
>think quality can be turned into quantity!

I still think that perceptions are reduced to single signals. This idea fits all kinds of circumstances and is still my theory of choice. But I have trouble making my own perceptions look that way. Blue and green still look like different colors to me -- not just different signals. With enough work I can narrow down on a single perception, and make it lose that funny "quality" thing -- but it's hard enough to do that I wonder if I'm not just talking myself into something.

We could say in a general fuzzy sort of way that signals acquire this "quality" thing when taken in the context of other signals. That would explain why focusing on one perception to the exclusion of all others makes it lose this quality attribute. What it doesn't explain is how I would model a system handling perceptual signals so that the quality attribute appeared somewhere in it as a signal. Maybe I've just reached the age where I don't understand some things that I used to understand. But maybe there's some concept in here that I'm just missing. How about explaining it to me? Maybe the solsution is more obvious to you than to me.

Rick Marken (910525) --

I was mostly kidding -- no subtle mastermind experiments here. But if you look over the posts for challenge and response, you will see pretty well what perceptions various people are controlling for. If you're going to maintain your position on any subject, in the face of mild disturbances, it's pretty hard to conceal the controlled perception. This is most useful when you do it with your own responses to challenges. For example,

if somebody's feelings get hurt, does one always have to apologize?
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Date: Mon, 27 May 91 03:18:50 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject: neural representation of signals

[Joe Lubin (052691)]

Gary Cziko (052591) asks:
>> Is there some way that hierarchical control theory (HCT) can be
>> made to work without reducing complex perceptions into single
>> signals somewhere in the hierarchy?
>>
>> Perhaps this would be a good time for someone on the network
>> to give us a little lesson on what the neurophysiology says about
>> this issue.

and Bill Powers (052591) responds:
> I still think that perceptions are reduced to single signals.
> This idea fits all kinds of circumstances and is still my theory
> of choice.

What is it that you two mean by "single signal?" This term is ambiguous with respect to the myriad signaling systems in the brain. There is much that a single neuron can compute. Shepherd and Koch provide a nice perspective on this using a "soft logic" metaphor in Shepherd's _The Synaptic Organization of the Brain_, 3rd Edition. For example, in a cortical pyramidal cell, the spines on the cell's dendrites contribute to the computation of "logical clauses" based on inhibitory and excitatory inputs, say, of the form

(((spine1 & spine2) or spine3) & !spine4).

Inhibitory signals impinging directly on the dendrite (near a branch point) can produce a global veto of the excitatory aspects of the clause. This is called dendritic computation.

At a lower level, there is a similar fuzzy logic in the interactions of the various currents to which a cell's membrane potential is subject. The various K+, Na+ and Ca++ currents combine in a single cell to produce changes in the probability of an action potential. These currents are each mediated by a number of factors including multiple neurotransmitter substances. The membrane potential can be brought closer to threshold by slow, sustained currents, or by fast, transient currents; it can be inhibited similarly by sustained or transient processes. So, a number of factors can combine to propagate a signal (by creating an action potential or burst of action potentials) or a single overwhelming factor can cause a singalling event.

At least two levels of computation lie between the level of "transmitters and currents" and the level of "dendritic computation." The level above the dendritic level, the "neuronal" level, accomplishes an integration of the dendritic computations, transmission (including fan-in and fan-out) of

the resulting signals, and other more metabolic functions which sustain the neuron's signalling capabilities. Above this, at the level of "local circuits" is where these comments start to pertain to the question at hand.

Although local circuits are often characterized as generic operations on sensory signals, (eg., lateral inhibition in retina or olfactory bulb) or rhythm generators (lobster stomatogastric circuit), this is where neuronal signals appear to take on sematics that we can begin to understand in behavioral terms (eg, mach bands, categorical perception). Realize however, that these local circuits are still subsymbolic -- they are only small facets of what may be described as a "field computation."

This, I believe, is where our answer lies. I have signed up to present at Durango what I feel to be a strong start in grounding Perceptual Control Theory (PCT) in theoretical neuroscience. From my intermittent reading of this group, and from reading a few of the recent papers disseminated herein, I have found strong resonance between the structure and ideas of PCT and the formalisms that I use in modeling neural circuits. This work is in progress -- I am waiting for [Powers 1973] to arrive.

This digression was meant to introduce the level at which I work, and the level at which I believe behavioral signals are to be found: Behaviorally relevant neural signals are spatiotemporal electrical fluctuations over assemblies of neurons. There are several possibilties concerning the functional significance of the temporal element of these signals, but as yet nobody has any clear-cut notions. So my statements boil down to this: Perceptions are indeed "reduced to single signals." These single signals are represented as spatial patterns across functional units which are, most likely, assemblies of neurons.

In Durango, Level 11 willing, I'll try to demonstrate how such assemblies can

(i) encode different signals within a single substrate,
(ii) represent reference levels,
(iii) represent afferent (sensory) data,
(iv) match or mismatch the reference and efferent signals and generate behaviors specific to these two contigencies,
(v) be organized into a hierarchy of representational levels, each acting as a reference signal for the lower-level signals which impinge on it, and even
(vi) reorganize the network in light of a persistent predictive failure (a failure of the evoked behavior to control or maintain the expected perception),

all within a self-organizing (learning), neurally-derived framework.

P.S.
In regard to the question about the simple, complex and hypercomplex hierarchy in visual cortex, its important not to be led into "the strong grandmother cell hypothesis." It is indeed essential to abstract properties out of lower-level

descriptions by performing integrative computations over
time or space or some other dimension (eg. edge detectors,
motion detectors, color cells, face cells, hand cells), but
we must realize that all of these abstracted representations
still exist within fields of similar abstracting elements (other
cells) and therein lies their semantics. It is the FIELD of
signals that can reliably represent behavioral signals in a
complex and noisy environment and which can be reliably
manipulated or computed with.

The best candidates yet for a "grandmother cell" are the
face and hand cells of inferior temporal cortex. There are two
strong reasons why even these cannot be local encodings. First,
they have been found: in an area of maybe a million cells how
likely would it be that experimenters could find a number of
these cells over a few recording sessions? Secondly, the
response records of many of these cells appear to either follow a
certain parameter (eg. degree of head turn) or are simply
too broad to be a local encoding.

Even for the best candidate for the title "single signal" (in the
grandmother cell sense), it is clear that the underlying
representation must be more field-like than single unit.

Joseph Lubin jmlubin@phoenix.princeton.edu
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Princeton University 609-258-4598
Princeton NJ 08544

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Date: Mon, 27 May 91 10:46:55 BRA
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Flavio Codeco <IBG10005@UFRJ.BITNET>
Subject: papers

Dear list members:

I'm new to this list and I'm getting very interested by the control theory,
but here in Brazil(where I live), it's impossible for me to find papers on
this issue, and I'd be very glad if any of you could send me some basic
papers about control theory and other related topics by email or surface mail.

thanks in advance,

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ê ê ! IBG10005@UFRJ.BITNET
0 0 Don't ! Flavio Codeco Coelho
> worry, ! rua da Passagem 71/802 Botafogo
___/ be happy!! ! zip:22290
! Rio de Janeiro - RJ
! Brasil

TEL:(021) 542-3225
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Date: Mon, 27 May 91 11:33:36 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Reply to Flavio Codeco

Dear Flavio Codeco:

You should know that you and Claudia Torres have the honor of being our first (known) CSGnet participant from South America. This means that CSGnet now spands North America, South America, Europe, Asia, and Australia. By my reckoning, that leaves only Antarctica out of the network. (I realize that many of our participants may not care much about this, but I know that it makes Bill Powers feel good.)

I can send you five items electonically:

1. Bill Powers's introduction to control theory
2. Bill Powers's control theory manifesto
3. Information about books on control theory
4. Rick Marken's paper on perception
5. David Goldstein's paper on clinical applications
6. Kent McClelland's paper on "social control"

I will start with 1 and 2. When you receive these and want more, contact me at my personal address and I will send the others that you wish to have (please give more than just the number since I will have forgotten by then how I listed them)

Other CSGnet participants who have other things to send you should do so. It would help if you would give us some information about yourself and your control theory interests.

--Gary

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Date: Mon, 27 May 91 09:46:29 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Time scales

I am just starting to read Bill Powers' 1973 book for the first time, and in talking about time-scales of response (page 54) I come across the following quote:
"Psychologists who believe that intermittent reinforcement is more effective than continuous reinforcement should give this whole speed-of-reaction problem serious thought--for a long enough time."

I realize that this was written a long time ago, and may be amended even later in the same book, but it does resonate with some of the threads that have been weaving through this mailing list--statistics, in particular. So although it may be unfair, I will comment.

Intermittent reinforcement is not usually seen as "more effective" but as more resistant to extinction. And a statistical reason is not hard to find. In the laboratory, the animal is confronted with a situation in which it is sometimes rewarded for behaviour A, but never for behaviour B (or less often, perhaps). Now if the experimenter decides to stop rewarding behaviour A, how can the animal know that the world has changed its rules? Previously, failure of reward for A has been followed by further reward on a later occasion. It cannot know that this will no longer be true. Only by implicitly evaluating the statistics of the reinforcing event can it determine after a while that a long period of non-reinforcement would have been unlikely under the regime to which it had become accustomed. If you like, there is "continuous" higher-order event--a statistical event--which occurs on a time-scale much longer than that of the single reinforcement.

In such an experiment, the experimenter tries to make sure that the animal has no access to information that might let it know which rule is in effect. Many experiments have been found to give results that depended on the animal hearing a click or something that the experimenter had not noticed, but that occurred only when reinforcement was going to be provided. The animal then has a context that turns the statistical event into a predictable event. It can know that the world has changed, if it no longer hears the click.

It should be much easier to learn a behaviour that has a perfectly predictable consequence, but normally we do not have access to all the factors that influence the consequences of our behaviour, so we have to resort to statistics to determine how our behaviour is influencing our perception. The control system may be fully determined in its behaviour, but if we cannot tell the difference between a context in which behaviour A leads to result P and one in which it leads to result Q, then all we can do is to take advantage of the best information we have; that is, for example, that A->P has happened 75% of the time we did A, and A->Q 25%. If we want P to happen, and it is not too bad if Q happens instead, then we would do A. But if Q would on this occasion be disastrous, we might try another way of getting P to happen rather than risking behaviour A.

Life, even in a control system view, is a statistical game.

Sorry if that's all too obvious to have been mentioned, but I have read so much trashing of statistics on this list that it seemed rather to be so obvious as to have been overlooked.

Martin Taylor

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Date: Mon, 27 May 91 11:52:24 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Reply to Lubin

[from Gary Cziko]

Joe Lubin (910527)

Thanks so much for your post on neural signals. I will will need to study this for a while before I can even begin to respond to it intelligently and I look forward to hearing your presentation at Durango. I suspect that Bill Powers will have some reaction before then.

I see that you are in civil engineering at Princeton. I had no idea that civil engineers dealt with neurons and dendrites. Somehow I got they idea that they built bridges, tunnels and stadiums. My goodness, are we psychologists going to have to put up with yet ANOTHER engineer who wants to do psychology?--Gary

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Date: Mon, 27 May 91 11:05:38 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Neural nets; Welcom Cuelho

[From Bill Powers]

Joseph Lubin (910527) --

you ask

>What is it that you two mean by "single signal?" This term is ambiguous
>with respect to the myriad signaling systems in the brain.

The "single signal" principle says that one controllable variable corresponds to one neural signal (rather than, say, a matrix of signals in which the variable is only implicit). A single signal is either one signal coming out of one neurone, or a set of parallel signals all of which covary with the same variable (as in length-signals from the stretch receptors in a single whole muscle).

This is a little like a "grandmother cell" hypothesis, I admit. But in the context of a multiple-level, multiply parallel model, a "grandmother" can exist at many levels. Only at a rather high level could a signal appear that indicates presence of "my grandmother" or "grandmothers." At that level, of course, only one signal is necessary because grandmother's attributes are multiple signals at lower levels. Grandmother's face, for example, could be represented as a single signal, while all the detailed sensations that make up various aspects of that face exist as hundreds of signals at the level of sensations. The configuration-level input function that receives those sensation-signals would combine them according to some weighting scheme (or more probably a scheme based on differential geometry or tensors or something else that is, regrettably, beyond me). The signal emitted by this input function would indicate the degree to which the set of sensation signals resembles grandmother -- or we could say it represents the projection of the sensation vector onto a

vector defined by the weightings, with the maximum signal being produced by a particular direction of the vector. So the grandmother's-face cell really doesn't compute grandmother's face all by itself -- there are many layers of computation with multiple variables beneath it in the hierarchy. And all that the single output signal has to represent is the FACT that grandmother's face is present to some degree. It doesn't have to resemble grandmother's face. We know what a particular face is " a little like grandmother" simply because we get a little bit of signal out of the grandmother-recognizing input function.

One reason I think that the single-signal hypothesis is important is that we must somehow tie the neural signals coming out of perceptual computers to the way the world appears to us. Perceptions of objects and relationships and so on in the apparent world seem unitary. This appearance seems most compatible with the single-signal idea. If you have a better idea, though, I will be listening.

Concerning dendritic computation, you say

>Shepherd and Koch provide a nice perspective on this using a "soft
>logic" metaphor in Shepherd's _The Synaptic Organization of the Brain_,
>3rd Edition. For example, in a cortical pyramidal cell, the spines on
>the cell's dendrites contribute to the computation of "logical clauses"
>based on inhibitory and excitatory inputs, say, of the form
>(((spine1 & spine2) or spine3) & !spine4).

The concept of dendritic computation is fine -- I've always assumed it. But it surprises me that modern authors would still be thinking of these computations as logical in nature, rather than analog. Fuzzy logic is only a compromise that attempts to retain the symbolic-logic format while allowing for a bit of continuous variation to creep in -- and it still seems to assume that a single output spike has meaning. When you get my 1973 book, you'll see (in Chapter 2, I think) a very elementary attempt to show how neural connections can be used to provide basic analog computing elements, when the fundamental information-carrying medium is assumed to be frequency of firing rather than single impulses. Of course a more sophisticated treatment of these computing elements would result if we take into account the details of neurochemistry in dendrites, including effects of placement of synapses on the soma, but I think that the same basic idea will still be defensible: that output frequency is a function of multiple input frequencies -- and that single impulses do NOT signify logical computations. Maybe it's different at the level I call "programs", but maybe not.

So the sort of dendritic computation I visualize is more like

(Ax1 + Bx2) * Cx3 ...

... where the x's are continuous variables measured as frequencies and the coefficients represent strengths of synaptic effects (inhibitory connections are just negative coefficients).

Where you say

>At a lower level, there is a similar fuzzy logic in the interactions of
>the various currents to which a cell's membrane potential is subject.
>The various K+, Na+ and Ca++ currents combine in a single cell to

>produce changes in the probability of an action potential.

... rather than saying "probability of an action potential" I would simply say "action potential." In a given neuron there is always a specific action potential that varies as a function of these currents, which in turn vary as a function of the frequency of incoming impulses. You can always say that a variable action potential has a calculable probability of being at a certain magnitude, but isn't it easier just to say that it has a certain magnitude at every moment? Then the neuron becomes a voltage-controlled oscillator. The processes of diffusion and metabolism combine with incoming jolts of neurotransmitter to smooth the discrete impulses into continuous changes of action potential, with excitatory inputs lowering the bias and inhibitory contributions raising it. Excitation and inhibition are just the positive and negative signs in these continuous computations. The net action potential determines the frequency of firing of the neurone, according to a reasonably linear curve over a good part of the range. "Electrical" synapses are, of course, a special case, but not the predominant case.

>The level above the dendritic level, the "neuronal" level, accomplishes
>an integration of the dendritic computations, transmission (including
>fan-in and fan-out) of the resulting signals, and other more metabolic
>functions which sustain the neuron's signalling capabilities.

This is the only level I considered in Chapter 2, because I didn't know enough about the more detailed level (and I had a hard enough time getting this "highly technical" material past my editor). The "single signal" idea applies at this level: the output signal of a neuron (considered as a variable frequency) is a function of multiple input signals (considered the same way).

The next level of perceptual computational unit (not connected with levels of perception) I consider to be the sensory nucleus. This relates to

>Although local circuits are often characterized as generic operations on
>sensory signals, (eg., lateral inhibition in retina or olfactory bulb)
>or rhythm generators (lobster stomatogastric circuit), this is where
>neuronal signals appear to take on sematics that we can begin to
>understand in behavioral terms (eg, mach bands, categorical perception).
>Realize however, that these local circuits are still subsymbolic -- they
>are only small facets of what may be described as a "field computation."

When I draw diagrams of the hierarchy, I organize it into complete control-system units, which is a topological transform of the actual arrangement. Each control system has an input function that receives multiple inputs, the same ones, in many cases, that reach the input functions of other control systems at the same level. The actual arrangement of input functions is that sensory nuclei receive multiple input signals and emit multiple output signals. All the input functions at a given level (related to a given sensory nucleus) are actually lumped together into nuclei rather than being spread out one per control system.

Within the sensory nucleus there are all sorts of interactions, crudely represented as "mutual inhibition" in some literature discussions, but I am sure encompassing far more than that -- more like the "field computations" you mention. What I have done is to treat each output

signal from a whole nucleus as a separate function of the multiple inputs, which is legitimate if all the interactions are properly accounted for but which does conceal any relationships among output signals of the same level imposed by the interactions. If you look up my Byte Magazine articles from 1979, you'll see (in part three) both representations of the hierarchy: the "real" arrangement, and my rearrangement into control systems. I think you'll agree that my control-system arrangement is equivalent to the other, and you'll also see that it would be better to take Nature's arrangement instead, if we can figure out a way to handle it. I couldn't, but I suspect that you are getting farther with this approach than I ever could hope to do.

finally --

>This, I believe, is where our answer lies. I have signed up to present
>at Durango what I feel to be a strong start in grounding Perceptual
>Control Theory (PCT) in theoretical neuroscience. From my intermittent
>reading of this group, and from reading a few of the recent papers
>disseminated herein, I have found strong resonance between the structure
>and ideas of PCT and the formalisms that I use in modeling neural
>circuits. This work is in progress -- I am waiting for [Powers 1973] to
>arrive.

I agree that this is where our answer most probably lies and I look forward in happy anticipation to your presentation at our meeting. I have always hoped to attract to HCT or PCT (or whatever the hell it is that we do) people with a talent for theoretical neuroscience. We have been stuck for a long time in trying to extend our modeling efforts to higher-level systems because we don't know how to model higher perceptual functions. This is clearly a problem that needs a broader perspective and new skills. There will be close attention to your words!

I have a general piece of advice concerning your presentation (intended also for others who will be presenting). We try not to run our meetings like most other scientific meetings, where the experience of listening to papers is much like watching television. Once in a while a full-blown worked-out presentation is OK, and it's pretty much up to the presenter to decide, but the best presentations are those that take about 10 or 20 minutes and then become a free-for-all discussion for the rest of the allotted time. We don't care much for papers that sound like they're trying to defend against every conceivable criticism, and thus get into all kinds of details that are really unimportant before a receptive audience. We aren't a PhD committee; we're not competitive (usually), and we're mainly there to learn something. And don't forget that we have afternoons off, which is the time when the people with deep interests in a given subject get together as they please and go at it in any depth they like (while sitting around, hiking, playing volleyball, or whatever). The whole idea is informality and interactions among friends and peers. That's why people keep coming back year after year.

Also remember that all sessions are plenary. Only a fraction of the audience will have expertise relating to a given presentation. Everyone benefits from hearing what is going on in other fields of interest; this helps blur the distinctions between disciplines, which is one of my ambitions and is shared by many others in the group. Everyone, therefore, will be listening with interest but not necessarily with full understanding. The discussions greatly broaden the amount of

understanding, where a formal paper is almost designed to keep outsiders from catching on. Also, I think that such broadening helps to keep theoreticians honest.

In our first few meetings we took 20 minutes of the first day to lay out the schedule on a blackboard where everyone could see how much time was available and could ask for slots with knowledge of how many people wanted to present. This time it's more formal, but I think that the same spirit could prevail. The discussions are what make the meetings work; the presentations are only the framework. I plan, of course, to participate in the discussions full-bore, but I'm not presenting anything in the way of a paper. I'll have my computer there, but not much new because this last year has been rather full of distractions, none of which I minded but which didn't allow enough time for making enough mistakes to end up with anything interesting to show for it. Thank goodness for CSGnet.

Flavio Codeco Coelho (910527) --

Welcome to CSGnet. Perhaps Gary Cziko will transmit to you a copy of the bibliography compiled by one of our members, Greg Williams. Others with a handy Xerox machine and a departmental budget may be willing to send you some papers. Our group is basically unfunded.

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Date: Mon, 27 May 91 12:11:16 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Probability and Extinction

[From Bill Powers]

Martin Taylor (910527) --

Everyone seems to be sitting in front of a computer on this Memorial Day. Well, so am I.

Martin, you say

>Intermittent reinforcement is not usually seen as "more effective" but
>as more resistant to extinction. And a statistical reason is not hard
>to find.

I agree in both regards. I was thinking in terms of "habit strength" and in terms of Skinner's "shaping" experiments when I said "more effective." Both are related to extinction. (Skinner found that by changing the schedule so as to deliver fewer reinforcements for the same behavior, he could INCREASE the rate of responding. He cited this as an instance of the power of intermittent reinforcement, never realizing that this relationship was the opposite of the one he always assumed to hold between reinforcement rate and behavior rate.)

As to the statistical reason, there are many cases in which a statistical analysis comes out with the same results as a modeling analysis without statistics. Suppose that an animal has learned to perceive the rate at which some almost-rhythmic stimulus appears. Representation of this rate

as a neural signal (by analog means) would require a smoothed frequency detector. The smoothing is required to eliminate the individual instances of an input and produce a signal whose magnitude is proportional to the rate of appearance. The amount of smoothing used determines the range of input frequencies over which the signal magnitude usefully indicates input frequency (too long a smoothing time yields maximum signal for all rates above a certain limit). Within the range of operation, the signal magnitude corresponds roughly to the probability that an input will occur within a given time inverval, related to the smoothing time. So the analog perceptual function can accomplish the same end as a probability calculation, but in a quite simple way. If we were choosing on the basis of simpklicity of circuitry, I would pick the analog method. Of course we must ultimately pick the method that the nervous system actually uses.

Given the smoothing time, it will take a certain number of input events to bring the perceptual signal to a constant level, and this will determine about how fast the related control system can act. When the input events stop occurring, the perceptual signal will take the same length of time to decay, so the system will go on attempting to control the signal after the input events have actually stopped (the extinction curve). This is in fact how it works: if learning takes a long time, so does extinction, at least in certain learning experiments.

I believe that this analog model gives about the same results as a statistical-perception model does. The analogue model works with inputs that have an average frequency with random variations. It does NOT work properly (and neither does the statistical model) when the input frequency is perfectly regular. We notice the first tick of the clock that is missing or comes too soon or too late. So that sort of situation requires not an average rate detector, but a synchronized detector (I think I would put it at my "event" level of perception, whereas the other kind of rate detection would go one level lower, at the "transition" level).

Generally, I think that your analysis of intermittent reinforcement is exactly correct. I'm only proposing an analog method that does, in effect, the same computations but without requiring statistical calculations.

I'm not against statistics in general, or even against statistical explanations of neural functioning (at the appropriate level). When we consider noise in control systems, statistical methods help us appreciate its effects. What I "bash" with enthusiasm is the misapplication of statistical facts to individual occurrances. I've tried to make my criticisms specific to that case. That would seem to be a subject different from the one you are talking about.

Generally, I don't think we often get into situations where the environment is ambiguous or unpredictable. When you look around, you see a pretty noise-free visual field, with clear demarcations between objects, colors, sensations, relationships, and so on. When uncertainties do arise, we may sometimes use statistical methods to deal with them, by which I mean literally in terms of computing chances, but I think that in many cases we simply smooth out our perceptions and operate on the basis of the artificially unambiguous result -- often wrongly.

Anyway, people don't seem to compute their behavior on very good

statistical grounds, do they?

Date: Mon, 27 May 91 11:45:15 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: reality therapy and stuff

From Ed Ford May 27, 1991

Keeping up with this net is like trying to keep up with a moving train by running along side it - while it continues to accelerate at a faster and faster speed.

To All - The newsletter containing the conference form and details will be out this week. Those paid up CSG members will also receive Greg Williams' Closed Loop #2 edition.

Reality Therapy - For 14 years, I was a faculty and certifying member of the Institute For Reality Therapy. I taught at every Intensive and Certifying week in L.A.(where most were held) for over ten years. I was very close to Glasser. I left the institute in 1983. Glasser once remarked (somewhat in jest) that he would have called what he did THE Therapy, but he might have run into problems with others. He called it reality therapy because it best described what he was trying to do which was to get people to deal with the reality of their present life. It was the most efficient therapy I knew at the time.

When I was introduced to control theory by Glasser, some of what he said didn't make sense (don't deal with perceptions, leave that to the theorists). At the 1989 convention mentioned below, Glasser said, when talking about perceptions, "they say it is a hierarchy and you always start out with this one and then got to this one and this one. I don't recognize ten. In don't get involved with it. In terms of them (CSGers), it's a fundamental difference". Thus his total disregard for the hierarchy of perceptions (which he wrote about in Stations Of The Mind then obviously abandoned). I then left him and became a pupil of Bill Powers. When, after many years, I had finally begun to understand what control theory was all about, I realized that we control perceptions. Glasser never has. I heard a taped recording of a workshop on what others are thinking and saying about control theory given by Diane Gossen at the institute's 1989 convention which Bill Glasser attended. During the presentation, Glasser kept making comments and corrections to what Diane was saying. His degree of understanding of control theory was very evident.

When someone asked Glasser at the conference when does output become input, he replied "the only way that the behaving organism becomes aware of the behavior is through its ability to perceive which is input...You can go through all kinds of outputs all the time but what they (CSGers) are saying is that the only time you become aware of it (Glasser's idea of behavior) is through input". Glasser never has understood the concept of the controlled variable and that we control for input. He sees control of perception to mean that when we perceive what we are doing, we are controlling

our perception of our behavior. This is how he understands the title of Bill's book, Behavior: The Control Of Perception. He has never gotten away from the fact that we control our behavior. Remember, behavior to him is output, behavior to us is the entire system in operation.

He doesn't understand the levels of perception. Glasser said "the reason I got rid of the levels of perception is when I started to teach you could adjust the levels and I don't think you can. I think it is all the way through the top. You adjust to what the ultimate picture is....that's what drives you. If you think you can dissect your behavior...I think that is absolutely impossible." For Glasser, reference condition is called the "picture in your head". The picture for him is the entire hierarchy of perception and that is all you can control. Obviously the entire system is engaged in the operation but he doesn't believe you can be aware at any one level. Another major problem is that he uses "picture in your head" interchangeably as both perception and reference level. He doesn't see the comparison going on inside the head (between our perception and reference condition) but rather between the picture in our head and what he calls behavior and what we call our actions. At one point he uses the picture in your head concept as building a perception and at the next, as something you want or a reference condition. The same word is used for two entirely different concepts. The bottom line is that Glasser has never, never gotten away from controlling output. For him the comparison is between what we want (which he calls the picture in your head) and what we are presently doing to get what we want (our actions or what we are doing which we call output).

Other areas of misunderstanding by Glasser: He says his idea of needs are what CSGers would call disturbances. Obviously he doesn't understand disturbance because he doesn't understand the concept of controlled variable. Glasser sees the reasons for disturbances occurring are the basic needs. And there you have another major problem, the concept of needs. There are basic needs such as the need for water, food, etc. Where it gets tricky is when you get to such needs which Glasser identifies as Power, Fun, Freedom, Belonging. Our genetic system sets the limits on basic needs. But when it comes to those areas through which all of us strive to find satisfaction, they can been seen quite differently by all of us (such as our dealing with 11th order levels and trying to define and discuss them). I really struggled with this idea (with a great deal of help from Bill Powers) in Chapter 7, Freedom From Stress. It seems to me that we set the limits and perimeters of satisfaction within our own hierarchy, especially at the higher levels. This setting of limits is really our individual mark or standard for areas of importance to us, what Glasser would call needs. I think Glasser's higher order needs say more about him than anything else. People define their own internal goals and areas of satisfaction (read needs for Glasser) and from my daily reading of this net, they sure vary a lot within our own group.

Other areas which I don't want to dwell on include the following: He calls feelings (along with doing and thinking) a behavior (remember, his definition of behavior is output). His understanding of reorganization is also very confused. He re-

titled his book, Take Effective Control Of Your Life, and called it Control Theory. The sad thing is that he has taken the name Control Theory and assumed total control of what it means. Bill Glasser taught me more than anyone else a lot of great techniques for counselling and dealing with others. Unfortunately, when he was exposed to control theory, he changed control theory to suite his own perception of the world and to suite reality therapy. Over the past eight or ten years, as I have been learning control theory, I have tried to take my ideas of reality therapy and other ideas in counseling and adjust them to the new and very different world of control theory. CT has opened a whole new understanding of the world to me, and thanks to Bill Powers, Tom, Rick, and the rest of you, I have been able to use CT to learn and grow as a counselor, father, teacher, husband, and all the other hats that I wear.

The basic tenants of reality therapy are the steps (get involved, ask what do you want?, what are you doing?, and is what you are doing getting you what you want? Then get a plan and commitment. It also says that what ever we are doing is our attempt to satisfy our needs. With my understanding of control theory, I have been able to use reality therapy as a jumping board from which to develop a control theory therapy. Or, to use a more modern term, I am now a Perceptual Control Therapy Therapist.

Rick - When I am speaking of teaching how we control a perception, remember I am teaching control theory to people who know nothing about these ideas. Thus, the idea of controlling a perception is very new to them and very hard to grasp. I struggled for many, many years to understand this concept. I assume others do as well. I am speaking not as a theorist, but as a teacher and counselor. When people are in counselling, they want to control their spouse, their boss, their kids (those things which they can't change and other which they have no control), but not their own perception (those things of which they do have control). They want to talk of any symptom-problem they have, which will get them off of having to talk about their responsibility for their own internal world, including how they perceive and what they want. I guess, Rick, we are dealing in two very different arenas. When I speak, I speak from my own world as both a counselor and teacher. I think our dialogue helps both of us to learn about each other's world. And, I love it. Oh, yes, Rick, Hester (my wife) is most willing to take a walk with me and, guess what, she enjoys it.

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Date: Mon, 27 May 91 17:03:29 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: neural representation of signals

Joseph Lubin (910527):
>
>The best candidates yet for a "grandmother cell" are the
>face and hand cells of inferior temporal cortex. There are two

>strong reasons why even these cannot be local encodings. First,
>they have been found: in an area of maybe a million cells how
>likely would it be that experimenters could find a number of
>these cells over a few recording sessions? Secondly, the
>response records of many of these cells appear to either follow a
>certain parameter (eg. degree of head turn) or are simply
>too broad to be a local encoding.
>
>Even for the best candidate for the title "single signal" (in the
>grandmother cell sense), it is clear that the underlying
>representation must be more field-like than single unit.
>

I seem to remember an article in Science a couple of years ago in which single cell recordings found cells in sheep cortex(?) that responded to sheep faces but not to human or dog faces, others that responded to human but not sheep or dog, and (?) others that responded to dog but not sheep or human faces. My memory of this is quite hazy and I can't give you the reference (if I could, my memory would be a lot sharper), but I remember thinking at the time that perhaps there really are grandmother cells.

Not that it would affect any of Mr Lubin's argument about what constitutes a "single signal."
a
Martin Taylor

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Date: Mon, 27 May 91 16:55:33 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Action potential -- correction

[From Bill Powers]

Joseph Lubin (910527) --

It just sank in that you said "action potentials;" I was thinking "post-synaptic potentials." Speaking of the probability of an action potential -- a firing -- implies that a single impulse has significance. I assume that post-synaptic potentials govern the rate of firing, and that only rates are significant carriers of information.

So maybe we have something to argue about as well as discuss.

Best -- Bill Powers

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Date: Mon, 27 May 91 19:35:34 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: saturn.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: therapy stuff

From: David Goldstein
Subject: Reality Therapy, Experiential Psychotherapy
Date: May 27, 1991

Many years ago, I attended a workshop which Glasser gave about the time when he started to publish books about Control Theory. I asked him the very question which Rick raises. From his answer, I received the strong impression that as a result of studying Control Theory he revised his attitude from emphasizing a more objective view to a more subjective view of perception. Ed Ford and others who started out in the Reality Therapy camp could probably tell us more about it. It seemed that he found the questioning a little discomforting. I will leave it to Ed and others in the CSG to tell us how Reality Therapy compares to HCT Therapy. I am writing to tell the CSG about Experiential Therapy.

I have recently read a book by Alvin R. Maher called Experiential Psychotherapy: Basic Practices. I would highly recommend this book to other HCT clinicians. While the theory of human beings behind Experiential Therapy is not HCT, it is a form of existential-humanistic theory, I think that much of the practice is very consistent with HCT Therapy. For example, the method of levels is there! And an effort to descibe a particular person's control system hierarchy is there!

The meaning of the word experience in Experiential Psychotherapy is very much like the meaning of perception in HCT. I am going to focus on the ways in which Experiential Psychotherapy can contribute to the practice of HCT Therapy rather than vice versa. I do see HCT as making a contribution to Experiential Psychotherapy in many ways but I will save that for another time and place.

To start with, in Experiential Psychotherapy, the selection of topics is based on the intensity of experience which the patient experiences when attending to the topic. From the point of view of HCT, this makes good sense. Life areas in which a patient experiences a lot of, or only a little bit of satisfaction are likely to be ones about which a person has strong experiences.

While they seem to select topics in similar way, Experiential Psychotherapy and HCT Therapy differ in the way a topic is pursued. It is here that I believe that Experiential Psychotherapy can teach us. The therapist attempts to share the patient's experience as much as possible including the bodily experiences which go with discussing a topic. HCT clinicians do not do this as far as I know; they do not try to generate the patient's experiences within themselves.

The therapist attempts to have the same experiences as the patient is describing. This is called the "working level" of experiencing for the therapist. When this is not occurring, the therapist and patient stop and go back to the point at which the therapist lost touch with the patient. The sameness of experiences includes bodily experiences!

Maher provides some specific methods for helping a person achieve a strong experience, for helping a person become aware of the higher level experiences behind the one being discussed, for helping a person be/behave in a way consistent with the higher level experience and for helping a person extend the changes in

being/behaving to the patient's everyday life. These specific methods are very helpful, are based on years of clinical practice, and I see no reason for not using them.

In closing, I wish to emphasize that Experiential Psychotherapy makes me rethink the way that I am using feeling/mood experiences in the HCT Therapy which I do. I no longer simply try to intellectually figure out(imagine) what the patient's blocked desire is which is generating the feelings/moods. I try to experience the feelings/moods and the topic being described by the patient. I then use the experiences which are occurring within me, which are not explicitly described by the patient, to experiencially figure out what the patient's higher level perceptions are. In short, Experiential Psychotherapy is like HCT Therapy but with more feeling for both the patient and the therapist.

Now back to Reality Therapy. While I am not a trained Reality Therapist, I have watched Ed, Diane and Perry work at the CSG meetings. And I have read their books. It is my impression that feelings/moods experiences and expressions also play a secondary role in the therapy which they do. Reality Therapy plugs into the last step in Experiential Psychotherapy.

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Date: Mon, 27 May 91 19:55:55 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Amateur Radio

[from Gary Cziko]

While at a party last night I got to talking to an amateur radio operator ("ham"). I was a ham for a brief period as a a college undergraduate, but then left the USA for grad school and let it all slide. I might think of starting up again, but the stuff I hear the hams chatting about these days is not very exciting (weather; latest medical operations; how many watts and how many antenna elements).

Now if there were a bunch of people interested in HPCT (hierarchical, perceptual control theory, to make everyone happy) on the airwaves, that could make a difference. Are there people out there who are amateur radio operators, and others who might be if others were?

Let me know.--Gary

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Date: Mon, 27 May 91 18:23:28 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

From: marken@AEROSPACE.AERO.ORG
Subject: Reality, Compensation

[From Rick Marken]

Well, I just got home from a wonderful Memorial day spending some quality time hiking through the Santa Monica mountains with my main squeeze and got home to this TON of mail. First, thanks to Ed and David for the info on reality therapy. I think I get why Glasser got interested -- just a poor choice of words, that "reality" thing. Also, Ed, I know Hester will go for a walk with you - and I know she would enjoy it; I would too. I just thought I would point out that people are control system, and while they are often coorperative and predictable (that's the nicest kind) they can (in principle) be contrary.

Given all the new threads that have emerged, it might be wrong to do this but I am really curious about this. In the LA Times today they had this article about the huge salaries that CEOs get and they mentioned something about "compensation consultants". It seems to me that "compensation" is something that Perceptual Control Theorists should have an interest in. Does anyone out there know what compensation consultants know or think they know aboout compensation. After all, compensation is a consesquence of what we do; and it is something where someone else has "control" over the nature of the feedback function that relates your outputs to the perceived consequences of your actions (the compensation).

A behaviorist would say (I think) that compensation is a reinforcer. The more you give, the harder the person should work. We (CSGer) know that this is only true in special circumstances. Indeed, the rule should be (if a person only worked for compensation) that the more you give a person, the less they would work. This discussion could also be related to the "intermittant reinforcement" thread that turned up briefly. A behaviorist might encourage a company to give compensation on a temporally random basis if the company doesn't want their worker's to quit (extinguish). They could produce more enduring work with less compensation.

Anyway, there are, I think, some interesting implications of control theory for "good" ways to provide compensation.

Any thoughts?

Regards

Rick Marken
marken@aerospace.aero.org

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Date: Tue, 28 May 91 08:00:43 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Experiential psychotherapy

[From Bill Powers]

David Goldstein (910527) --

David, I presume that you remember a discussion in which we talked about devising "qualitative models" for kinds of behavior that were difficult to handle in a purely quantitative way, on a computer. Your description of Maler's "Experiential Psychotherapy" strongly suggests that it is a way of doing exactly what we were talking about. The "computer" in which you run your simulation of the other person is, of course, yourself. This living computer already contains the capacity to carry out, in imagination, all the functions of a human being (oddly enough) at exactly those levels of functioning that actually exist. There is no programming problem -- we don't have to figure out how relationship-perception works, or program-perception, or system-concept perception. The computer is guaranteed to be able to run any process at any level that is required. It is also guaranteed to contain exactly the levels that are required, not skipping any and not adding any that don't belong in a model of an adult human being.

As I read your description I was reminded of the problem of listening to someone who is giving you directions (something that a house-hunter does a lot of). When I hear the directions, I try to imagine the actual trip that is being described. Good directions give you a picture that is vivid enough so you don't have to write anything down -- when you actually follow them, it's as if you have already been there. Poor directions, on the other hand, are full of skips and jumps, private associations and incidental anecdotes; they convey a shifting point of view, sometimes from the viewpoint of the one taking the journey and sometimes as if from an aerial map or the position of a bystander.

When you try to follow poor directions in your imagination, you get a picture of a very confused mind. You don't, of course, actually sense the other person's confusion. But by trying to imagine following the directions, you become confused yourself. That is, when you try to run the model that the other person is describing, it leads you to see gaps and contradictions and other problems that leave you confused and by implication indicate at least a similar kind of confusion in the other person.

I think that this method can be refined by a control theorist into something even more workable than it already is, and also that it can tell us a lot about the role of language in the model. Language in the broadest sense of communicating through manipulation of perceptions is the medium through which one person tries to convey his experiences to another. It undoubtedly has limitations -- there may be inherent difficulties in trying to communicate principles and system concepts by any means but demonstration, and in trying to communicate very low-level perceptions such as the way a face looks or the way ice cream tastes. There are problems inherent in private meanings of words. But in an intimate and protracted relationship with one other person, a therapist should be able to cross-check the meanings, and put together a quite reasonable model of the other person, through imagination.

This means that the therapist must become an utterly flexible general-purpose simulation device without cultural biases and with no blind spots -- a selfless person. At least during the process of therapy. You would not want to simulate every person as if he were, for example, a middle-class Jew or an eccentric engineer. You have to allow the properties of

the other person, as nearly as they can be communicated, to enter into yourself and to operate as if they were yourself. It seems to me that doing this would amount to a discipline at least as rigorous as that which the Zen masters demand, at least as deep as the analysis that psychoanalysts are required to go through before they are considered ready to treat other people's problems.

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Date: Tue, 28 May 91 10:38:25 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Reality, Compensation

Rick Marken (910527):

>A behaviorist would say (I think) that compensation is a reinforcer.
>The more you give, the harder the person should work. We (CSGer)
>know that this is only true in special circumstances. Indeed, the
>rule should be (if a person only worked for compensation) that the
>more you give a person, the less they would work. This discussion
>could also be related to the "intermittant reinforcement"
>thread that turned up briefly. A behaviorist might encourage a
>company to give compensation on a temporally random basis if the
>company doesn't want their worker's to quit (extinguish). They
>could produce more enduring work with less compensation.
>

I agree with the theoretical point in the first part, but does it work that way in real life? Perhaps the reference signal isn't a fixed but high income, but an increasing income, and the behaviour that brings the perception into alignment with the reference is continually increasing work load. At any rate, that seems more plausible to me.

As to the implied rejection of the "behaviourist" view on the second point, why should the PCT theorist differ? I think that intermittent bonuses supplied for good work (i.e. not temporally random, but on random occasions of particularly good work) would be very likely to "produce more enduring work with less compensation." Do you disagree? If so, could you provide a CT rationale for disagreeing?

Martin Taylor

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Date: Tue, 28 May 91 10:37:35 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject: neural representation

[From Joe Lubin (052791)]

Gary Cziko (052791):

> I see that you are in civil engineering at Princeton. I had no
> idea that civil engineers dealt with neurons and dendrites.
> Somehow I got they idea that they built bridges, tunnels and
> stadiums. My goodness, are we psychologists going to have to

> put up with yet ANOTHER engineer who wants to do psychology?
> --Gary

I am an engineer, but I am not Civil. The robotics lab is in the Civil dept. I am essentially a theoretical neuroscientist in the robolab and the Human Information Processing Group.

Bill Powers (910527):

First off, in terms of neural coding, we probably have similar hypotheses and assumptions, and little can be gained in a discussion of the shadings of local versus distributed representations. I start to worry however (for altogether different reasons) when I hear stuff like:

> One reason I think that the single-signal hypothesis is
> important is that we must somehow tie the neural signals
> coming out of perceptual computers to the way the world
> appears to us. Perceptions of objects and relationships
> and so on in the apparent world seem unitary. This appearance
> seems most compatible with the single-signal idea. If you have
> a better idea, though, I will be listening.

This paragraph screams of a common, intuitive misconception. If I am not mistaken I take you as meaning that consciousness (that difficult word) maps more easily onto a local (eg. single cell or cell assembly) representation than it does onto a global representation. The reasoning behind this hypothesis is the seeming unitariness (unity) of our perceptions. In my work I am extremely wary of ascribing a perceptual feel to the underlying neural mechanism. Humans simply don't have the capacity to think in terms of the multidimensional nonlinear systems that we are. In order to approach understandings of such systems we must take small steps and continually check our simplifying assumptions. Since only a few people (Sommerhoff, Crick and Koch) have even a vague set of hypotheses concerning what consciousness is, I won't even attempt to localize the nexus of neural signal processing and conscious perception. (This interface, more than anything I can think of, boggles my mind: I just can't imagine how WE are embodied in mechanism.)

> The concept of dendritic computation is fine -- I've always
> assumed it. But it surprises me that modern authors would still
> be thinking of these computations as logical in nature, rather
> than analog.

You are well founded in your surprise. "Modern authors" think of these things in many ways. The soft logic analogy is a way of making explicit some essential properties:

(i) many different computations can be carried out locally within even a single dendritic arbor,
(ii) these computations are often clearly separated into excitatory and inhibitory components,
(iii) the sites of action as well as the signs of the signals

(excit, inhib) are crucial to the computation -- in this sense there is a bit of a syntax (eg. an inhibitory input on a spine will just sum with an excitatory input or shunt it away, but the same size inhibitory input located at a branch point which joins a number of dendritic subarbors can shunt to zero all signals inpinging on it from the subarbors).

> ... the fundamental information-carrying medium is
> assumed to be frequency of firing rather than single impulses.
> ... that output frequency is a function of multiple input
> frequencies -- and that single impulses do NOT signify logical
> computations.

I agreee completely. Even more complicated, is the strong possibility that MODULATIONS of firing frequency (not just the lesser-greater frequency dimension) represent codes in certain neural signalling systems. My working assumption is, however, as you stated it.

> So the sort of dendritic computation I visualize is more like
>
> (Ax1 + Bx2) * Cx3 ...
>
> ... where the x's are continuous variables measured as frequencies
> and the coefficients represent strengths of synaptic effects
> (inhibitory connections are just negative coefficients).

This is essentially equivalent to what I would propose, and even to Shepherd and Koch's analogy, if you explicitly include the possibility of shunting effects -- both shunting excitation (mass action gating) and shunting inhibition. This could easily be accomplished in your computation. The "discrete" character of neuronal signalling arises from the continuous nature of the components (ionic currents) as a results of two limiting mechanisms: the positive feedback loop which incites the action potential, and shunting inhibition mechanisms which can render an otherwise very active cell virtually silent.

> ... rather than saying "probability of an action potential"
> I would simply say "action potential." ....

This threw me off for a while, then I began to read "membrane potential" for "action potential." I think rather than postsynaptic potential (PSP) you probably mean membrane potential. PSPs (inhibitory and excitatory) are localized postsynaptically and correspond to specific synaptic signalling events.

> We have been stuck for a long time in trying to extend our
> modeling efforts to higher-level systems because we don't know
> how to model higher perceptual functions.

Neither do I, so I am beginning by doing as faithful a job as possible where the data exist. I guess we are both hoping that the mechanisms will look similar all the way up the hierarchy.

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Princeton University 609-258-4598
Princeton NJ 08544

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Date: Tue, 28 May 91 01:19:20 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject: neural coding

[From Joe Lubin (052791)]

Gary Cziko (052791):

> I see that you are in civil engineering at Princeton. I had no
> idea that civil engineers dealt with neurons and dendrites.
> Somehow I got they idea that they built bridges, tunnels and
> stadiums. My goodness, are we psychologists going to have to
> put up with yet ANOTHER engineer who wants to do psychology?
> --Gary

I am an engineer, but I am not Civil. The robotics lab is in the Civil dept. I am essentially a theoretical neuroscientist in the robolab and the Human Information Processing Group.

Bill Powers (910527):

First off, in terms of neural coding, we probably have similar hypotheses and assumptions, and little can be gained in a discussion of the shadings of local versus distributed representations. I start to worry however (for altogether different reasons) when I hear stuff like:

> One reason I think that the single-signal hypothesis is
> important is that we must somehow tie the neural signals
> coming out of perceptual computers to the way the world
> appears to us. Perceptions of objects and relationships
> and so on in the apparent world seem unitary. This appearance
> seems most compatible with the single-signal idea. If you have
> a better idea, though, I will be listening.

This paragraph screams of a common, intuitive misconception. If I am not mistaken I take you as meaning that consciousness (that difficult word) maps more easily onto a local (eg. single cell or cell assembly) representation than it does onto a global representation. The reasoning behind this hypothesis is the

seeming unitariness (unity) of our perceptions. In my work I am extremely wary of ascribing a perceptual feel to the underlying neural mechanism. Humans simply don't have the capacity to think in terms of the multidimensional nonlinear systems that we are. In order to approach understandings of such systems we must take small steps and continually check our simplifying assumptions. Since only a few people (Sommerhoff, Crick and Koch) have even a vague set of hypotheses concerning what consciousness is, I won't even attempt to localize the nexus of neural signal processing and conscious perception. (This interface, more than anything I can think of, boggles my mind: I just can't imagine how animus is embodied in mechanism.) I do not want to try to model level 11 in neuralstuff.

> The concept of dendritic computation is fine -- I've always
> assumed it. But it surprises me that modern authors would still
> be thinking of these computations as logical in nature, rather
> than analog.

You are well founded in you surprise. "Modern authors" think of these things in many ways. The soft logic analogy is a way of making explicit some essential properties:

(i) many different computations can be carried out locally within even a single dendritic arbor,

(ii) these computations are often clearly separated into excitatory and inhibitory components,

(iii) the sites of action as well as the signs of the signals (excit, inhib) are crucial to the computation -- in this sense there is a bit of a syntax (eg. an inhibitory input on a spine will just sum with an excitatory input or shunt it away, but the same size inhibitory input located at a branch point which joins a number of dendritic subarbors can shunt to zero all signals inpinging on it from the subarbors).

> ... the fundamental information-carrying medium is
> assumed to be frequency of firing rather than single impulses.
> ... that output frequency is a function of multiple input
> frequencies -- and that single impulses do NOT signify logical
> computations.

I agreee completely. Even more complicated, is the strong possibility that MODULATIONS of firing frequency (not just the lesser-greater frequency dimension) represent codes in certain neural signalling systems. My working assumption is, however, as you stated it.

> So the sort of dendritic computation I visualize is more like
>
> (Ax1 + Bx2) * Cx3 ...
>
> ... where the x's are continuous variables measured as frequencies
> and the coefficients represent strengths of synaptic effects
> (inhibitory connections are just negative coefficients).

This is essentially equivalent to what I would propose, and even to Shepherd and Koch's analogy, if you explicitly include the possibility of shunting effects -- both shunting excitation (mass action gating) and shunting inhibition. This could easily be accomplished in your computation. The "discrete" character of neuronal signalling arises from the continuous nature of the components (ionic currents) as a results of two limiting mechanisms: the positive feedback loop which incites the action potential, and shunting inhibition mechanisms which can render an otherwise very active cell virtually silent.

> ... rather than saying "probability of an action potential"
> I would simply say "action potential." ....

This threw me off for a while, then I began to read "membrane potential" for "action potential." I think rather than postsynaptic potential (PSP) you probably mean membrane potential. PSPs (inhibitory and excitatory) are localized postsynaptically and correspond to specific synaptic signalling events.

> We have been stuck for a long time in trying to extend our
> modeling efforts to higher-level systems because we don't know
> how to model higher perceptual functions.

Neither do I, so I am beginning by doing as faithful a job as possible where the data exist. I guess we are both hoping that the mechanisms will look similar all the way up the hierarchy.

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Date: Tue, 28 May 91 11:09:04 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: experimental cosmology, linguistics

For higher-level control issues it might be useful to look at some of Milton Rokeach's "experimental cosmology" work, described in _The Open and Closed Mind_ (Basic Books, 1960s I think). His distinction between closed-mindedness and rigidity is presumably a single phenomenon at two different levels of control. (One is reluctance to entertain new beliefs/disbeliefs, the other is reluctance to entertain alternative belief/disbelief systems.)

This is a very stimulating discussion and a marvellously collegial group, and I am delighted, though I can barely keep up. I have notes on various responses I would like to make, but my time is

severely constrained. Some time maybe soon I'll get something off, though it will doubtless be 6 months behind the stream on at least some threads.

Very little experimental work is done in linguistics (my field), thanks to the profoundly antiscientific bent introduced by Mr. Chomsky. A recent issue of Phonological Review was devoted to experimental phonology, and some of the work there (e.g. inducing slips of the tongue) might bear looking at from a CSG perspective. I'll look for reference. There is some work by some of the same people (Ewing, others) on experimental morphology (affixation, word derivation), but I haven't seen it.

Bruce Nevin
bn@bbn.com

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Date: Tue, 28 May 91 23:28:14 BST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: cam@AIFH.ED.AC.UK
Subject: Re: Perceptual Control Theory

[From Chris Malcolm]

-- This message is a repost, having been advised that it may have missed many sites.

In reply to Peter Cariani:

> Well, I actually like the name "Perceptual Control Theory." I think there
> would be a large number of people who would understand such a term (aside
> from psychologists, neurobiologists, and cogsci philosophers-- roboticists,
> and other more practically-oriented, less literally-minded people),

As a cogsci philosopher and roboticist I vote for "Perceptual Control Theory" too. It is reminiscent of the right book.

> but Cliff is right--there is no
> good general term for "perception".

"Perception" in general is a high level term, as Cliff suggested; but there are precedents for its use at a low level too, e.g. "percept" is sometimes used that way. The trouble with low level terms like "measurement" or "sensation" is that they are restricted to low level use, and one of the things CSG should emphasize is the importance of hierarchical control, i.e., running the gamut from "sensation" to "perception". Note also that perceptions sometimes require appropriate kinds of action to produce them, e.g., visual flow and collision avoidance, i.e., "perception" has a multi-level flavour.

"Input" should definitely be avoided, since it suggests the misleading and oversimplified "input -> think -> output" model of behaviour generation.

"Perception" has all the right associations, and sometimes a few wrong ones. I can't think of any other word with all the right ones. I think people find it easier to specialise a general term for a particular

domain by losing a few associations, rather than stretching it by adding associations.

Chris Malcolm
Dept of Artificial Intelligence, Edinburgh University

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Date: Tue, 28 May 91 18:06:10 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Neural models

[From Bill Powers]

Joe Lubin (910528) --

>If I am not mistaken I take you as meaning that consciousness (that
>difficult word) maps more easily onto a local (eg. single cell or cell
>assembly) representation than it does onto a global representation.

Not exactly. To speak of "mapping of consciousness" implies a third-party point of view from which both the map and that which is mapped can be seen. My thesis is at the same time simpler and more difficult to handle. It is that the world we experience IS THE MAP. In other words, if you want to know how neural signals representing the world appear to consciousness, just look around. The world as we experience it through all sensory channels HAS ALREADY BEEN ENCODED BY THE NERVOUS SYSTEM. We are looking at the outcome, not the inputs that lead to this outcome. I'm sure you will see immediately all the implications this has for practical neurological research: the "correlate" of a measurement of neural activity is, in the final analysis, neural activity in the brain of the observer! As the Fat Man said, a fine kettle of fish.

From the modeler's point of view, therefore, the problem is to begin with the physics-model of the external world and of the nervous system, and find computations and modes of representation that will end up with exactly the world we experience, at all levels. This means seeing the face of the clock with all the numbers arranged at regular intervals on its face, with the stationary minute and hour hands and the slowly moving second hand -- where every term in that description is embodied in neural signals: face, clock, number, regular, slowly moving, and so forth.

I have a suspicion that working all this out is ultimately going to demand some modifications of the physics model, not just of the neural model.

I don't pretend to know what consciousness is. But the objects of consciousness, I have assumed almost from the start, are neural signals. The world presented to consciousness by a model of the brain, therefore, must be a set of neural signals in a form capable of yielding the appearances we experience. If we assume that each entity in an experiential field is a global phemomena, like Pribram's holograms, then we are tacitly leaving all of the sorting out into individual items up to consciousness. Consciousness would have to be able to separate out which aspect of the global activities is experienced as the second hand, which as the hour hand, etc.. This seems intuitively wrong to me. That doesn't mean it IS wrong. But I will still argue that there is a case to be made

for "one percept, one neural signal." I don't think that modelers should make the job of consciousness any harder than it has to be.

On the other hand, I'm aware of difficulties with this idea, a new one to me being your observation that FINDING such a signal with an electrode would be very unlikely -- yet it is often done. That stumps me. But do you see my point concerning the world of experience?

>I agree completely [about analog rather than digital]. Even more
>complicated, is the strong possibility that MODULATIONS of firing
>frequency (not just the lesser-greater frequency dimension) represent
>codes in certain neural signalling systems. My working assumption is,
>however, as you stated it.

"Modulations" of neural frequency have to be demodulated, don't they? In my model, this possibility would be taken care of by a higher-level input function sensitive to rates of change or repetitive changes in magnitude. If real-time information is carried by the frequency of a neural signal, then a signal representing something like the state of a configuration would be "modulated" if the configuration began varying in some way. At the configuration level, the control system would just try to keep up, because it's not designed to see any temporal patterns in its input. But the next level up, which receives a copy of that same signal, WOULD be so designed, and the modulation pattern would end up as a steady-state signal representing the pattern of the modulation. And if the pattern changed ... Anyway, the hierarchical control model deals exclusively in terms of modulated neural frequencies, so I am glad to hear that you consider such things as strong possibilities.

Essentially all of my theoretical work has involved a study of the functions that must be carried out to account for experience and behavior. It doesn't really matter to me whether these functions are localized or distributed. But I'm always hoping to maintain some sort of continuity with physiology and physics, so the model doesn't start requiring things that are in principle impossible for a brain to do. It's going to be very educational for me to learn what you can tell us about real neural functioning. So far it doesn't sound as though the basic control-system model is going to require any radical alterations as a result, but we're ready to go in any direction called for by the facts. I am more heartened than you can know by all the agreements you find with my rather amateurish guesses; I hope there will be more of them (guesses and agreements)!

Best -- Bill Powers

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Date: Wed, 29 May 91 00:58:10 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject: neural reps, consciousness, and grandma

[From Joe Lubin]

Bill Powers (052891)

> It is that the world we experience IS THE MAP. In other words,
> if you want to know how neural signals representing the world

> appear to consciousness, just look around. The world as we
> experience it through all sensory channels HAS ALREADY BEEN
> ENCODED BY THE NERVOUS SYSTEM.

I completely agree with this. What I was referring to is stuff that I actually don't like to talk much about. It is the difficult question of who or what is doing the perceiving; difficult because there is really very little data to back up one's positions. The polarities on this issue are (i) neural mechanism as life and consciousness and all there is, and (ii) the position of duality of body and spirit. As broad and tolerant as this group may be, this is still probably not an appropriate forum for this discussion.

> I don't pretend to know what consciousness is. But the objects
> of consciousness, I have assumed almost from the start, are
> neural signals. The world presented to consciousness by a model
> of the brain, therefore, must be a set of neural signals in a
> form capable of yielding the appearances we experience.

I agree that the topology of the projections of sense data on our transducers is likely preserved in more central representations. There is much data supporting this in sensorimotor, visual, and auditory modalities.

> If we assume that each entity in an experiential field is a
> global phemomena, like Pribram's holograms, then we are tacitly
> leaving all of the sorting out into individual items up to
> consciousness. Consciousness would have to be able to separate
> out which aspect of the global activities is experienced as
> the second hand, which as the hour hand, etc.. This seems
> intuitively wrong to me. That doesn't mean it IS wrong. But I
> will still argue that there is a case to be made for "one
> percept, one neural signal." I don't think that modelers should
> make the job of consciousness any harder than it has to be.

Looks like I'm gonna have to keep talking about "consciousness." Perhaps to make it more palatable (and less sweeping) I'll replace "consciousness" with the term "attention." I do this for two reasons. First it is apropos to your paragraph and my discussion of it, and second it is the "mechanism" invoked by Crick and Koch in their "model" of consciousness. From their abstract:

> Visual awareness is a favorable form of consciousness to study neurobiologically. We propose that it takes two forms: a very fast form, linked to iconic memory, that may be difficult to study; and a somewhat slower one involving visual attention and short-term memory. In the slower form an attentional mechanism transiently binds together all those neurons whose activity relates to the relevant features of a single visual object. [Crick, Koch 1990]

The assumption that an experiential field will be a (nondecomposable?) global phenomenon is a strong one, and is not

warranted in light of much neurobiological data. If you are attributing this assumption to my statements concerning global versus local neural encodings, you are off track. This is not what I was saying.

A absolutely agree with "one percept, one neural signal" with appropriate qualifications on what is taken as a single neural signal, and on how attentional mechanisms conspire to produce a percept. The cortex is highly parcellated (and getting sliced finer daily) in terms of mapping its functional divisions. It is likely that attentional processes, in forms similar to those proposed above and by many others, acts as a searchlight [Crick 1984] to illuminate one (or a small number) of these regions at a time to bring necessary data "into consciousness" for appropriate processing.

Visual form (shape or contour), visual features (color, shading), and visual motion are separated early (in the retina) and maintain separation until somewhere past striate cortex (a likely locale for visual consciousness). Such signals need to be integrated into a single perception of feature and form in depth simply because that's what we see. This is what I refer to as visual consciousness. There are other sorts of consciousness that are visual in nature but are very different. Consciousness of a ball screaming towards your head is certainly visual, but you are not employing an attentional spotlight to follow the ball as it screams INTO your head. With such processing strategy you would end up screaming. Rather, a separate processing area specialized for detection of motion towards the head must be involved. It is from this area that signals originate with sufficient strength that they command one's attention. Another form of "visual attention" which is decidedly nonvisual is the phenomenon of blind sight. Patients with damage to striate cortext report being blind. When their doctors ask them to report where a visual stimulus is they scoff bitterly at the joke. When the doctors persist and the patient begins to play along, it becomes apparent that the patient can actually localize (point to) stimuli in space which they cannot "see" with high regularity and decent accuracy.

We process "where" information in the dorsal visual system (terminating in the medial temporal area, MT) and "what" information in the (basically separate) ventral system (terminating in inferior temporal cortex, IT). The following quote illustrates this point and relates to your paragraph of (052891) which follows.

> As one proceeds to areas higher in the hierarchy, the "mapping" becomes more diffuse. At the same time the neurons appear to respond to more complex features in the visual field. Different cortical areas specialize, to some extent, in different features, one responding mainly to motion, another more to color, etc. In the higher areas a neuron hardly knows where in the visual field the stimulus (such as a face) [face cells in IT cortex all have receptive fields which are centered on the fovea -- retinotopy, hence spatial information is neglected and must be "reintegrated" by accessing the dorsal

> system -- jml] is arising, while the feature it responds to may
> be so complex that individual neurons are often difficult to
> characterize effectively. [Crick 1984]

A mechanism postulated by Crick to link features in an attentional construct involves "producing transient cell assemblies, including 'vertical' ones that temporarily unite neurons at different levels in the neural hierarchy." [Crick 1984]

> This is a little like a "grandmother cell" hypothesis, I admit. But
> in the context of a multiple-level, multiply parallel model, a
> "grandmother" can exist at many levels. Only at a rather high level
> could a signal appear that indicates presence of "my grandmother"
> or "grandmothers." At that level, of course, only one signal is
> necessary because grandmother's attributes are multiple signals at
> lower levels. Grandmother's face, for example, could be represented
> as a single signal, while all the detailed sensations that make up
> various aspects of that face exist as hundreds of signals at the
> level of sensations. ... So the grandmother's-face cell really
> doesn't compute grandmother's face all by itself -- there are many
> layers of computation with multiple variables beneath it in the
> hierarchy. And all that the single output signal has to represent
> is the FACT that grandmother's face is present to some degree. It
> doesn't have to resemble grandmother's face. We know what a
> particular face is "a little like grandmother" simply because we
> get a little bit of signal out of the grandmother-recognizing input
> function.

There are people in the world who can draw a portrait of their grandmother (with her sitting nearby) and not recognize her (prosopagnosics). Others could recognize that a person was there, but not be able to see who it was. Still others could only perceive the right half of her face and body (contralateral neglect). These pathologies outline the multiply hierarchical nature of our processing. And I think they give trouble to those who want to localize grandmotherness in any functional unit.

So what is
visual attention? It is likely a series of pseudo-orthogonal mechanisms which, due to sequential (slow) mediation of our attentional subsystems, appear to be coherent. (Another strong constraint which endows coherence is the temporal and spatial coherence of the physical world and our interactions in it.)

These are just examples of what I would consider to be one percept, and how they interact with the issues of consciousness and local/global encoding (I am a localist in the macroscopic and a distributed representationalist at the level of neuronal encodings).
The point is that, as you said, a global nondecomposable percept is not good for much. It may be pretty but it wont allow one to extract the specific information needed to survive in complex environments.

> But do you see my point concerning the world of experience?

I think I do. We just need to get a sense of each other's terminology. Most likely we have been saying the same things to each other.

> "Modulations" of neural frequency have to be demodulated,
> don't they?

If a neural signal is carrying a complex time-dependent code (say, spike, spike, burst, rest, spike, burst) what is required on the other end is a physical mechanism that is triggered according to the sequence (eg, open Na+ channels, open K+ channels, close K+ leak channels, open Ca++ channels, open Ca++-mediated K+ channels). The time-dependent code may be the minimal possible code necessary to trigger (in sequence) a second temporal code from the receiving neuron. The complex code would be the signal -- "demodulation" would destroy this signal.

> ... Anyway, the hierarchical control model deals exclusively in
> terms of modulated neural frequencies, so I am glad to hear
> that you consider such things as strong possibilities.

I liked this paragraph very much.

> So far it doesn't sound as though the basic control-system model
> is going to require any radical alterations as a result, but
> we're ready to go in any direction called for by the facts.

I still haven't read anything about the "nuts and bolts" of your theory. (I just copied the BYTE articles.) My guess is that while your "system level" ideas are extremely strong, your intuitions about instantiating these in terms of processing elements will be less powerful. Mine may also be, in light of the emerging complexity of the brain's signalling systems.

[Crick F 1984]
Proceedings of the National Academy of Sciences,
81:4586-4590.

[Crick F, Koch C 1990]
Towards a neurobiological theory of consciousness.
seminars in THE NEUROSCIENCES,
Vol 2(4):263-275.

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Date: Wed, 29 May 91 16:54:37 +0200
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: BBRBFU01 WILL DISAPPEAR AFTER AUG 31ST, 1991 !!!
From: Z09302@BBRBFU01.BITNET
Subject: Programme 1st Principia Cybernetica Workshop

Programme of the

****

1st WORKSHOP OF THE PRINCIPIA CYBERNETICA PROJECT

computer-supported cooperative development
of an evolutionary-systemic philosophy

****

Free University of Brussels, Belgium
July 2-5, 1991

Subject
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Principia Cybernetica is an attempt by a group of researchers to collaboratively build a system of cybernetic philosophy, moving towards a transdisciplinary unification of the domain of Systems Theory and Cybernetics. This philosophical system will be developed as a network, consisting of nodes or concepts, linked by different types of semantic relations. The network will be implemented in a computer-based environment involving hypermedia, electronic mail, and electronic publishing. The project naturally splits into two issues:

1) development of the philosophy itself, which is systemic and evolutionary, emphasizing the spontaneous emergence of higher levels of organization or control through variation and natural selection. It includes: a) a metaphysics, based on processes as ontological primitives, b) an epistemology, which understands knowledge as constructed by the subject, but undergoing selection by the environment; c) an ethics, where survival and the continuance of the process of evolution is taken as supreme value.

2) development of computer-based tools and methods for collaborative theory building (CSCW, groupware, SGML, knowledge acquisition...): many participants with different backgrounds and working in different places exchange knowledge and opinions about a common problem; their different contributions and reactions must be integrated and structured, in order to form a coherent system of concepts and values, transparently modelling the problem domain.

Both issues are united by their common framework based on cybernetical and evolutionary principles: the computer-support system is intended to amplify the spontaneous development of knowledge which forms the main theme of the philosophy.

Schedule
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Tue (first day) Wed, Thu, Fri
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		09:30	Talk 1
10:00	registration	10:30	coffee pause
	informal meeting	11:00	Talk 2
		12:00	Talk 3
12:30	informal lunch	13:00	lunch
14:00	Heylighen	14:30	Talk 4
15:00	Turchin	15:15	Talk 5
16:00	coffee pause	16:00	coffee pause
16:30	Joslyn	16:30	Talk 6
17:30	McNeil	17:15	Talk 7
19:30	reception	20:00	evening event

With the exception of the reception on Tuesday evening (see Social Programme), all activities will take place in the "Union de Anciens Etudiants" (U.A.E.) building on the Campus Plaine of the Free University of Brussels (U.L.B.), Boulevard du Triomphe, 1050 Brussels, Belgium; Phone: +32 - 2 - 640 20 52.

List of Lectures and Events
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Tue am
Registration and informal meeting at the bar of the U.A.E., followed by an informal lunch, in order for the workshop participants to get acquainted

Tue pm
FRANCIS HEYLIGHEN
   An Evolutionary System Modelling Evolutionary Systems: introducing the Principia Cybernetica Project
CLIFF JOSLYN
   General Notes about the Principia Cybernetica Project and Related Initiatives
VALENTIN TURCHIN
   A Tentative Sketch of the Starting Nodes of PCP
DONALD H. MCNEIL
   The Principia Project

Tue evening
Welcome reception with Belgian beers in the cafe of the Toone puppet theatre ("Impasse du Schuddeveld" in the center of Brussels)

Wed am
LARS LOFGREN
   Foundational Issues Addressed by Cybernetics
GORDON PASK
   The Foundations of Conversation Theory, Interaction or Actors Theory, all Cybernetic and philosophically so
ALVARO MORENO, ARANTZA ETXEBERRIA & JON UMEREZ
   Biological Information: The Causal Roots of Meaning

Wed pm
VALENTIN TURCHIN
   Metasystem Transition as the Quantum of Evolution
RANULPH GLANVILLE
   Excavation and Underpinning, Foundation and Building
GERTRUDIS VAN DE VIJVER

Error: Epistemological Options in Cybernetics
ELAN MORITZ
The Case for Imperfect Machines

Wed evening
Conference dinner at the U.A.E. restaurant

Thu am
CLIFF JOSLYN
Control Theory and Meta-System Theory
FRANCIS HEYLIGHEN
Evolutionary Foundations for Metaphysics, Epistemology and Ethics
ELAN MORITZ
Memetics: Introduction and Implication to the Evolution of Knowledge

Thu pm
MARC E. CARVALLO
Self-organization, Evolution, and Religion: Some Notes on Erich Jantsch's Theory of Religion
ROBERT GLUECK
Metasystem Transition in the Machine and its Application to Knowledge Systems
CHARLES HENRY
Non-Verbal Aspects of Language and Knowledge Structuring
J.L. ELOHIM
Culture, Cybernetically Interpreted, is a Cybernetic Reflection of Nature Altered by Culture

Thu evening
Cybernetic concert performed by PETER BEYLS in the lecture room at the U.A.E.

Fri am
CLIFF JOSLYN
Software Support for Principia Cybernetica Development
DIRK KENIS
MacPolicy: Delphi and Group Decision Support Ideas for Computer Supported Cooperative Working
FRANCIS HEYLIGHEN
Structuring Knowledge in a Network of Concepts

Fri pm
Computer demonstrations of hypermedia systems, with some example nodes of the Principia Cybernetica Project.
Afterward there will be a (panel) discussion about the future of the Principia Cybernetica Project, where an attempt will be made to synthesize the different contributions and discussions brought forward in the Workshop. This should address as well the content, the foundations for a cybernetical philosophy, as the form, the practical tools and methods for further collaboration.

About the Workshop programme
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The Workshop is intended to allow all researchers interested in the Principia Cybernetica Project (PCP) to meet and to discuss the main issues. The event will be organized in the tradition of a relaxed, infor-

mal setting and warm social contacts initiated by the conference on "Self-Steering and Cognition in Complex Systems", held at the same site in 1987. Therefore we have tried to make the programme not too heavy, to allow ample time for discussion, to provide comfortable surroundings, and a pleasant and stimulating social programme.

This is not meant to be an ordinary "Congress" where a lot of unrelated papers are presented in sequence or in parallel, but a real "Workshop" with a specific theme, where the emphasis is on direct discussion and interaction between the participants, in order to prepare an eventual further collaboration on the PCP.

Together with the programme, all participants will receive the Workbook which contains short papers and abstracts selected by the scientific committee. This should help them to find the common threads between the different contributions, and to get an up-to-date overview of the PCP.

How to get there

Brussels, the capital of Europe, is very easy to reach. There are direct trains from London, Paris, Koeln, Berlin, and Amsterdam, which all stop at the three main stations of Bruxelles Nord, Bruxelles Central, and Bruxelles Midi. Brussels National Airport at Zaventem can be reached from all major cities in the world. It has a train service three times every hour to the city centre. The train station can be reached from the arrival hall (after passing the customs), where you take the escalator down to the terminal, where you can buy your ticket. (It is also possible to take a taxi at the airport, but this is not advisable, as it is rather expensive). Stay on the train until it arrives (after about 20 min). at the Bruxelles Central station.

From there, the underground (metro) takes you directly to the university. In the main hall of the station, follow the signs with an M (for metro), and after passing through a short tunnel under the road, you will arrive at the metro ticket office. After buying your ticket you should enter the metro station following the indication "direction Hermann-Debroux" (trains going toward the East). Get off at the Delta station, about 12 min. after the Central station.

In Delta follow the signs for U.L.B. You will come out in the pleasant green surroundings of the Campus Plaine of the university. From there follow the signs for the "Union des Anciens Etudiants" (U.A.E.), where you should arrive after about 5 min. walking. The registration office and lecture room of the Workshop are situated on the second floor (next to the bar and restaurant) of this small building.

For people coming by car along the highway: follow the great ring around Brussels until the crossing with the highway leading to Namur. Take the Namur highway in the opposite direction, that is to say in the direction of the city centre. Follow the road leading inward until its very end: the crossing with the traffic lights where you enter the Boulevard du Triomphe. Instead of following the large boulevard either to the left or to the right, you should enter a narrow private road somewhere in front: entrance number 4 of the Campus Plaine of the U.L.B. The parking place at the end of that road is situated just within view of the U.A.E. building.

Conference Registration

People who wish to participate in the conference should send the regis-
tration form as soon as possible to the address below. The conference fee

(3000 BF) can be paid at the registration desk, open on the first morning (July 2) at the workshop location. For registrations received after May 15, the fee will be 5000 BF. The fee covers attendance to all sessions, workshop documentation, including the "workbook" with abstracts and short papers, coffee, the reception and the concert. Lunches, the workshop dinner, all other meals, and accomodation are not included. Participants with limited means (e.g. students) may be exempted from the fee: please contact the workshop chairman.

Accommodation and Meals
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We have reserved a number of rooms in nearby hotels of different categories (see below). Please contact the hotel directly for reservation, mentioning your participation in the Workshop.

Meals can be taken at the (gastronomic) restaurant of the U.A.E. (menus starting at 600 BF without wine), or at the ULB students restaurant next to the U.A.E. (about 200 BF). (see map). There are a lot of other restaurants on the road towards the other campus (Solbosch) of the U.L.B., which is about 15 min. walking from Campus Plaine across the railway (to the west).

Social Programme
++++++++++++++++++

Reception
On the first evening of the workshop a welcome reception will be organized in a picturesque location in the centre of Brussels: the medieval cafe of the Brussels puppet theatre "Toone".

The cafe, where different types of puppets are exhibited, is specialized in Belgian and especially Brussels beers. Since beer in Belgium has about the same status concerning the varieties of tastes and the care with which it is brewed, as wine has in France, we decided to offer some typical beers to the reception participants, instead of the traditional aperitifs.

The theatre is situated in a tiny dead-end street ("Impasse du Schuddeveld"), about 1 to 2 metres wide, connected to the "Petite Rue des Bouchers" (street of the butchers) which is not much wider. This street of the medieval center of Brussels is famous for the fact that every house in the street is a restaurant. The street is closed for traffic (anyway it is too narrow to let cars pass), and this allows the restaurants to exhibit some of their products (especially sea-food and fish) in a rather spectacular fashion in front of the passers-by.

The "Rue des Bouchers" is just a few minutes walking from the Grand' Place, the central market square of Brussels, which is said to be the most beautiful in the world. The square is dominated by the magnificent 15th century Town Hall, with its hundreds of little statues.
Even with the Town Hall in relatively bad shape, the splendid 17th century buildings, with their golden inlays, surrounding the square, will certainly enchant you.

Dinner
The congress dinner, on Wednesday evening, will take place at the restaurant of the U.A.E., at the same location as the workshop. People wishing to participate should inscribe on their arrival at the registration office. The price will be about 1000 BF.

Concert

On Thursday evening, at the same location, we will have an artistic performance which perfectly fits into the conference theme. Computer artist Peter Beyls will feature in a concert with two of his compositions, "The Headless Horseman" for homemade instruments and computer, and "Statements of Innocence" for infra-red violin and computer.

The originality of this concert resides in the fact that Peter plays his instruments in dialogue with a set of A.I. computer programs that he has written, connected to sound synthesizers. The programs correspond to different actors with different styles and personalities. They react to the input of Peter's playing by improvising variations on the motives proposed by him.

In the first piece, the different actors together function as modules integrated in one system along the lines of Marvin Minsky's theory of the "Society of Mind". In both compositions, the different human and computer actors together form a self-organizing system, developing musical patterns that cannot be produced or predicted by either of them separately. The result is truly impressive, at least for listeners with an open ear for novel sounds and structures.

Things to see in Brussels
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Brussels is the second international city in the world, as measured by the number of headquarters of international organizations.

With its more than 1000 years of history the city offers many fascinating sights to visit. It boasts the most beautiful historic market square and the highest concentration of restaurants in the world (see social programme). Not far from that magnificent Grand'Place in the medieval center, you can also find the tourist attractions of Manneken Pis (a not very impressive but quite funny statue of a pissing boy) and the cathedral of Saint Michel, part of which is being renovated. Other interesting places to visit include the Atomium, the imposing Palais de Justice, and for the nature lovers the many parcs, especially the splendid Bois de la Cambre, which is not far from the university, and the beautiful forest to which it once belonged: the Foret de Soignes, with its centuries-old beech trees.

The Atomium, an enormous steel construction representing an iron atom with 9 spheres connected by corridors, is situated in the parc of the Heysel, north-west from the center, which can be reached directly by metro. Here you will also find the Bruparck amusement park, which offers among other things Mini-Europe, a permanent outdoor exhibition of small scale (1/25) precision-made replicas of Europe's most famous architectural sights.

Brussels offers numerous musea. Special mention deserve the (integrated) Royal Musea of Ancient and Modern Art (with a special section on the well-known Belgian surrealist Magritte), the museum of natural history with its collection of dinosaur skeletons, the museum of Art and History, near the impressive Cinquantenaire arc, and the recently opened Autoworld, which boasts the largest collection of old and new cars in the world.

Hotels
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Phone +32-2-...,Fax +32-2-...,Prize(BF):Single/Double,#rooms reserv.;
Address

Hotel Plasky,
733 75 18, 733 74 75, S 900-1250/D 1200-1620, 5;
Avenue E. Plasky 212, 1040 Brussels, Belgium

Hotel Lloyd George,
648 30 72, 646 53 61, S 995-1995/D 1350-2495, 9;
Av. Lloyd George 12, 1050 Brussels, Belgium

Hotel Lambermont,
242 55 95, 242 55 95, S 2400/D 2700, 8;
Ch. de Lambermont 322, 1030 Brussels, Belgium

Hotel Metropole,
217 23 00, 218 02 20, S 3900-4500/D 4900-5500, 5;
Place de Brouckere 31, 1000 Brussels, Belgium

Useful addresses
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General touristic information, guides and maps can be received, and hotel reservations can be made, at the following service centers:

Information bureau for tourism at the Brussels National Airport (in the arrival hall): Phone: 720 30 00, 720 30 01.
Tourism Belgium: 61 Rue du Marche aux Herbes, 1000 Brussels; Phone: 512 30 30, 513 90 90; Fax: 513 69 50
Tourist Information Brussels (TIB): Town Hall, Grand'Place, 1000 Brussels; Phone: 513 89 40.

Hotels can also be booked at:
Belgium International Reservations, 44 Toulousestraat, 1040 Brussels; Phone: 230 50 29.

Workshop Chairman:
F. Heylighen (Free University of Brussels)

Scientific Committee:
C. Joslyn (State Univ. New York at Binghamton)
V. Turchin (City University of New York)
J.P. Van Bendegem (Free University of Brussels)
G. Pask (London and University of Amsterdam)
G. de Zeeuw (University of Amsterdam)
J. Ramaekers (Int. Assoc. of Cybernetics)

Local Organization:
A. Vranckx (Free University of Brussels)
E. Van Engeland (Free University of Brussels)
A. Housen (Free University of Brussels)

Registration Form

1st Workshop of the Principia Cybernetica Project (Brussels, July 2-5, 1991)

Name : (Prof¦Dr).....
First name(s) :.....
Function :.....
Institution: .....
.....
Address : .....
.....
.....
Phone : (office)..... (home).....
Fax:.....
E-mail :.....
Title of Paper :.....
.....
Specific domains of research or interest : .....
.....
.....

o I hereby register as a participant in the Workshop, and I will pay the conference fee (5000 BF) at the workshop
o I submit a paper for presentation at the Workshop (abstract included)
o I would like to receive more information about the Principia Cybernetica Project (issue 0 of the Newsletter)

Date : Signature:

to be returned to :

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Dr. Francis Heylighen (workshop chairman)
PO-PESP, Free Univ. Brussels, Pleinlaan 2, B-1050 Brussels, Belgium
Phone +32 - 2 - 641 25 25 Email Z09302@BBRBFU01.bitnet
Fax +32 - 2 - 641 24 89 Telex 61051 VUBCO B

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Date: Wed, 29 May 91 11:38:50 BRA
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Flavio Codeco <IBG10005@UFRJ.BITNET>

Hi you all,

First of all, thank you for the help with the papers!
Second, as I'm new to the list, I'll make a short description of my interests in the PCT field: It consists basicaly of the possible applications of the PCT in the analysis and modeling of the "behaviour" of ecosystems. But as I'm still don't know very well the PCT, I don't even know if it's possible at all... Wish me good luck!

without more,

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ê ê ! IBG10005@UFRJ.BITNET

  0 0   Don't   ! Flavio Codeco Coelho
   >   worry,   ! rua da Passagem 71/802 Botafogo
 ___/ be happy!!   ! zip:22290
                     ! Rio de Janeiro - RJ
                     ! Brasil

TEL:(021) 542-3225
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Date: Wed, 29 May 91 08:48:38 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Compensation, Consciousness, Collected Papers

[From Rick Marken]

First, a quick reply to Martin Taylor (910528)

>As to the implied rejection of the "behaviourist" view on the second
>point, why should the PCT theorist differ? I think that intermittent
>bonuses supplied for good work (i.e. not temporally random, but on
>random occasions of particularly good work) would be very likely to
>"produce more enduring work with less compensation." Do you disagree?
>If so, could you provide a CT rationale for disagreeing?

I reject the behaviorist view, just not the phenomenon (of intermittant reinforcement). Intermittant bonuses would produce more enduring work with less compensation -- if the work is being done only for the bonuses. One difference between behaviorism and control theory is that the former views behavior as controlled BY its consequences (bonuses in this cases) and the latter sees consequences as controlled by behavior (the work that is done). PCT suggests that bunuses are a controlled variable (if the worker does stuff to get them -- and changes his work level to keep getting them [ie compensates for disturbances to occurance of the bonuses]) so the worker can change (for higher level reasons) the reference level for the controlled variable. The manager could find, for example, that the worker suddenly (and inexplicably from a behaviorist point of view) acts to avoid getting bonuses. I was simply suggesting a discussion of how control theory would deal with the matter of compensation -- and this is a very BIG topic; it is not just about whether rewards increase or decrease behavioral output, because PCT views humans as something considerably more complex than a set of emitted outputs shaped by reinforcments. But PCT certainly does not deny real phenomena that have been studied by behaviorists -- indeed, operant conditioning experiments are a great testing ground for PCT -- and, so far, PCT does as good or better than behaviorist models (well, behaviorist descriptions).

Powers and Lubin (Last couple of days)

Now I know what it must have felt like for those not directly involved in the "theory of 11th order" discussion. I can barely keep up with the Powers-Lubin exchange on consciousness. But it's great stuff. I'll try to jump in when I can. The discussion is definitely relevant to some of the issues I discuss in my "Hierarchical behavior of perception" paper. I would love it if Lubin would read and comment on that paper -- it is available as a text file from Gary Cziko.

One last note. Tom Bourbon asked for copies of all my reprints on PCT. I ended up with a pretty thick stack of papers -- and they fit into a neat set of about five different topic areas. It made me think 1) that it would be expensive to send the whole set to Tom and 2) this could work as a little book. I talked to Greg Williams by phone about it. Making the book would cost more then sending the xeroxes to Tom, but I thought that if some of you who teach would ever use such a book as an optional, supplementary readings text, maybe it would defry some of the costs (which I would assume) of printing up a bound version of the papers (with introductory comments on the subject areas). I would like some feedback about this -- would any of you teachers be willing to assign such a book (published by CSG Press, of course)? What do say Tom, Dick Robertson, Dennis Delprato, Wayne Hershberger, anyone else?

Thanks

****

Richard S. Marken USMail: 10459 Holman Ave
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Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)

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Date: Wed, 29 May 91 12:12:56 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: psy_delprato@EMUNIX.EMICH.EDU
Subject: RE: Compensation, Consciousness, Collected Papers

[FROM Dennis Delprato]

Rick Marken (910529):

Could you post a list of the proposed Table of Contents of the collected papers?

Dennis Delprato
Dept. of Psychology
Eastern Mich. Univ.
Ypsilanti, MI 48197

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Date: Wed, 29 May 91 12:54:01 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: sneaky behavior

Bill (910524),

I've been trying to understand how one might apply Rick's and your comments on performance/competence. A couple of things fall out--

I would want to label the profanity or the handwriting result "unintentional" behavior; that is, I didn't "mean" to do it, it happened because I was working on something else.

The idea of getting someone to do things this way IS intriguing. I haven't yet thought of how one might get verbal analogues. There is a language methodology which I think has this in mind: Suggestopedia (Lozanov). By getting people into a relaxed state (hypermnesia), Lozanov supposed that many aspects of language would sort of "sneak in" along with what students paid attention to. Hmmm. I'm trying to perceive how I could produce a syntactic form or lexeme in a completely new language without having the rule or word "mean" something. Maybe there's something missing in my definitions of performance/competence from a CT perspective. You can get me to write a profanity in Swahili but it wouldn't mean anything to me. Now later I may find out it's significance and then rememeber I already know how to write/say it. Part of my crash course in Spanish consisted of memorizing long passages. Only later did I really understand the meaning or appreciate the syntactical constructions. A few I remember to this day (10 yrs. later). But I don't think I did anything in Spanish before having a basis for doing it.

Joel Judd

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Date: Wed, 29 May 91 12:45:34 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG

[From Rick Marken]

Dennis Delprato (910529) and whomever else might be interested:

Here is my proposed Table of Contents (and title) for a book of supplementary readings in PCT:

Mind Reading: Experimental Studies of Purpose

Richard S. Marken

Introduction

The Nature of Behavior

Marken, R. S. (1988) The nature of behavior: Control as fact and theory. Behavioral Science, 33, 196- 206.

Purposeful Behavior

Marken, R. S. (1982) Intentional and accidental behavior: A control theory analysis. Psychological Reports, 50, 647-650

Marken, R. S. (1983) "Mind reading": A look at changing intentions. Psychological Reports, 53, 287-270.

Marken, R. S. (1989) Behavior in the first degree. In W. Hershberger

(Ed.) Volitional Action, Elsevier Science Publishers: North-Holland.

The Causal Circle

Marken, R. S. (1980) The cause of control movements in a tracking task. Perceptual and Motor Skills, 51, 755-758.

Marken, R. S. (1981) Closed-loop behavior: Human performance as control of input. Western Psychological Association Meeeting, Los Angeles, CA.

Control of consequences

Marken, R. S. (1985) Selection of consequences: Adaptive behavior from random reinforcement. Psychological Reports, 56, 379-383.

Marken, R. S. and Powers, W. T. (1989) Random-walk chemotaxis: Trial-and-error as a control process. Behavioral Neuroscience , 103, 1348-1355.

Hierarchical Control

Marken, R. S. and Powers, W. T. (1989) Levels of intention in behavior. In W. Hershberger (Ed.) Volitional Action, Elsevier Science Publishers: North-Holland.

Marken, R. S. (1990) Spreadsheet analysis of a hierarchical control system model of behavior, Behavior Research Methods, Instruments, & Computers, 22, 349 - 359.

Coordination

Marken, R. S. (1986) Perceptual organization of behavior: A hierarchical control model of coordinated action. Journal of Experimental Psychology: Human Perception & Performance, 12, 67 - 76.

Marken, R. S. (1991) Degrees of freedom in behavior. Psychological Science, 2, 92 - 100.

Applications

Marken, R. S. (1986) Human factors and human nature: Are psychological theories really necessary? Human Factors Society Bulletin , 29, 1- 2

The nice thing about this book is that it would save me the trouble of actually having to write one. I'll do that anyway but I do think this little collection -- though not always easy reading -- would be a good supplement to the Robertson/Powers intro text or even the Powers text. I would write a short introduction and chapter introductions. What do you think?

Hasta Luego

****

Richard S. Marken USMail: 10459 Holman Ave
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Date: Wed, 29 May 91 17:22:14 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: A short note before leaving again/ a request

FROM CHUCK TUCKER 29 May 91

I just dropped by to get my mail (48 posts!) before I return to the beach this weekend for about two weeks to be with my Grandson and his parents. I have printed the mail and may have something to say in several weeks when I return but in the meanwhile I have a request:

Please mail me a citation for those manuscripts and/or publications that you have done this year so I can catch up on my reading before the CSG meeting.

Thanks Chuck More later

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Date: Wed, 29 May 91 17:54:06 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Neural modeling

[From Bill Powers]

Joe Lubin (910529) --

> ... stuff that I actually don't like to talk much about. It is the
>difficult question of who or what is doing the perceiving; difficult
>because there is really very little data to back up one's positions.
>The polarities on this issue are (i) neural mechanism as life and
>consciousness and all there is, and (ii) the position of duality of body
>and spirit. As broad and tolerant as this group may be, this is still
>probably not an appropriate forum for this discussion.

Well, there's always the risk of substituting one dogma for another, but when you're cooking up theories, I think you have to serve up hot pastrami as well as cold potato salad or go out of business (i.e., those who can't stand the heat ...). I'm with you in not wanting to go beyond the data too far, but I don't see any reason for not trying on different points of view to see if they make sense of what we don't understand yet.

So how's this? The brain is a three-dimensional interface between reality and an entity capable of signal reception (awareness) and arbitrary signal-injection (volition), but without capabilities for organized perception or thought. This is dualism, but the only magic it contains is

the kind Arthur C. Clark talked about.

From my viewpoint, the main problem right now is not to identify that which observes, but to figure out what it is FOR. Speculations along those lines are what led me to associate awareness with a reorganizing system. The only really serious mistake we could make would be to deny that there is an Observer.

I said "The world presented to consciousness by a model of the brain, therefore, must be a set of neural signals in a form capable of yielding the appearances we experience."

You said
>I agree that the topology of the projections of sense data on our
>transducers is likely preserved in more central representations. There
>is much data supporting this in sensorimotor, visual, and auditory
>modalities.

Hmm, that's a pretty cautious agreement. Physical adjacency in the brain and physical topology in general may facilitate the development of certain computations, but there's nothing in the brain that can detect the physical arrangement of neurons. Signal-wise, it doesn't matter how the signals are mapped onto various cortices. If the connections were preserved, the world would look the same for any geometrical mapping. I'll admit that many computations would become far less likely to occur without some form of mapping that preserves retinal and bodily topology.

When I speak of "appearances we experience," I am referring to far more than spatial relationships. The world we experience consists of intensities, sensations, configurations, transitions, events, relationships, categories, ordered sequences, programs (networks of choice-points), principles, and system concepts -- if you're to believe my latest conception of the hierarchy. Items in each of these classes must be represented neurally if they are to exist for us -- and in every sensory modality. SOME perceptions entail spatial and adjacency relationships.

Your citation:
>In the slower form an attentional mechanism transiently binds together
>all those neurons whose activity relates to the relevant features of a
>single visual object. [Crick, Koch 1990]

While there are more than a few questions begged here (what is "binding" and what determines "relevance" for instance?), this idea contains one germ that I have toyed with: the idea that the hierarchy is transient. When you see my whole model, you will see that it assumes every control system to be present and functioning all the time, from the moment it becomes organized. The "reorganization system" helps a little, but basically this system just grows and grows and grows. The obvious problem is how you turn off the systems that are irrelevant in different contexts. I'm not bothered by the number of systems implied (because each system is exceedingly simple in my decomposition and, as you are making clear, even a single neuron is a powerful computer), so much as by the problem of telling systems to stop controlling for their own perceptions. A reference signal of zero won't do it: that just implies avoidance behavior. You need at least those signals you mentioned at the junctions

of dendrites that shunt or turn off the signals from computations taking place further out. There may be a compromise somewhere in here: a combination of analog computers with a switching system that establishes a matrix of connections under higher-level control. That "hybrid computer" that's been mentioned several times on the net.

At the lower levels we must have a lot of systems that are essentially always active (especially if you include the biochemical systems). But at higher levels we clearly have contexts. So I have speculated that the basic hierarchy related to a given context may be STORED, so that the same neural tissue can be used to support many different hierarchies. All of them would adhere to the same basic structure of levels, because a level is defined by the kinds of computations that are available for transforming variables at one level into variables of a higher-order kind. But inactive versions of the hierarchy are simply (functionally) not there at all. "Remembering how" would then be literally remembering an organization of control systems, and installing it (presumably using a function called INSTALL.BAT).

>The assumption that an experiential field will be a (nondecomposable?)
>global phenomenon is a strong one, and is not warranted in light of much
>neurobiological data. If you are attributing this assumption to my
>statements concerning global versus local neural encodings, you are off
>track. This is not what I was saying.

Good.

>I absolutely agree with "one percept, one neural signal" with
>appropriate qualifications on what is taken as a single neural signal,
>and on how attentional mechanisms conspire to produce a percept. The
>cortex is highly parcellated (and getting sliced finer daily) in terms
>of mapping its functional divisions. It is likely that attentional
>processes, in forms similar to those proposed above and by many others,
>acts as a searchlight [Crick 1984] to illuminate one (or a small number)
>of these regions at a time to bring necessary data "into consciousness"
>for appropriate processing.

We're sniffing around the same ideas here. Here's something else to consider concerning attention and "appropriate processing." I've mentioned it before, but your comment provides a context.

In the hierarchical model, perceptions of one level are derived not directly from sensory inputs, but from perceptual signals existing at the next lower level (or perhaps several lower levels). But this is not just a perceptual hierarchy; it's a control hierarchy. So you can tell experimentally whether a given perception is under control, using disturbances to see if the system acts to oppose them. If a disturbance is resisted, the associated perceptual signal IS BEING COMPUTED, whether or not the signal is part of the conscious world. If there is control at some higher level, the model insists that the perceptions of lower order are ALSO under control, EVEN IF THEY ARE NOT CONSCIOUS. This is easy to show by applying appropriate disturbances, and seeing them corrected.

If there is any kind of processing appropriate to systems operating in the conscious mode (my way of putting it), it can't be ordinary perceptual processing. Ordinary perceptual processing is the same whether consciousness is involved or not. I think we still don't know what

consciousness is for. There are some really promising lines of experimental research here -- I think the CSG is going to get to them pretty soon (David McCord, at Western Carolina University, was going to start a collaborative project with me when my plans were abruptly altered last Fall. When that project is done I may have to take everything back).

The phenomenon of "blind sight" (which you mention later in the post) completely supports this argument. If the experimenters had known the Test for the controlled variable, they could have found out for sure what was still being perceived -- and at what level.

One other thought. When the cortex is "parcellated" into functions, I think there's a tendency to assume that all cortices must be parcellated in the same way. That's probably an unnecessarily limiting concept. Even in the motor cortex, the famous homunculus is not the same in all people or animals with respect to location, shape, or size. Fine slicing of the cortex is useful as a way of defining functions that MAY appear in a given brain, but not as a map of "the brain." I think that it's just as useful to define functions experimentally; control theory gives us a pretty good window to what is being perceived. Anybody can propose that some variable is being perceived. But control theory lets you prove or disprove the proposal.

As to your comments on the "grandmother cell" subject:

Grandmother is not a simple unitary perception, as you say. But let's not confuse the objective grandmother out there in hypothetical reality with the neural grandmother that we experience. At one level of experience, grandmother is a mass of sensations; at another, a familiar configuration; at another, certain little moves and speech habits; at another, something in relationship to me; at another, an example of a category; at another, something that behaves in certain familiar sequences -- saying familiar phrases, bustling about in familiar ways; at another ... Anyway, at each level of perception, these attributes must appear as neural signals if they are to be experienced.

I don't limit experience to the highest level. We can focus attention on any level of signals, excluding others from consciousness (the "searchlight"). But all the other levels keep right on working, even if they're outside the searchlight's cone. This means that the ultimate "grandmother signal" -- the one that I would call the system concept of grandmother -- is always present and always determining the principles, programs, etc., that come into play when we deal with grandmother. One thing I DON'T do is to say, "well, Grandmother herself is the WHOLE THING," waving my arms and involking holistic magic. The model says that grandmother is represented by many signals at many levels, but it also says that at a given level, grandmother is represented in a specific way by a simple neural signal, or set of signals representing multiple attributes of the same level. Even though a higher-level concept of grandmother may be implicit in the multiple attributes, that higher-level concept has no operational existence until it is computed and represented as a signal (Frank Rosenblatt's principle). Furthermore, in a given collection of attributes, an infinity of higher-order concepts is implicit. Which one becomes explicit depends on how the signals are combined in a computation, not on how reality is organized.

Is this relevant to what you were saying? I sort of wandered off on a

different track there.

Concerning "modulation:"

>The complex code would be the signal -- "demodulation" would destroy
>this signal.

Language problem. A demodulator, in my world, receives a modulated signal and emits a signal that depends only on the modulation, not on the carrier. I think we're just talking about the relationship between two levels of perception. A musical piece that goes "tick tick blat pause tick pause blat" would be perceived at one level as simply a continuous flow of sensations. A person creating this flow of sensations by playing a musical instrument wouldn't "demodulate" the sensations at that level. He would be working his fingers like mad to make the sensations follow the detailed pattern of reference-sensations being received by that level of control systems. At a higher level, the same fluctuations in the neural signal would be recognized as part of a familiar melody. As long as the familiar melody continued, the higher-level signal would be constant, saying "Yep, it's still Bach's Prelude in G." The complex processes involving potassium and calcium that you propose would be the mechanism by which the variations in the lower-level signal came to be represented as a single persistent signal representing a higher-level aspect of the sensory flow. I'm delighted, by the way, to hear of a way in which such complex temporal patterns could actually be perceived by a single neurone. Although I do wonder what mechanism could make an output signal dependent on such a complex sequence of channel openings and closings. Are you being a teeny bit fanciful?

Finally --

>My guess is that while your "system level" ideas are extremely strong,
>your intuitions about instantiating these in terms of processing
>elements will be less powerful.

You are a very good guesser, although "extremely" is extreme. I am having a funny reaction to our conversation. For a long time, an unreasonable number of years, ideas like the ones we've been talking about have been floating around in my head, but the world I've inhabited hasn't included anyone else interested in the same levels of analysis. So all this neurological stuff has become interwoven in my mind with the behavioral model. Now that you're here with a lot of notions about how the neural systems really work, I'm beginning to feel a sort of proprietarial twinge. I'm losing control of control theory!

Actually, this is exactly what I want, even though it's hard to let go of any part of it. The functional part of the model has a solid core, I think, and will at least turn some thoughts in new directions. But in retrospect, I realize that I never did have much grasp of the real nitty-gritty of neural functioning -- just some Big Picture sketches of what seems to be going on. I can't help feeling a bit threatened -- what if neurones don't really work the way I thought? What if neurological research turns up some completely different organization, making control theory into a figment of my imagination? It's not always so easy to hear myself saying "I'd rather know the truth than be right." Sometimes I truly would just rather be right and have everyone admire me.

Well, that sort of insecurity passes, and I remember that it is more important for us, all of us, to understand human nature than it is come come up with any particular understanding. I think that we need a lot of progress in a short time if a science of life is to have any effect on the course of human affairs before it is too late. We are right on the edge of a new way to deal with human beings, a complete break with the dismal past. With that sort of thought in mind, I think I can stand just about anything. So do your worst.

Best --- Bill Powers

General Note:

This morning, Mary and I went out to the road that runs by our condominium, and waved hello to Wayne and Joyce Hershberger as they passed by on the Durango-Silverton Narrow-Gauge Railway that runs next to Highway 550. Wayne called ten minutes before the train left to tell us they were in town. Dinner tonight, now that they know where we live.

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Date: Wed, 29 May 91 21:34:07 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: misc.

From Ed Ford May 29, 1991

Tom - I sent out all the newsletters this morning. The corrected information can be sent out to people as they send in their reservations.

Rick - I'd love to get two copies of your manuscripts (at least), one for the ASU library (to which I send my students) and one for my library.

To all who received the newsletter: Bill Powers correct address is: PO Box 2566, Durango, CO 81302-2566. Also, the correct number for the dorm where we will be staying is 303 247-7488. Any corrected information will be sent out when reservations are received.

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Date: Thu, 30 May 91 14:08:36 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Marken Book; Tolman

[from Gary Cziko]

Rick Marken (910529)

>The nice thing about this book is that it would save me the trouble of
>actually having to write one. I'll do that anyway but I do think this

>little collection -- though not always easy reading -- would be a good
>supplement to the Robertson/Powers intro text or even the Powers text.
>I would write a short introduction and chapter introductions. What do
>you think

I would certainly be interested in getting this for myself. And if my course on control theory flies in spring 1992 so that they let me do it again, it could conceivably mean a dozen or sales each year.
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On Edward C. Tolman on Purpose:

I'm working on a book which is to show how all intances of fit (adaptive complexity) is ultimately due to a Darwinian process of blind variation and selective retention. At least two chapters will deal with learning (acquisition of adaptive complexity in behavior) starting with conditioning and its problems and ending up with control theory and its advantages and promises. As a bridge between the two I'd like to say something about behavioristic approaches to purposeful behavior, and I suppose that means Tolman.

I am starting to read Tolman's stuff, of which I know quite little (other than the image of rats rolling around in mazes in my mind). Rick Marken mentioned that Tolman discovered a phenomenon but had no theory to explain it. This would make a great bridge between conditioning and control theory if accurate. I would appreciate it if Rick and/or others could provide me some more background to Tolman and perhaps point me to published critiques of his approach. That may save me from trying to make sense out of all of his formulas with S and R subscripted and superscripted and dotted and summated, etc. connected with arrows and more dots and sometimes all put in curly brackets to boot!

Of course, I would acknowledge in the book the assistance of anyone who provided me any useful information.--Gary

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Date: Thu, 30 May 91 17:02:40 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject: Re: neural reps

[From Joe Lubin]

Bill Powers (052991)

> >From my viewpoint, the main problem right now is not to identify
> that which observes, but to figure out what it is FOR.
> Speculations along those lines are what led me to associate
> awareness with a reorganizing system. The only really serious

> mistake we could make would be to deny that there is an Observer.

I agree that this level of inquiry which I would call ontological, is necessary at this point to complement (extend) inquiries concerning how the organism accomplishes its computations. Associating "awareness with a reorganizing system" seems a very good start.

> Hmm, that's a pretty cautious agreement. Physical adjacency in
> the brain and physical topology in general may facilitate the
> development of certain computations, but there's nothing in the
> brain that can detect the physical arrangement of neurons.
> Signal-wise, it doesn't matter how the signals are mapped onto
> various cortices. If the connections were preserved, the world
> would look the same for any geometrical mapping. I'll admit that
> many computations would become far less likely to occur without
> some form of mapping that preserves retinal and bodily topology.

Yes, indeed. It was my cautious attempt to map a rather vague formulation:

> "The world presented to consciousness by a model of the brain,
> therefore, must be a set of neural signals in a form capable
> of yielding the appearances we experience."

onto concepts that I feel comfortable with.

> When I speak of "appearances we experience," I am referring to
> far more than spatial relationships. The world we experience
> consists of intensities, sensations, configurations, transitions,
> events, relationships, categories, ordered sequences, programs
> (networks of choice-points), principles, and system concepts --
> if you're to believe my latest conception of the hierarchy. Items
> in each of these classes must be represented neurally if they are
> to exist for us -- and in every sensory modality. SOME perceptions
> entail spatial and adjacency relationships.

All of these experiences must be represented in the brain in terms of three general manifolds that jointly comprise the state space of the representations: a spatial manifold (topology, space constant within neurons), a temporal manifold (latency due to connectivity, time constant within neurons), for and a signal manifold (channels, currents, spikes, frequencies, temporal codes). Thus spatial relationships among neurons are fundamental to (i) the types of computations that are done and (ii) to the possibility of organizing any such computations in the first place. It does appear that at the lower levels of representation hierarchies that more spatial order is required, and at the higher levels more random connectivity is required.

> When you see my whole model, you will see that it assumes every
> control system to be present and functioning all the time, from
> the moment it becomes organized. The "reorganization system"
> helps a little, but basically this system just grows and grows
> and grows. The obvious problem is how you turn off the systems
> that are irrelevant in different contexts. I'm not bothered by

> the number of systems implied (because each system is exceedingly
> simple in my decomposition and, as you are making clear, even a
> single neuron is a powerful computer), so much as by the problem
> of telling systems to stop controlling for their own perceptions.
> A reference signal of zero won't do it: that just implies
> avoidance behavior. You need at least those signals you mentioned
> at the junctions of dendrites that shunt or turn off the signals
> from computations taking place further out. There may be a
> compromise somewhere in here: a combination of analog computers
> with a switching system that establishes a matrix of connections
> under higher-level control. That "hybrid computer" that's been
> mentioned several times on the net.

The two issues, the utility of forgetting, and conflicting data management, are difficult ones to grapple with in a neural framework because they probably imply higher level control. These are what you would probably call reorganizational issues. In terms of choosing which subsystems to use to solve certain problems there are good candidates for this sort of control in the many inhibitory "gating" systems. Once again, your ideas appear to have a mapping onto neural mechanism.

> At the lower levels we must have a lot of systems that are
> essentially always active (especially if you include the
> biochemical systems). But at higher levels we clearly have
> contexts. So I have speculated that the basic hierarchy related
> to a given context may be STORED, so that the same neural tissue
> can be used to support many different hierarchies.

Yup, this has emerged from several sources over the last few years. As I have mentioned, several people have proposed mechanisms for linking dynamic hierarchical templates (eg. von der Malsburg's fast acting synaptic correlations [von der Malsburg 1981]; cortical oscilations of 40-70 Hz which may act in a binding capacity to associate disparate regions by coincidence of processing in time). This becomes as issue of how short-term memories (STM) are encoded and subsequently converted to long-term memory (LTM). See this week's Science News [Ezzell 1991] for a good review of Daniel Alkon's work involving the molecular basic of learning and some speculation on STM and LTM.

> If there is any kind of processing appropriate to systems
> operating in the conscious mode (my way of putting it), it
> can't be ordinary perceptual processing. Ordinary perceptual
> processing is the same whether consciousness is involved or
> not. I think we still don't know what consciousness is for.

I agree with all of this, but let me propose that consciousness as I embodied it in terms of attentional processing may be resposible for facilitating reorganization by switching from a "direct access" or automatic mode to a serial search mode. Such a switch would require detection of pattern mismatch between levels. The serial search mechanism would them come under conscious control whereby the organism uses intentionality (instantiated neurally somehow) to guide reorganization.

Grossberg's Adaptive Resonance Theory (a pattern recognition neural network) uses an orienting subsystem to switch network processing from an automatic mode to a serial search mode in the event of a mismatch between an expectation template (reference signal) and the bottom-up pattern. [Grossberg S 1976a, 19876b; Carpenter GA, Grossberg S 1987]

> I don't limit experience to the highest level. We can focus
> attention on any level of signals, ...

This may be true for your levels -- I don't know them yet, but it is not true of the brain. We can not access our retinal, or cochlear representations, only abstractions of these.

> But all the other levels keep right on working, even if
> they're outside the searchlight's cone. This means that
> the ultimate "grandmother signal" -- the one that I would
> call the system concept of grandmother -- is always present
> and always determining the principles, programs, etc., that
> come into play when we deal with grandmother. One thing I
> DON'T do is to say, "well, Grandmother herself is the WHOLE
> THING," waving my arms and involking holistic magic. The model
> says that grandmother is represented by many signals at many
> levels, but it also says that at a given level, grandmother is
> represented in a specific way by a simple neural signal, or set
> of signals representing multiple attributes of the same level.
> Even though a higher-level concept of grandmother may be implicit
> in the multiple attributes, that higher-level concept has no
> operational existence until it is computed and represented as a
> signal (Frank Rosenblatt's principle). Furthermore, in a given
> collection of attributes, an infinity of higher-order concepts is
> implicit. Which one becomes explicit depends on how the signals are
> combined in a computation, not on how reality is organized.
>
> Is this relevant to what you were saying? I sort of wandered off
> on a different track there.

It is very relevant, but I still wonder if a root node to the grandmother hierarchy exists. Rosenblatt's principle is just fine, but could it be that when you think about your grandmother you are employing a linguistic subsystem to access ONE OF THE HIGHER LEVEL REPRESENTATIONS that form your conception of your grandmother. If you are in a visual mode you get an image, in an olfactory mode, maybe chicken soup. What is the use of a higher level representation that ties all of these together? Perhaps it is necessary to tie all of these together physically, but does this involve one node with top-down connectivity or lateral excitatory connectivity between a number of high-level representations.

> I'm delighted, by the way, to hear of a way in which such complex
> temporal patterns could actually be perceived by a single neurone.
> Although I do wonder what mechanism could make an output signal
> dependent on such a complex sequence of channel openings and

> closings. Are you being a teeny bit fanciful?

Theoretical neuroscience is usually at least "a teeny bit fanciful." Two hard-data neuroscientists (Richmond and Optican) have stirred up some controversy in the last few years with their ideas about temporally modulated spike trains. [Richmond BJ, Optican LM, Gawne TJ 1987] This discussion that we've been having has prompted me to extract one of their articles from one of my piles and actually read it. They demonstrate a number of things, both theoretically (statistics, information theory, signal processing theory), and experimentally (recording in striate cortex and inferior temporal cortex of rhesus monkey). Essentially they postulate that (some) neural signalling is accomplished by temporally multiplexed principal components of the distribution patterns of spikes during and after their response to a stimulus. Their arguments and data are strong. Some quotations:

> Several studies have shown that interspike intervals are not random ... even to tenths of milliseconds, and that structure in stimulus-elicited spike trains carries information....
>
> When information was calculated for these two codes [the modulated code and a conventional spike frequency code], the amount of information transmitted about the stimulus was approximately twice as great for the temporal-modulation code as for the spike-count code.... Furthermore, the amount of information in a temporally modulated code increases with the number of principal components included. This shows that the stimulus-dependent responses are multidimensional, i.e. more than one parameter is required to account for the information carried by the neuron.
>
> Since principal components are mutually orthogonal, this means that multiple messages are carried simultaneously by, or are multiplexed onto, the spike train.
>
> Our findings suggest that, when the multivariate nature of the response is taken into account, infomration about multiple features may not be confounded, as is assumed by the response strength/receptive field match hypothesis [of neural coding].
>
> ... in a space defined by the first three principal components the responses elicited ... [in rhesus cortex by a stimulus,] ... irrespective of duration [of presentation] or luminance, appeared to lie within a thin volume [i.e. aproximately planar].
>
> For a temporally modulated neural code to be meaningful, mechanisms for its decoding must exist. We suggest that the distribution of synapses on dendritic trees may be used to emphasize some temporal patterns of activity while de-emphasizing others.

I would call this RECOGNITION of a complex signal rather than a decoding, unless explicit representation and use of the components occurs.

> It is also possible that further stages of temporal filtering of the signals cold be performed by small local networks of neurons with reciprocal connections or feedback. In addition, the existence of multiple transmitters with different time courses of action within the same neuron makes another mechanism for temporal decoding possible.... If the release of two transmitters were differentially affected by the _strength and pattern_ of stimulation, the temporal modulation would be directly decoded by the synaptic mechanism.

> You are a very good guesser, although "extremely" is extreme.
> I am having a funny reaction to our conversation. For a long
> time, an unreasonable number of years, ideas like the ones we've
> been talking about have been floating around in my head, but the
> world I've inhabited hasn't included anyone else interested in
> the same levels of analysis. So all this neurological stuff has
> become interwoven in my mind with the behavioral model.

My feeling is that the only way for your model (or any other) to be elaborated into a testable, functional system is to go the route of neurally grounded modeling. Another guess is that you have done all you can do from a philosophical, theoretical and motivational standpoint. Where else is there to go with your theory? As the neural constructs as they are understood in 1991 begin to be elaborated in your understanding, a flood of more data driven work will burst forth from Powers. Does the mill need some grist? Or is it sufficiently supplied at this point?

> Now that you're here with a lot of notions about how the neural
> systems really work, I'm beginning to feel a sort of
> proprietarial twinge. I'm losing control of control theory!

It can be tough being a dad. There are two possible outcomes for the middle term: you will either be a gigantic fish in a small pond or a very large fish in a very large pond. Either way, you'll remain a proud dad.

> I can't help feeling a bit threatened -- what if neurones don't
> really work the way I thought? What if neurological research
> turns up some completely different organization, making control
> theory into a figment of my imagination?

Probably for the rest of our lives neuroscience will elaborate a large number of possible coding and computational mechanisms without pinning too many of the subsystems down in terms of actual, undisputable mechanism. For your work all this can provide is a plethora of ideas for instantiating the mechanisms you know so well. In addition, from my perspective your systems ideas are strong and map well onto other more neurally-motivated system ideas with which I am familiar, but -- again I am guessing -- your instantiations need more grounding in neuroscience in order for them to go anywhere.

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Date: Thu, 30 May 91 19:05:30 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Neural networks

[From Bill Powers]

Sorry, everyone, to be monopolizing the conversation with Joe (below), but I sense a bridge abuilding here that is vital to the CSG. Chime in, anyone who feels like it...

Joe Lubin (910530) --

Re the levels of perception:

>All of these experiences must be represented in the brain in terms of
>three general manifolds that jointly comprise the state space of the
>representations: a spatial manifold (topology, space constant within
>neurons), a temporal manifold (latency due to connectivity, time
>constant within neurons), for and a signal manifold (channels, currents,
>spikes, frequencies, temporal codes). Thus spatial relationships among
>neurons are fundamental to (i) the types of computations that are done
>and (ii) to the possibility of organizing any such computations in the
>first place.

We're not quite meshing here. I think you're working from the bottom up where I'm working from the top down (neither is better). Those classes of perception I mentioned are aspects of the "real world" that we experience. The "manifolds" you define are the result of applying those classes of perception to neural networks -- I mean you, personally, applying them, not the mathematical analysis applying them or the modeled networks applying them. The concept of "spatial" in "spatial manifold" is already a human perception. "Latency" implies the human ability to perceive events spaced out in time. And so on.

Let's take "latency" as an example as I attempt to explain this arcane recursive idea I'm trying to get across. Suppose there are two perceptual signals, one delayed with respect to the other. The second is a function of the first. The experiencer does not experience this as a delay. Instead, the two perceptions appear phase-shifted so that the second (which depends on the first) covaries not with the current value of the first signal, but with the value of the first signal at some previous time. But there can be no experiencable sign of that delay UNLESS THE DELAY ITSELF IS REPRESENTED AS A NEURAL SIGNAL. So delays in the network do NOT correspond to the experience of a delay. In order to experience the delay, there would have to be a circuit that does something like a continuous cross-correlation of the two signals, representing the first peak's delay in the form of the frequency of a signal. Then the amount of delay would have an experiential representation.

In the same way, computations based on adjacency relations are not experienced as adjacency relations. In order for that experience to occur, there would have to be a neural signal whose magnitude/frequency is a measure of the degree of adjacency of the physical components-- and I don't think that any such neural signals exist. If the connections

between the components were stretched so as to alter their physical adjacency, the computations would not be altered at all, and there would be no change in the experienced signals. The physical arrangements in the brain are not represented, as far as I know or can imagine, as neural signals. Therefore they are not themselves represented in experience. No doubt I'm just talking to myself and you already agree with this.

What I'm doing is dragging up out of the depths a very basic assumption that I've used and that I haven't thought about for a long time. It is that experience consists only of *present-time* neural signals: the "eternal now." The past and the future exist in the same way: as present-time sets of neural signals. NOTHING that is not explictly represented as a neural-signal analog is experienced. It seems to me that this puts network modeling and the modeling of perception into two different categories. The internal workings of neural networks with their "space constants" and "time constants" may underlie perception and account for it as an object of awareness, but these details do not appear in the experienced world. Only their outcomes, as signals, do.

This line of reasoning suggests to me that there are two main subjects involved in modeling neural processes. One is simply how neurons work -- what sorts of computations are possible in single neurons and functionally interacting aggregates of neurons. The second is what the building-blocks can be connected to do: that is, what kinds of circuits you can design using the neurons and neuron-aggregates as building-blocks. The second subject relates the neural level to my block-diagram level. I say, for example, that we need a type of circuit that can take as inputs signals standing for the size, orientation, and shape-parameters of configurations (themselves represented as signals), and generate as output a signal indicating that the configurations are changing in some manner that would be useful to represent in perception. This type of circuit would stand between the levels of perception (perceptual signals) that I call configurations and transitions.

I think there is behaviorally-observable evidence for saying that transition perception depends on the existence of configuration perception, but not vice versa. I offer this, tentatively, as a fact, a target for neurological research. At the moment I have no idea what sorts of circuits would be required to create the necessary relationship among signals. What I'm hoping is that those who are studying the brain can look at the neural networks, find the physiological places where the respective types of perception reside (and I believe these two have been identified), and from the neural structures deduce something about the computations that are being performed. This in turn will tell us a lot more about what we are trying to mean by words like configuration and transition, so we can refine our experiments, learn more about the evidence of the relationship, and throw the ball back to the neural researchers. So we will bootstrap our way through the nervous system, learning simultaneously what it does and how it does it, each aspect informing the other.

This could go on forever so I'll just cut it off and get to one other subject from your post that relates to the above.

> ... but I still wonder if a root node to the grandmother hierarchy
>exists. Rosenblatt's principle is just fine, but could it be that when
>you think about your grandmother you are employing a linguistic

>subsystem to access ONE OF THE HIGHER LEVEL REPRESENTATIONS that form
>your conception of your grandmother.

It isn't a "root node" that I'm talking about, if I understand what you mean, but precisely what you say: one of the higher level representations. What I call a system concept is a higher-level representation that is a function of many lower-level representations -- principles are the next-lower, I think. But the sense of grandmother that you get as a system concept simply does not exist at any lower level. It's like moving around the country and paying your gas, electric, and water bills at various offices, and seeing that there are similarities in the way they work, the functions they carry out, their relationships to your facilities at home -- all without ever putting it together into the concept of "utilities." "Utilities" is a perception at the system concept level. You can perceive all the elements of utilities without having this system concept, but you can't perceive the system concept without having perceived the elements. There is a "utilities" neuron (circuit, I suppose). You have to have it in working order, receiving the right inputs, before you have a signal to which you can attach a linguistic pointer such as "utilities", and more to the point, before you can get that sense of a unitary entity that I call a system concept. If that level doesn't exist, there's no system concept to be experienced. Only its elements are experienced.

I may be misconstruing you, but in many network-type discussions where "nodes" are mentioned, the node is a link to sub-nodes, so one visualizes the connection as proceeding from node to sub-node. In my perceptual model, the higher-level signal depends on a set of lower-level signals, convergently, in the manner of a many-to-one function. It's like running one of those logic trees from child to parent instead of the other way, with the leaves being the source, not the destination. [This is one reason why I haven't seen a way to use object-oriented programming in representing my model: it starts from the top and elaborates into more and more specific instances of the objects, where what I want works in the other direction.]

Well, one last comment --

In the quotes from [Richmond BJ, Optican LM, Gawne TJ 1987], I get an odd sense that they're saying something obvious in a complicated way. I get the impression that they're not thinking of neural signals as dynamic analog representations of a continuously-changing world of lower-level signals (or external stimuli), but more along the lines of categories: a signal indicates a class of event. When we think of neural signals as continuous real-time analogues, we don't expect to find simple trains of spikes, but pulse-trains with continuously variable spacing that can present any pattern whatsoever. When the authors say, to quote you,

"that (some) neural signalling is accomplished by temporally multiplexed principal components of the distribution patterns of spikes during and after their response to a stimulus"

... I can't imagine how that is different from saying that the varying output signal (varying in terms of spike frequency) is some function of a set of input signals (each varying the same way). Maybe one can observe that in the pulse-train some spikes can be identified as coming from one source and some from the other, but this is by no means "multiplexing."

It is simply adding frequencies -- at the destination, these spikes will be smoothed out into post-synaptic potentials, and smoothed even more into membrane potentials (thank you for the distinction; I get it, I think). There's no demultiplexing at the destination to separate the two contributions again, so this isn't really multiplexing at all. I think these authors are getting too fancy -- unless they have some very good reason for choosing this interpretation that I don't know about.

Another quote:

> Several studies have shown that interspike intervals are not random ... even to tenths of milliseconds, and that structure in stimulus-elicited spike trains carries information....

I'm nonplussed. When you subject a sensory ending to a constant stimulation, the pulse frequency will be quite uniform, in some cases after an initial exaggeration of the on-transient. Where does the idea come from that the pulses ought to be randomly spaced? This doesn't fit any model of a neuron that I know about. OF COURSE the structure in the pulse train carries information: it represents the magnitude of the stimulation, which in general will be continuously varying. In higher-level neurons, it represents the fluctuating value of a function of a set of varying input signals. What's the alternative?

From your comment,

> would call this RECOGNITION of a complex signal rather than a decoding,
>nless explicit representation and use of the components occurs.

... I rather guess that you agree with me, mostly.

As to the encouragement and support expressed at the end of your post, I can only say that without words like these from people who know what they are talking about, I would probably have gone nuts long ago. Thanks.

Love, Dad.

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Date: Fri, 31 May 91 07:28:17 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Anxiety

While Bill Powers and Joe Lubin continue to plumb the depths of the neuron(e), I thought that I would give the clinicians something to play with.

David Goldstein a while back presented a case concerning Gail with the psychological lump in her throat. There was much discussion about the control theory interpretation with of course conflict being a major candidate as the cause and elimination of the conflict as the cure.

My question relates to anxiety. I can understand how anxiety seems to be explainable as resulting from conflict. If I'm afraid of something, I should just run away and avoid it. But if doing so brings me closer to another situation of which I am afraid, then I've got a problem with two opposing control systems and, voila, anxiety.

Now we come to the heart of my question. Even if the original source of conflict can be found and eliminated, it is possible (I've seen it) that the person can still remain ANXIOUS ABOUT BEING ANXIOUS, particularly if he's experienced anxiety attacks. The person then could be afraid of any public exposure since he immediately starts thinking "what would happen if I freaked out here" and of course, thinking about freaking out may be just what's needed for a freak out. So, even if the original source of conflict can be found and eliminated, that original conflict has now become irrelevant, hasn't it?

It would almost seem as if some type of desensitization would be needed here. A walk up and down the block--nothing happens. A walk around the block--still fine. A trip to the corner store; shopping center; back to work, etc.

Any ideas about this? --Gary

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Date: Fri, 31 May 91 09:28:11 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: Neural networks

Bill (910530)

>Sorry, everyone, to be monopolizing the conversation with Joe (below),
>but I sense a bridge abuilding here that is vital to the CSG. Chime in,
>anyone who feels like it...

No problem--I hate coming in the middle of a construction project anyway. Let us know when you have the ribbon cutting ceremony. Bridges are useful for going places.

Joel Judd

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Date: Fri, 31 May 91 09:46:06 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPROBER@BOGECNVE.BITNET
Subject: comment on mtaylor on learning

[from dick robertson]
You raisd a questiion a few weeks back about learning that I didn't get to post before going away. I hope my comment is still relevant. I noted that Bill Powers replied in a somwhat different vein. I will go ahead and send my earlier post for what its worth.

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Date: Fri, 31 May 91 09:49:07 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPROBER@BOGECNVE.BITNET
Subject: comment to M Taylor's post on learning

To: Martin Taylor Re - Error Production, Disciplined Inagination
[From Dick Robertson]
You said >There is a fairly common real-life parallel to this effect that has always interested me...< in reference to Rick Marken's (910509):

>>The closest thing I have seen to what looks like intentional production of
>>(technical) error is found in my article with Bill Powers in Hershberger's
>>Volitional Action book. In the polarity reversal experiment there is a
>>1/2 second period where the subject actually makes things WORSE -- increasing
>>the discrepency between target and cursor in an accelerated, positive feedback
>>sort of way. When you are a subject in this experiment you can actually feel
>>it happening "against your will"....But this happens only because the higher
>>level systems cannot correct things fast enough. It is explained just fine as
>>the behavior of a two level negative feedback control system that is trying
>>(as always) to minimize (technical) error.

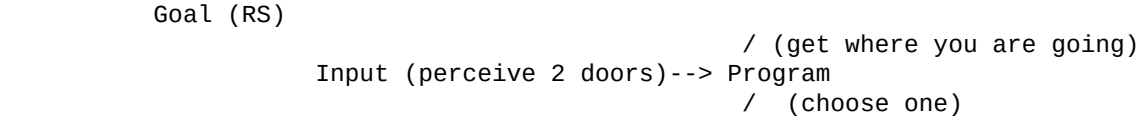
>...I tried to get the people in this human-factors institute
>to explain it to me when I was a student here 30+ years ago, and I still
>haven't found anyone with an answer. Maybe CSG people have an answer.
>Here's an example situation; there are parallel situations where the same
>thing happens, but I think one example is enough:

>In a certain corridor there is a doorway with two swinging doors (i.e. when
>both are open it's a very wide doorway, but opening one is all one needs
>to do to pass through.) Usually, one of the two doors is locked shut,
>and people pass through the other. It is always the same one that is
>locked. The first time someone encounters this doorway, they may go through
>the openable door, and if so, everything is OK thereafter. But if on
>this first encounter they try the locked door, and then move to the
>unlocked one after failing to get through the locked one, there is trouble.
>For a long time thereafter, even if they use the doorway several times
>a day, they are liable to try the wrong door first. The subjective
>impression (it has happened to me a few times) is that one mentally
>oscillates "I know it isn't the one I first thought it was, which means
>it is not that one, because I think it is that one, so it must be the
>other one....BANG!" It seems that the more times one goes through this
>routine, at least for perhaps tens of experiences, the more likely it is
>that one will eventually choose the wrong door. It's a very frustrating
>thing, very common (other people confirm it happens to them, and it can
>be observed casually), and without any explanation that has satisfied me yet.
>Is there a CT explanation? It sounds a bit like Rick's experimental
>situation, though that corrects itself more quickly.

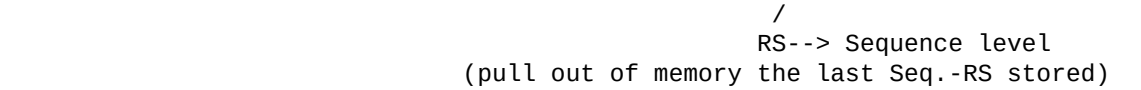
This brought to my mind Edwin R. Guthrie's theory of learning (that we learn wha we do)- which was the only learning theory I ever got very excited about eons ag when I took my first course in it. Guthrie's wasn't much of a theory; it consisted mainly of "explaining" learning by postulating that what we observe to be the facts are the facts (like most psychological "theories"), but the

particular observations that he described were very concrete and very practical. I still use them when I want to teach somebody anything that we call "skill." When I want to teach any of my students word processing (e.g.), I don't bother showing him or her anything. I have her/him sit at the keyboard and I give verbal instructions as to what to punch. Providing that I don't screw up the instructions (and the subject doesn't innovate) s/he never makes any move or per illustrations by which I've been persuaded that old Guthrie was right: we construct memory (of the skill we want to acquire) out of the perceptual signals we have to produce in order to satisfy our references.

You might see how we could begin to forge an answer to your question about what CT might have to say about this as follows (this is a first approximation):



(Now here we come to an interesting situation; you described an intricate set of cognitions that you go through - I, on the other hand often find that I have no conscious awareness at all until the moment the BANG occurs, I have been thinkin of whatever I am doing and my actions would seem to imply an assumption that my body "knows" which door to head for (or whatever other similar situation applies). In either case, though, I think it's just a matter of what level of variables each of us typically monitors in such situations, so continuing the Program)



As you say, if you lucked out the first time you seem to make the same move agai and never get the Program enlarged by some decision-making components, but if yo didn't luck out (and you're like me) you repeat the previous movement and THEN get hung up and incur the conscious decision-making stuff.

I think Guthrie would have been an early CSG convert if CT had been around at th time he was.

This doesn't seem to me to apply directly to Rick's report that you quoted, because in his case the subject is reorganizing at the point referred to. In th case of your illustration I think reorganization hasn't begun until several more or less automatic attempts to run through the program have gotten stymied.

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Date: Fri, 31 May 91 11:38:44 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject: Re: Neural reps

[From Joe Lubin (910531)]

Bill Powers (910530)

> We're not quite meshing here. I think you're working from the
> bottom up where I'm working from the top down (neither is better).

Yes.

> Those classes of perception I mentioned are aspects of the
> "real world" that we experience. The "manifolds" you define are
> the result of applying those classes of perception to neural
> networks -- I mean you, personally, applying them, not the
> mathematical analysis applying them or the modeled networks
> applying them. The concept of "spatial" in "spatial manifold" is
> already a human perception. "Latency" implies the human ability
> to perceive events spaced out in time. And so on.

Some bottom-up language:
I use spatial to mean nothing more than neurons are arranged in the brain with great regularity. There are spatial organizing principles on multiple scales from how neuron communicate with one another in a crisply defined structural syntax, to the orientations of neurons relative to cortex (pyramidals with axons down, dendrites up), to horizontal layers, vertical columns, spatiotopic organization, modality regions, and hemispheric symmetry.
Similarly I take latency to mean the neural signaling delays or the time course of a signal due to passive ionic current spread, active ionic currents, local circuit interactions, intercolumnar interactions, and interregional interactions.
Essentially they are complementary manifolds (I would use the word dimensions, but they really embody a number of dimensions so I went for the more general term.) By complimentary I mean that one can stand in place for the other in a given computational framework: the time/space computation tradeoff.

> So delays in the network do NOT correspond to the experience of
> a delay. In order to experience

I did not mean to imply this.

> In order to experience the delay, there would have to be a circuit
> that does something like a continuous cross-correlation of the two
> signals, representing the first peak's delay in the form of the
> frequency of a signal. Then the amount of delay would have an
> experiential representation.

Yes, it would have to be an explicit computation. There are known circuits which compute various types of delay quantities for both auditory and visual modalities. Much work has been done on the barn owl's spatial localization system which explicitly employs interocular delay detection.
Considering a spatial localization computation or a motion computation as a type of delay computation might appear contradictory but the underlying computations are probably similar, they are just made into different explicit representations. I use this example because I don't know where or how delay, per se, would be computated or represented.

> In the same way, computations based on adjacency relations are not

> experienced as adjacency relations.

Yes, again.

> The physical arrangements in the brain are not represented, as far
> as I know or can imagine, as neural signals. Therefore they are not
> themselves represented in experience.

Yes, but by being fundamental to the computing substrate, they
constrain experience. (Note that I am a strong proponent of
spatial computations in the brain. There is a bunch of very
illuminating work being done in this regard.

> What I'm doing is dragging up out of the depths a very basic
> assumption that I've used and that I haven't thought about for a
> long time. It is that experience consists only of *present-time*
> neural signals: the "eternal now." The past and the future exist
> in the same way: as present-time sets of neural signals. NOTHING
> that is not explictly represented as a neural-signal analog is
> experienced.

Well said.

> This line of reasoning suggests to me that there are two main
> subjects involved in modeling neural processes. One is simply
> how neurons work -- what sorts of computations are possible in
> single neurons and functionally interacting aggregates of
> neurons. The second is what the building-blocks can be connected
> to do: that is, what kinds of circuits you can design using the
> neurons and neuron-aggregates as building-blocks. The second
> subject relates the neural level to my block-diagram level.

This is a good first-order approximation.

> The second
> subject relates the neural level to my block-diagram level.

Sounds reasonable.

> I think there is behaviorally-observable evidence for saying that
> transition perception depends on the existence of configuration
> perception, but not vice versa.

I would not be so quick to make this statement. What
"behaviorally-observable evidence" are you invoking? And why are
you invoking "behaviorally-observable evidence" in the first
place (more of my caution in mapping what we perceive onto what
we think must be the underlying computations)? If
motion detection is an example of a transition perception then
your statement is probably false. Visual motion and spatial
information can be extracted computationally without employing
explicit configuration perception constructs (shape

representations). In the brain, these computations are separated into different systems (the dorsal and ventral systems) -- which only implies that the higher-level configurational perceptions are probably not used to extract spatial and motion information. I think your vice versa is correct.

> I may be misconstruing you, but in many network-type discussions
> where "nodes" are mentioned, the node is a link to sub-nodes, so
> one visualizes the connection as proceeding from node to sub-node.
> In my perceptual model, the higher-level signal depends on a set
> of lower-level signals, convergently, in the manner of a
> many-to-one function. It's like running one of those logic trees
> from child to parent instead of the other way, with the leaves
> being the source, not the destination.

Yes, this is a bottom-up, data-driven characterization of a hierarchy. My trees are upsidedown just like yours: data comes in the leaves and is abstracted towards the root node(s). The primate visual system is likely composed of two of these hierarchies. There is a very significant augmentation, however: almost all visual areas that communicate with one another do so bidirectionally -- that is, the top-down projections are often similar in size to the bottom-up connections. The close-the-loop group should have some nice ideas on what to do computationally with these loops. I'll tell you what I think they are in Durango.

> In the quotes from [Richmond BJ, Optican LM, Gawne TJ 1987], I
> get an odd sense that they're saying something obvious in a
> complicated way.

It is much more significant than what I was able to convey without typing in the whole thing. They present some radical ideas with good support theoretically and experimentally. Get the paper -- its not a tough read. If you want me to send it to you send me an address.

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Date: Fri, 31 May 91 08:46:36 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Neural reps

[From Rick Marken]

OK, I've got a free moment so I'll try to get into this neuron's/

consciousness discussion. I'll do so by commenting on some of Joe Lubin's suggestions because I always agree with everything Bill Powers says (now, watch him test this by suggesting that Rick Marken is an idiot; well, perhaps I can prove him right in advance with this posting).

Joe Lubin (052991) says:

> We can not access our retinal, or
>cochlear representations, only abstractions of these.

I don't understand this. We simply experience. The physiological evidence and models suggest that all neural representations (from the retina, up) are abstractions of what our current physical models tell us is reality. I take the word "abstraction" to mean an representation of only certain aspects of one reality (in this case, the "outside world") by another (in this case, neural activity). Are you saying that our experience is like a second process of abstraction which takes one reality (cochlear neural activity) and represents only some aspect of it by another (awareness)? This may be the case but I don't believe there is any psychophysiological evidence on this point (though there is certainly evidence for the first kind of abstraction -- from outside world to neural activity. I imagine that awareness is somehow "in" the second half of this first abstraction. A certain level of neural activity (and I'm inclined to think of single neuraon firing rate as the significant physiological variable here) simply is a particular experience (a level of loudness, for example) from the point of view of the brain. I imagine that the world that I experience is just what neural activity looks like when you ARE a brain. Awareness is still a problem because it is obvious that I can become aware of different aspects of my experience "at will". So something is moving from one area of neural activity to another -- probably other neural activity. But all these experiences (qua neural activities) are there as long as the neural activity is there.

>My feeling is that the only way for your model (or any other) to
>be elaborated into a testable, functional system is to go the
>route of neurally grounded modeling. Another guess is that you
>have done all you can do from a philosophical, theoretical and
>motivational standpoint. Where else is there to go with your
>theory? As the neural constructs as they are understood in 1991
>begin to be elaborated in your understanding, a flood of more
>data driven work will burst forth from Powers. Does the mill
>need some grist? Or is it sufficiently supplied at this point?

and then:

> In addition, from my perspective your systems
>ideas are strong and map well onto other more neurally-motivated
>system ideas with which I am familiar, but -- again I am guessing
>-- your instantiations need more grounding in neuroscience in
>order for them to go anywhere.

Since you are a neurophysiologist, I can understand why you would feel this way. Since I am a, well, a psychologist, perhaps you can understand why I would disagree with the above statements -- at least partially. I do think that evidence from neurophysiology (I think this is what you mean by "neural grounding") will definitely put constraints on Bill's control model. After all, the model is, at least in part, a functional representation of the behavior of the nervous system, which we know to be the system that is

responsible for the phenomenon of control that we see being exhibited by living systems (I say "in part" because I think that an important aspect of Bill's model is an attempt to represent the behavior of our own experience; much of this behavior is surely neurally based, but the aspect of the model that is watching and modelling this phenomenon is also part of the model, and that part is less obviously part of the neural model -- at least for now).

So, while I agree that neural findings constrain the details of the model somewhat, I do not believe that the model should be based on those findings. Indeed, those findings, being experiences themselves and contingent on the accuracy of current models of physical reality (which are tentative -- remember, we used to think Newton was right) are themselves data to be handled by the control model itself! I don't want to seem too arcane about this point; what I'm saying is really very simple. Control theory is a model that is designed to account for a phenomenon (control). To the extent that that model implies something about the system that makes control possible (the nervous system) then the behavior of that system becomes a phenomenon of interest to the model. Just as new findings about the phenomenon of control (such as temporal relationships between controlled variables, types of controlled variables) will influence and, sometimes, require changes in details (or more) of the model, so too will new findings about the nervous system require (maybe) changes in the details of the functional model itself. But these changes would only be made if the functional model, by virtue of these changes, does, therefore, a better job of mimicing the phenomenon of control. If neural evidence requires changes in the model that make it a poorer model of control, then there would have to be some serious re-looking at the neural evidence, the phenomenon of control and the model itself. Your knowledge of neural processing can certainly "keep us honest" by limiting, to some extent, our speculations about how the functional model should work. But I doubt that we would change the functional model, based on neural evidence, so that it did not behave correctly.

This last comment reminds me of a little neuro-paradox I once thought of. I realized that it would be impossible for a neural researcher to show that the nervous system was incapable of representing a particular aspect of one's own experience. How could you have that experience if you can't have it?
The paradox is this: suppose that a neural researcher
puts his electrode into every neuron in the brain and finds not one that responds when the letter X is presented to the retina. He finds that this is true for all brains he has studied and is convinced it is a general characteristic of brains. This would be a pretty chilling discovery since the researcher is sitting there looking at the letter X. Obviously, in such a case, I would think that the researcher would trust his EXPERIENCE of the letter X and start reconsidering the neural evidence. Neural grounding could produce absurd results.

Best Regards

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Date: Fri, 31 May 91 17:06:15 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Artificial cerebellum; misc comments

[From Bill Powers]

Joe Lubin(910531) --

Here's an example of how the system-level modeling I've been doing interfaces with neuroscience-level modeling.

Years ago I worked out an algorithm that derives graphical transfer functions (non-analytic) from real-time tracking data. The method involves a lot of computation, and is hard to explain, so we eventually went back to simpler models that worked as well as we could ask. But the transfer-function method kept dragging me back to it. Last year I figured out how it could be incorporated as part of a control system to make the system adaptive (self-stabilizing). Some reading about the cerebellum that I had done earlier suggested that the algorithm could be translated into a feasible neural circuit that works in a way suggestive of the mechanisms in the cerebellar cortex (parallel fibers, climbing fibers, Purkinje cells, stellate cells, basket cells).

This led to a project last summer in which I put this "artificial cerebellum" into the model of a robot arm that Greg Williams and I had been developing, as a sort of spin-off from the main effort. The arm model that I used is two-dimensional, bending only at the shoulder and the elbow, but it does have mass and employs the correct dynamical equations. There are four control systems, two for each joint. One controls velocity, and the second controls position, for each joint. When the program starts, the gains of all control systems are zero. The "artificial cerebellum" keeps computing a transfer function and correcting it as the arm is given varying reference angles for the two joints. At first the arm is limp; then it begins clumsily tracking, and as time goes on it gets better and better at it. The transfer functions in question are actually the output functions of the four control systems. The algorithm uses no information but the error signal in each control system. The transfer function is a table that yields an output by convolving the transfer-function table with the error signal delayed as appropriate. The correction method is VERY simple, and also uses only the error signal information.

I can show you this demo at the meeting in August, and also explain the algorithm and how it translates into neural circuitry. But I can't take the last step, which is to show that the connections in the cerebellum really are organized this way. You might find this an interesting enough side-project to collaborate with me on filling in the neuroscience part and co-authoring a paper with me.

Bill Cunningham (910530) --

If you'll post your mailing address, I'll send the disk I have ready for you.

Are you thinking of trying to use the hierarchical model as a model of a

system composed of separate individuals? If so, it would be well to remember that EACH individual in an organization has ALL the levels of organization -- it's essentially impossible to get them to act as if they have only one level of functioning. For modeling organizations, I think you need something more like what the System Dynamics people are doing -- George Richardson at SUNY Rochester would be a starting point. I don't have his address handy (nothing is handy right now) but you can find him through the literature.

Loose ends --

Rick Marken --
The book sounds like a great idea, and I will certainly want one for myself (and buy it!). All those ingenious experiments should be collected together in one place.

Gary Cziko --

"Rats rolling in a maze" reminds me of a story told by someone who knew Lashley's lab assistant (do I mean Lashley? The non-localization guy). After the animals had big chunks of brain removed, they were put in the maze and lurched, staggered, crawled, and rolled through the maze, and Lashley (?) turned to his assistant and said "See? No effect." Wish I could remember who told me that. Might have been John Stroud. No, I just can't recall.

Best -- Bill Powers

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Date: Fri, 31 May 91 17:40:46 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Testing against experience

[From Bill Powers]

Rick Marken (910531) --

Rick, you have said a very important thing, and Joe Lubin, I hope you heard it clearly, if only to pass it on as a great way to state the issue. Rick is saying that experience is the final arbiter when it comes to brain models -- not just experience of images in a microscope or traces on an oscilloscope, but experience of the world and one's inner self. This is why functional models will ALWAYS be important. Experience tells us what is there to be explained; function models put experience into some rational form that can be checked experimentally to see if a model's implications fit experience. Of course the blocks that make up functional models can be realized in the nervous system in an immense variety of ways, so the functional model does not reveal how the functions are created. But if the functional model tests out against experience with sufficient accuracy, it can provide a way for neural researchers to conceive of the system they are trying to explore.

You have to have some idea of what you are looking for before you can find it. A very large amount of neurological research has been done on the assumption that we are looking for stimulus-response chains (which of course have been found). Essentially none has been based on the idea that

we are looking for control systems (at least that's my impression). I would go even farther, and say that you always find what you are looking for in neural research -- it's too easy to create put-up experiments that are designed to favor a preselected interpretation. That's why it's essential to test the interpretations at the behavioral level, to see if they fit into any model that actually works.

Global conceptions of what behavior and experience are quide neurological research; I don't think that analysis at the neural level alone will ever lead anyone to recognize the larger functions that are being carried out. Sometimes I think that the tendency goes the other way -- to admit the existence only of what can be seen one synapse at a time, and reject the idea that the larger functions even exist. Of course I'm talking over your shoulder to someone else, Joe -- I'm sure that you're already on our side with respect to this "reductionism" issue.

I'll be off the net now until Saturday afternoon, because the University of Colorado is going down for the next 18 hours. Some of you may not think that 18 hours is a long time.