CSG_9102

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Date: Fri, 1 Feb 91 15:10:46 EST
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: PRAGMATIS, ACCEPTANCE OF CT AND WAR

Chuck Tucker (910201): Pragmatism; Acceptance of CT; War

Cziko (910130)

I am continually amazed (but should not be) at the misrepresentations of the ideas of John Dewey especially by some of those in schools of education (I am not referring to you, Gary, but those who have told you about Dewey's ideas - I had a continuous battle with my father-in-law about this until I had him read some of Dewey's works and then he thought that Dewey was an idealist). How could a person who wrote "The Reflex Arc", <<The Logic of Inquiry>>, <<The Quest for Certainty>>, as just part of this 38 volumes of collected works which, from my reading in them, are consistent from about 1897 through 1951 (he died in 1952 at age 92) be characterized as Lamarchian? Dewey is the philosopher of this century (although he gave this honor to Geroge Mead) and he is the important developer of pragmatism. My view of pragmatism comes out of Dewey-Mead-Bentley-Pierce-James work. Dewey's notions about education are throughout his writings but an important document is his <<How We Think>> (be sure to look at both versions since his revised edition answers the very view that you report). Dewey's notions of all human acitivities is based on scientific inquiry which is pragmatic or a problem solving approach - he is very consistent about this (see his letter to a reader in the appendix of <<Knowing and the Known>>) and very critical of absolutist, dualistic and static views of scientific method (which is what most social scientists use today). But one has to be careful when reading Dewey (and very careful with Mead) since his style was to state the positon that he was opposing and then submit it to analysis. Thus, he may state the absolutist postion and then evaluate it. If one fails to read further or know about this tactic he/she may mistake the opposing view as Dewey's view. My bet (to be generous) is that some distortions occur when readers fail to note which view is Dewey's and which he is opposing.

Acceptance of CT: Cziko (910130); Marken (910130); Judd (910131)

Bob Stewart, Clark and I have been struggling with an explanation or account of the failure of even our students to accept our view (which is quite consistent with CT although developed out of our looking to pragmatists for assistance is solving our problems). We believe some of the lack of acceptance can be accounted for simply by a lack of interest or seriousness about understanding social life, some of the people simply believe they have "the answer" and therefore don't even understand what the question is or the reason anyone would have any question about living systems, some are not able to tolerate the "disturbances" that

accompany the effort to develop some answers to question while some are simple not skilled enough (being kind again) to do the work required to pursue the issues [just think about the fact that most of us on this NET have been working on these issues and problems for most of our adult life and still have to admit that we have made only some progress and can't answer some questions that others claim (wrongly) they can]. We think that the majority of those who have difficulty accepting our approach simple hold to the assumptions about the world that Dewey attacks in <<The Quest for Certainty>> and that we reject with our approach: that the real world will be revealed to you if you just use the "proper" methods and work hard enough [this is the "realism" discussion we had before]. If you tell these people that your approach won't reveal the "true forever world" then they seem to have much less interest in what you have to say especially when you are asking them to question that very belief. Another feature of many of those who reject our view is that they are not "problem oriented" - that is - they do not tolerate ambiguity, uncertainty, and problem solving activity for very long - they want the answer quickly and cheaply (or statistically). But our approach does not offer such a magic solution but just hard dirty difficult work with no absolute assurances that a solution will be fashioned let alone work. Think about it - would you give up such a pleasant life of certainty and bliss for the one we offer - probably not. We should be thankful that there are a few of us that continue to work on these issues. That is why I keep telling all of us on this NET how fortunate we all are to know one another and to keep up this work. Does anyone have any other speculations about this "acceptance" issue? I would like to read them.

War: Marken (910130); Judd (910131)

My hope was that we would agree on the issues related to the War in the Gulf and it is comforting to read that I was mostly accurate. We should figure out ways that we can encourage people - especially those in the military - to view war as the last choice after all other procedures have been seriously tried. Now there is a project for us to work on - how many lifetimes do we have for this one?

Finally

I have not heard lately about Mary's progress - How is she doing? I would also be interested in her comments on any of these matters we have discussed of late. We should be willing to move our discourse up a few levels! I hope all is well.

HOPE FOR PEACE CHUCK TUCKER N050024 AT UNIVSCVM

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Date: Fri, 1 Feb 91 20:06:32 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Dennis_Delprato@UM.CC.UMICH.EDU
Subject: Acceptance of New Idea(Different) Ideas

FROM Dennis <DELPRATO@UM.CC.UMICH.EDU>

Chuck Tucker:

You ask about ideas re. acceptance of what amount to as naturalistic approaches to human psychological and social events. I've said it in this forum and elsewhere--with no evident acceptance:

Sheer data do not speak, data cannot exert power to change human behavior. Rather and especially in the case of human psychological and social activities, indispensable is historico-critical examination of the sources of thinking that function as impediments to even thoughtful appreciation and consideration of departures from culturally transmitted ways of approaching humans. Again, I cite Mach's mechanics and Kantor's Scientific Evolution of Psychology. In the case of human behavior, we are up against more than 2000 years of cultural tradition, much of it institutionalized. The de-mystification (i.e., secularization) of human behavior is not to be taken lightly. The mainstream approaches with cause-effect, mentality--->behavior, Envt---> Cognition--->behavior, et al. are carrying on a powerful cultural tradition, a tradition for which many gave their lives either in defense or because of their opposition. One of the many eye-openers here is to consider the Father of Experimental Psychology. Naive observers assume that the first experimental psychologists of the modern era were thoroughly scientific, and they simply needed to shape up their science (methods, theory, and so on). FAR FROM IT: The Father (Fechner) was a profound mystic whose fundamental aim was to "prove" his view that the ultimate reality was the spiritual. What kind of heritage is this? The science of human behavior founded to support supernaturalism....

Dennis
Delprato
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Date: Sat, 2 Feb 91 13:01:13 -0600
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: chaos and the brain

Have all of you acquired a copy of the Scientific American article that Bill mentioned? (The Physiology of Perception, by Walter J Freeman, Feb 1991). WOW! What an incredible article! I also read "How Brains Make Chaos in Order to Make Sense of the World" by Skarda and Freeman in Behavioral and Brain Sciences, Vol 10 (2), which the former article suggested as further reading. There's a lot here to talk about. Some of it we've talked about before.

Here's a few of the number of questions I have. First, Freeman speaks of his model being connectionist, rather that info-processing. I didn't realize that they were considered exclusive categories. If this is presently so, what is CT? I've always thought of it as both. Is it a

serial vs. parallel distinction?

Second, if I blindfold you and then place a pizza under your nose, is there anything wrong with saying that bringing the pizza up to your nose (the pizza "scent" to be clearer) is the stimulus and your awareness (or identification) of the pizza is the response? This is my ATTENTION question again--it seems fundamentally different than BEHAVIOR.

Bill, since you answered my question before (910109) about "I didn't know I wanted it until I saw it--the attractive woman problem" and you have read this article , maybe you have some new or confirmed ideas on the matter. Last time you gave three possible solutions. (1) Something like "AS the effects of output on input become minimal, error sensitivity decreases, and hence output decreases. (2) As error increases, higher order systems switch over to other lower level systems (mate-finding to card-seeking). (3) Reference signals come in as negative (inhibitory) while perceptual signals are excitatory (this explanation I did not understand, and I would appreciate it if you could explain it again sometime)

Anyway, option (1) seems to be referred to in some way on page 81 of the first article. There is reference to the Hebb Rule: synapses between neurons that fire together become stronger, as long as the synchronous firing is accompanied by a reward. Am I correct in assuming that this is related to what you (Bill) were referring to?

I couldn't help but notice that despite the S-R base, there was a lot of CT talk in the articles, especially the latter. There's even a section on "The sensory/motor loop. In this section, "existing prototypes" are referred to; why wasn't 'Powers' referred to? Who is Walter, Ashby, and Grossberg, anyway?

I'd like to hear comments before I say anymore, especially since there is no real direction to this post. I feel as if the chaos info from these articles "confirms" physically what I've been thinking conceptually. Gary, you will appreciate the comments in the articles in relation to chaos as a driving force for the "trials, in a trial and error process."

One last thing, pay attention to the comment on page 84, third column : "...an act of perception consists of an explosive leap of the dynamic system from the "basin" of one chaotic attractor to another."

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Date: Sat, 2 Feb 91 15:24:32 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Dennis Delprato <USERXEAK@UMICHUM.BITNET>
Subject: Reprint Request--Tom Bourbon

FROM Dennis <Delprato@um.cc.umich.edu>

Tom Bourbon:

I'd appreciate a reprint of Bourbon et al. (1990) ì, "On the accuracy and reliability of predictions by control-system theory" from Perceptual & Motor Skills.

Dennis Delprato
Department of Psychology

Eastern Michigan University
Ypsilanti, MI 48197

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Date: Sat, 2 Feb 91 21:59:05 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Freeman, SR, chaos.

Mark Olson (910202) --

I wouldn't start doing back-flips about Freeman's concepts. He writes so as to get a WOW reaction, but so does any good science-fiction writer, some of whom are scientists. He hasn't actually explained anything about either perception or recognition. All he has are some synchronous nerve firings, some topographic maps of nerve activity, and a computational model that may or may not have anything to do with what is going on in the olfactory bulb. He's shown that there are typical patterns of activity that change with the scent, but different patterns don't amount to different perceptions unless there is something that can respond differentially to the different patterns. In other words, we still need a pattern perceiver, so I don't see that the topographic patterns bring us any closer to understanding perception. I think that the patterns are probably side-effects, having no intrinsic significance of their own. Like the flashing lights on the display of a pinball machine. I think we should be watching to see what the ball does.

I feel drawn toward believing that he has something, but that's mostly from his skill at presentation, to which I'm as susceptible as anyone. His data aren't connected either to perception or to behavior. His invocation of chaos is unconvincing to me. Anyone can find phase-space plots in any physical phenomenon (plot any variable against its first derivative), and if this phenomenon is taking place inside a very complex system, you're going to get non-repeating but sort of regular trajectories in phase space. Maybe that's all you need to demonstrate the presence of a strange attractor, but if it is, chaos doesn't amount to much.

I'm glad that Freeman is doing this very detailed sort of investigation of neural phenomena. I'm sorry he is doing it with EEGs, which are a blunt instrument. I'm suspicious of his free-swinging explanations and his symmetrical network of second-order differential equations. I say, wait five years and see if he goes off just as enthusiastically in a different direction after this one fails to pan out. My nose detects trendy science. On the other hand, some kinds of cheese smell worse than they taste. I have an uneasy feeling that Freeman is smarter than I am. That doesn't make his idea right, but it does affect the odds. I'll keep my mind open just a crack.

Someone else will have to distinguish between connectionism and information processing. I get the vague feeling that information processing is mostly verbal, while connectionism is more slanted toward working models of brain functions. In that case, control theory is more like connectionism. I've always thought of connectionism as being the discovery that connections in the brain are important. Back to analog computing, but now it's different: it used to be spelled "analogue."

If you put a pizza under my nose and I become aware of a smell, is this an

SR effect? Yes, it is. All the individual functions that make up the inner part of a control system are SR subsystems. Another word for an SR effect is FUNCTION. The output is a function of the input. We use the same word to refer to the physical device that imposes this functional relationship: perceptual function, comparison function, output function. That's really shorthand for saying "device that creates an output signal that is a particular function of the input to the device." The perceptual function is a neural device that creates a perceptual signal that is some particular function of a set of inputs from either lower-level systems or from sensory receptors, depending on the level. It's sort of interesting that in a control system, there is no function called the "control function." Control is what all these simple SR functions, connected into a complete system, do.

The Hebb Rule seems much too restrictive to me. If the only effect of learning on a synapse is to increase its "strength", why don't synapses end up transmitting signals as fast as possible? A student of Harry Klopf's tried to model cell assemblies using the Hebb Rule, and that's exactly what happened in the model. All the cells just became maximally sensitive. Klopf had to add all sorts of ad-hoc rules to make it come out any other way.

Actually, to make any kind of computation take place appropriately, synaptic weights have to be adjustable upward and downward with equal facility. They're the coefficients that determine the shape of the function. Inhibition has to be able to turn into excitation and vice versa (presumably through growth and atrophy of various fibers connecting to various places on the neuronal soma). Perceptrons don't use the Hebb Rule. And anyway, who says that if a little response is appropriate, a larger one will be even more appropriate? Try that idea out on learning to thread a needle.

As to the negative reference signal, this is just a peripheral idea and I don't know if it signifies anything. Basically, it's a system in which the perceptual signal increases with the external variable's increase as usual, but instead of entering the comparator with a negative sign it enters with a positive sign. The reference signal is inhibitory. There can't be any output from a neural comparator if inhibition exceeds excitation. So there's no error signal until the perceptual signal gets bigger than the reference signal.

I can come up with only a contrived example, but maybe it will convey the idea. Suppose you're monitoring the carbon monoxide level in a room. You're told that the level is safe as long as the meter reading is less than half-scale. If the reading rises above that level, you're to adjust the speed of a ventilator fan to bring the level back down to less than half scale. So you can actually perceive a reading of CO without detecting any error. The amount of CO has to rise above a certain reference level before it signifies error -- then, and only then, do you act to reduce the concentration by speeding up the ventilator. So as long as the CO level is below half-scale, there is no error and no action, even though there is a perception and it is varying. In effect, the error is the CO level MINUS the reference level, with negative errors being equivalent to zero. Something external has to bring the perception above the reference level before the control system comes into action to oppose any FURTHER changes.

One last loose idea floating around that may have some bearing on your questions. I realized a little while ago that we can't possibly have

reference settings for most perceptions. When you reach out to touch a button on your phone, you certainly want to see your fingertip coming close to and then touching the button. But while you're doing that, you're also seeing your fingernail, the wrinkles in your knuckles, the little hairs, the back of your hand, your sleeve, the shadow of your arm, the telephone stand, and so on in extreme detail. You might possibly also have set a reference position for your elbow if you're reaching past a full glass of milk between you and the telephone. But most of the perceptions are just along for the ride, as it were. You pick out reference conditions for the few perceptions that matter, and base your action on what happens to them. As your fingertip and your elbow are made to behave as you want to see them behave, all the intervening and connecting parts of the scene simply change as the universe dictates they must. A point in the middle of your forearm just goes where it has to go to stay halfway between your elbow and your fingertip. You aren't controlling that point on your arm. But you can certainly be perceiving it, if you attend to it. And the perceptual signal is certainly there at least at the level of intensities if that part of your forearm is imaged on your retina.

What does this have to do with your questions? Nothing directly, I suppose. But as you control the few perceptions toward which you have intentions, it's inevitable that a much larger number of perceptions perforce changes. That can easily become relevant to other control systems -- for example, when moving your hand gets in the way of the light by which you're reading the telephone number. Maybe the hierarchy of perceptions will become easier to understand if we realize that we're looking only for a relatively small number of KEY perceptions, with all the rest just filling in the picture and changing incidentally. This may tell us something about which perceptions need to be learned and controlled, and which can be allowed to follow along as nature dictates. This is making me feel more like a realist today.

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

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Date: Mon, 4 Feb 91 11:32:15 -0600
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: connectionism vs. info proc.

Mark: I haven't read the Sci. Amer. article yet, but I would guess that the distinction made between connectionism and information processing refers to subsymbolic versus symbolic computation, an issue in AI. The "strong" AI thesis, as promoted by Newell and Simon and the majority of AI researchers, is that all intelligence, natural or otherwise, can be modeled by the manipulation of symbols. In the connectionist approach, symbols and their manipulation are not explicitly represented. The manipulation of symbols is often considered synonymous with info processing.

So on which side would control systems fall? Surely such systems are not symbol manipulators, although control loops could be set up in certain symbolic AI systems. But I wouldn't classify them as connectionist either, although control loops seem necessary for any learning in connectionist architectures (e.g., back propagation). So I would say that control systems are non-symbolic and not purely connectionist

systems either. Perhaps they are a specialization of the more general connectionist architecture.

-Jeffrey Horn (jeffhorn@uiuc.edu)
Graduate Student in Artificial Intelligence
University of Illinois at Urbana-Champaign

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Date: Tue, 5 Feb 91 00:50:31 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: LIST

REVIEW CSG-L

Tom Bourbon <TBourbon@SFAustin.BitNet>
Dept. of Psychology
Stephen F. Austin State Univ.
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Date: Tue, 5 Feb 91 08:42:41 -0600
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: LIST

Tom:

This message
>
>REVIEW CSG-L
>
which you sent to csg-l should have been sent to listserv@uiucvmd.
Anything sent to csg-l simply gets transmitted to all on the network and so
everyone sees your mistake (I've done it, too). Commands such as this need
to be sent to the listserver.--Gary

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Date: Tue, 5 Feb 91 09:02:13 +0530
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: attention? etc.

Bill and Mary (910131),

Thanks for the ole one-two punch. I realize that everyone has a little child mentality in them; my six year-old is just so overtaken with the not-my-fault mentality that it really stuck out. Anyway, I am just learning to observe children.

At the end of your reply to Mark Olson (910202) you commented on how "we can't possibly have reference levels for most perceptions". This seems to be something very important in particular for higher level behavior. Before getting into a lot of detail, let me just ask if "picking out a few reference conditions for the perceptions that matter" is more like attention than awareness? This attention thing is the big loose thread in my proposal for a CT view on language learning. Especially in an L2, attention is something that has limits, and the idea that an overriding perception or perceptions is all that we can attend to at a given moment goes a long way towards explaining some of the most noticeable aspects of non-native speakers; namely, the tradeoffs among communicability and fluency and form/grammar accuracy...(he dangles the hook, waiting for the bite...)

Joel Judd

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Date: Tue, 5 Feb 91 11:33:23 -0600
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: freeman, etc

Bill,
Thanks for explaining the concept of a Function--that makes sense to me. Of course now I realize that each of these Functions is a rather complex set up--we could have a "Perceptual Function Network" and have a lot to talk about.

In relation to the Hebb Rule, I mentioned it because I thought it sounded like the explanation you gave about "error sensitivity decreasing as output has a Significantly Less effect on Input." I'm I totally off here?

I don't think the Negative Reference Level idea is a solution to my question about the attractive woman in the card shop. At least I can't make the analogy of the CO level work with the idea. I can't equate "CO" with "woman" here because the former is undesirable and the latter is desirable. What would be the negative reference level for the latter?

In relation to your comments on having reference levels for only certain perceptions, are you saying a more accurate title for you book would have been "Behvior: the control of SPECIFIC perceptions"? I just want to clarify this. This reminds me of when I jam, or bruise, or cut a finger. I become aware of it and its relation to all the little things I do everyday. Is this because, as you say, this finger is normally just along for the ride and now that particular ride brings it to experience input (often pain) {is pain an input or an error} where it wouldn't before? Or is it what you are not saying, that there is a reference level X for that finger and now it cannot do X, hence the error? You are saying the former? This makes me feel like a realist too.

You have burst my bubble on chaos and perception. I'd be suprised if chaos is as easy to come by as you say--I'll ask around an find out. ARe your reservations basically empirical or are they theoreticcal too.
Theoretically it makes alot of sense that chaos would be the basis for

perception because it ensures that the space is covered without having to "search" for "identification" of a particular input. {this is poor terminology, I know}. As I reread your comments just now I understand that we don't want a "pattern perceiver." I suppose that is all you need to say on the issue. I hope Freeman IS smarter than you--I really want (and therefore think that) chaos will play a part, and that chaos does amount to much.

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Date: Tue, 5 Feb 91 12:02:34 -0800
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: subs

REVIEW CSG-L

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Date: Tue, 5 Feb 91 20:08:16 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Dennis Delprato <USERXEAK@UMICHUM.BITNET>
Subject: E. coli

REALLY FROM Dennis <Delprato@um.cc.umich.edu>

Rick Marken, Bill Powers:

I found a student who is interested in getting the E. coli lab set up for IBM PCs. In the Beh. Neuroscience paper you mention availability of codes for PC. We could use this.

Dennis Delprato, Dept. of Psychology,
Eastern Mich. Univ., Ypsilanti, MI 48197

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Date: Tue, 5 Feb 91 21:32:32 -0600
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: CSGnet (CSG-L) Subscribers

To all (but particularly Bourbon and Marken):

Whenever you get a message that looks like a command (e.g., REVIEW CSG-L) this means that the person who sent it (e.g., most recently Bourbon and Marken) sent it to the WRONG ADDRESS. All such commands should be sent to LISTSERV@UIUCVMD (Bitnet) or LISTSERV@VMD.CSO.UIUC.EDU (Internet) and not to CSG-L.

When such commands are sent to CSG-L all that happens is that they are broadcast to the entire network and EVERYBODY knows that you made a boo-boo.

Sending REVIEW CSG-L to LISTSERV will get you in return a list of all CSGnet (CSG-L) subscribers (we now have 39).

Here's the most recent list to avoid more boo-boos. @ followed by a single name (no periods) are Bitnet addresses. Multiple names (joined by periods) after @ are Internet addresses. If you are on Internet and wish to send to

Bitnet, you should add .bitnet after the single node name. Most Bitnet machines will properly forward Internet addresses as given here.

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marken@AEROSPACE.AERO.ORG MARKEN Rick: Aerospace Corp, LA
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Date: Tue, 5 Feb 91 22:35:49 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: AI, Attention

Jeff Horne (910204) --

Seems to me that the "strong" AI thesis, "that all intelligence, natural or otherwise, can be modeled by the manipulation of symbols", is simply a preemptive definition. If we come up with an example of non-symbolic problem-solving behavior, the AI view would then have to be that it isn't intelligent. I don't dispute that complex symbol-manipulation according to systematic rules is something we would include in a concept of intelligence. But I would also include other things in that concept, such as the ability to discern a principle in a set of procedures for cooking, or the capacity to understand music as a system of harmonic and temporal relationships. Of course it can be claimed that all such perceptions and actions based on them are mediated by symbolic processes, but such a claim would be hard to substantiate. I don't believe that anyone has figured out how to derive a principle or a system concept from a collection of symbols. The processes that would be needed must be of a type that we can't even guess at yet. They probably have nothing to do with what we now think of as "computing."

Human beings certainly handle symbols in complex ways. Why not just leave it at that, instead of trying to make that the whole story?

All the AI systems I've heard about (I'm no expert here) involve goal-seeking control processes. It's not hard to identify the perceptual process (the report on the current state of affairs), the reference signal (the specification of what state of affairs is to exist), the comparator (the estimate of the amount and direction of the difference), and the output function (converting the difference into a process-step that will make the difference smaller). In most cases it's possible to discern an underlying continuum, which is indicated by words like slightly, somewhat, moderately, mostly, and nearly -- degrees of variables which, on the surface, are discrete. Fuzzy logic seems to be an attempt to acknowledge this underlying world of continuous variables.

You have to try to see beyond tracking experiments where the continuum is obvious. Control arises from an interacting set of functions that operate on variables. There's no reason to exclude symbol-manipulating control systems. The KIND of variable doesn't matter. What matters are the functions and their relationships: perception, comparison, action. If you have those, and can show that the feedback loop is closed and negative, you have control.

Also, the control-system organization is much easier to see if you remember to include action in the picture, and the external part of the loop that makes the input depend in part on the action.

Joel Judd (910205) --

Can you imagine attention without awareness? I don't see these as separate entities. My own mental model of awareness is analogous to someone walking through a building with a flashlight. The beam of light illuminates small

areas of the rooms and halls, while the wielder of the flashlight notices what is revealed. "Attention" is simply directing the flashlight to one part of a room rather than another. "Awareness" is seeing what the flashlight illuminates. The analogy is made even better if we imagine that there is always a dim general illumination, so awareness can pick up on things that are outside the beam -- with the result, sometimes, of directing the flashlight for a clearer look.

Somehow I think it's going to be a while before anyone figures out how all that works. I can't begin to imagine the properties that a material system would have to have to produce the phenomena that I experience.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Wed, 6 Feb 91 09:15:36 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Stevan Harnad by way of Gary A. Cziko g-cziko@uiuc.edu" <HARNAD@PSYCHO.BITNET>
Subject: PSYCOLOQUY

CSGnet Subscribers:

I am appending to this message some information about an electronic journal sponsored by the the American Psychological Association. This is set up somewhat like CSGnet (CSG-L) but it is moderated meaning that submissions must be approved by the editors.

I hesistated in bringing this to your attention since I realize that some CSG members have had bad experiences with BBS and its editors. But perhaps the electronic nature of this journal will make it more open to challenging, nontraditional ideas. If so, it might be a way for us to have some discussion with "the world psychological community."

And since we are already organized electronically, we might collectively consititute a very powerful voice and argument for control theory ideas.

The listserver for PSYCOLOQUY is LISTERV@PUCC (add .BITNET if you are on Internet). A message saying "SUB PSYC full_name" sent to the LISTSERV should make you a subscriber. The journal address is PSYC@PUCC. Harnad's address is given above in the header.

--Gary Cziko

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To: PSYCOLOQUY Readership

Subject: PSYCOLOQUY Selected Among Best New Magazines of 1990
Call for Submissions to PSYCOLOQUY
"Test-Pilot" potential BBS material through "Skywriting"

The American Psychological Association's Press Office has just been notified that PSYCOLOQUY, an electronic journal sponsored by APA and implemented at Princeton, has been selected as one of the best new magazines of 1990 in the Library Journal's annual survey (by Bill Katz, to appear on April 15). This is a tribute to the electronic medium too, and we will redouble our efforts to develop the Net's vast potential in scholarly communication.

BBS (Behavioral and Brain Sciences) is a non-electronic journal specializing in Open Peer Commentary -- multiple peer feedback -- on current research in the biobehavioral sciences. It has become quite influential, but the interactive potential of the electronic medium is immeasurably greater than that of print. PSYCOLOQUY can and should become the "BBS of the Air."

PSYCOLOQUY provides the unique possibility of "test-piloting" material that may eventually become a target article for open peer commentary in BBS. It can also provide much faster feedback than BBS can, and with a global scope and interactiveness that no other medium can even begin to match.

Authors are encouraged to submit brief squibs (preferably not much more than a few screensful, though very short articles are potentially acceptable too) reporting recent ideas or findings on which you wish to invite peer feedback in the form of interactive "skywriting" discussion with the world psychological community.

All contributions are refereed by members of PSYCOLOQUY's Editorial Board, which will cover all areas of psychology and related fields. (Nominations for the Editorial Board are also invited.)

Stevan Harnad, Princeton
Perry London, Rutgers
Co-Editors

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Date: Wed, 6 Feb 91 13:07:12 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: JEB1@MSSTATE.BITNET
Subject: Re: LIST

I received your test message, Tom.

- Gene Boggess

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Date: Wed, 6 Feb 91 12:53:11 -0800
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: Symbols & War

Gary (910205) -- I can't believe I posted the request for "review" to csg-l. I'm really sorry. I was very consciously controlling for posting to the listserver. Unfortunately, the high level system that was carrying out that program picked out the wrong lower order "address" perception in order to carry out that program. How embarassing these lower level systems can be.

Bill Powers (910205) -- re: symbolic control. Very nice post. It is related to some stuff I have been thinking about relative to this Gulf War. That is, a great deal of the opposition and justification for this war is based on verbal symbol manipulation. Diplomacy has a lot to do with the "correct" way to say things -- which must have to do with what kinds of imaginings we expect these symbols to produce. I was set to thinking of the disproportionate

emphasis on symbol manipulation by attempts to justify (or, more often, to oppose) the war by describing what "really" happened to motivate it. It seems to me that these symbol manipulations often point us in the wrong directions (as the verbalisms of the law often do). They point us away from the non-verbal principles and system concepts that lead us to experience the war as "good" or "bad".

I think that social organizations exist to help everyone involved control what they need to control a bit better than any individual could control by him/herself. This requires cooperation, which means everyone can't necessarily have things exactly the way they want -- they must defer gratification or settle for a little less than they might get if they didn't have to cooperate (take the requirements of others into account). It would be nice if people could pay better attention to this aspect of social organization (mutual benefit through compromise and cooperation). I think one way to tell when people are NOT taking this into consideration is when they start talking about "legal rights" and "historical precedents" and other verbalizations that are used to justify screwing people up. In the gulf war, one side talks about "legitimate claims and grievances" that Iraq had with respect to Kuwait. I say, who the f**k cares about such claims -- they are just words and phrases. What I care about is that a very heavily armed group of people came in and very forcefully prevented another group of people from having any chance of being able to control the variables they needed to control. I think this violates the fundemental sense of justice that Hugh Gibbon talks about with respect to law; coersion is perceived as just when it is used to stop someone from intefering with the agency of another person (who is not, through their agency, interfering with anyone else). I think it is difficult to see what is happening over there as anything other than Iraq forcefully and brutally depriving Kuwaiti people of their agency. This was not done justly -- to prevent Kuwaiti's from brutally surpressing another group. There is no set of symbol manipulations that can make Iraq'a actions seem just. So coersion was exerted by the US - -since coersion can only be exercised by an agency that is physically able to exert it.

I guess I'm saying that coersion is just when it prevents some person or other agency from depriving another person or agency of their ability to control. Verbalism's about "legitimate rights", "god given rights", "manifest destiny", "legacy of imperialist domination" seem to me to be most often used as smokescreens to justify unjust coersion; depriving people of their ability to control for no reason other than unwillingness to take the time to look for cooperative solutions.

I will say that many of the US's verbal justifications for the war were also irrelevant -- probably an old habit left over from our earlier committment to using coersion to surpress rather than expand people's ability to control. I think the reason for the overwhelming support for the war is that many people see this war, at a non-symbolic level, as a just use of coersion to prevent deprivation of agency - the same conditions under which we recognize the use of coersion as just in this society. Yes, the US is acting as the policeman; but as the policeman exerting legitimate coersion (police coersion can also be seen as quite illegitimate if it doesn't seem to be used to protect agency).

Sorry about the lenghth (and possible lack of coherence). You can think of it as a kind of public war therapy session.

****

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Date: Wed, 6 Feb 91 20:11:16 -0600
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: Dewey as Lamarckian

Chuck Tucker (910201)

I have been finding the books of the Popperian philosopher of education Henry C. Perkinson (_Since Socrates_; _Learning from Our Mistakes_) quite consistent with what I would imagine to be a control theory view of education (against all forms of coercion, control and authoritarianism). And it is mostly from his writing that I know anything at all about Dewey. Here is an example from the former book (1980, New York: Longman pp. 201-2)

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Dewey's theory of growth or development was Lamarckian rather than Darwinian. This is not to say that Dewey believed in the biological transmission of acquired characteristics, but simply that, like Lamarck, Dewey (perhaps under the influence of Lester Frank Ward), construed human evolution as a telic process. Here is how he put it in _The Quest for Certainty_:

"Intelligence is a quality of some acts, those which are directed, and directed action is an achievement, not an original endowment. The history of human progress is the story of the transformation of acts which, like the interactions of inanimate things, takes place unknowingly to actions qualified by understanding of what they are about; from actions controlled by external conditions to actions having guidance through their intent:--their insight into their own consequences. Instruction, information, knowledge, is the only way in which this property of intelligence come to qualify acts originally blind." (p. 249)

. . . . Dewey's theory of human growth was decidedly Lamarckian. He, too, believed in the transmission of acquired characteristics--not a biological transmission but a transmission through instruction; and not, as we saw, the transmission of ideas or facts or subject matter, but the transmission of the method, the scientific method. Human progress or growth, Dewey believed, rested on the instructional transmission of the method of experimentation. Once they had acquired this method, human beings would know how to learn, how to grow.

It is at this point that we can see Dewey's educational authoritarianism. It sounds shocking to accuse Dewey of educational authoritarianism because all his life he battled against teaching methods that imposed knowledge and predetermined answers and solutions on the young. He never recognized his

own authoritarianism, his methodological authoritarianism. He believed: "The value of any cognitive conclusion is dependent on the _method_ by which it is reached" (_The Quest for Certainty_). And since he thought it was the best method he would have teachers impose the scientific method on the young. This doesn't sound like authoritarianism, especially since Dewey defined his method as the method of intelligence. But it is authoritarianism. It is presented as the ultimate justification for all answers to all problems."
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I understand control theory as another powerful argument against social and educational Lamarckism. Each organism is limited to its own perceptions. As Powers has noted, in living control systems, there is no engineer standing by with a screwdriver to fix things up if errors appear. There is no direct instruction from the environment. If there is intrinsic error, all that can be done is try something different, something unjustified and without insight. We cannot digest ready-made knowledge or even methods of obtaining knowledge from others. Education is not transmssion of knowledge, it is the active construction of knowledge by the individual. This is why I get so turned off by the term "Lamarckian" applied to anyone.

In addition to the reaction that this will undoubtedly elicit from Chuck Tucker, I would welcome any thoughts that the only (to my knowledge) educational philosopher on the network would care to offer. Have you got all those memos and phone calls done yet, Hugh?--Gary

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

Rick (Marken 910206) made some mention of the need to give up a little when living in a society for the sake of cooperation with others. In keeping with my interest in applying control theory to education, this reminds my of A. S. Neill's school called Summerhill and his book by the same name which I am now rereading.

Summerhill was remarkable for the total lack of authority in the school. Classes were optional. Students could do anything they wished as long as they did not infringe on the rights of others. Violations of others' rights were dealt with at a weekly meeting run by the students. Student's were made to repay for goods or services stolen or damages, but there was no real punishment and absolutely no moralizing about good or bad.

A. S. Neill's acceptance and approval of each student was absolutely unconditional. He did not withhold his love and support so that he could "reinforce" desired behaviors. In fact, he often "reinforced" undesired

behaviors, as when a student were rewarded for stealing. This reward was seen by the student as a sign that he was approved as a person by Neill no matter what he did and made him feel to be part of the school community. And when this happened, the stealing stopped.

I wonder if anybody else out there is familiar with A. S. Neill's non-authoritarian method of child raising and would care to comment on its relationship to control theory. It seems to me that Summerhill is the closest any community has ever gotten to the type of community control theory would lead us to have.

I remember a while back having some interesting exchanges when I brought up Skinner's _Walden Two_. There is quite a contrast between these two communities!--Gary

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Date: Thu, 7 Feb 91 09:49:45 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Special Air Fares

SPECIAL NOTICE TO ALL WHO WANT TO COME TO THE CSG MEETING BY AIR

America West is offering 50 % off all fares if you buy tickets on Feb. 7,8, or 9. Tickets are good until Jan. 5th, 1992. AMERICA WEST SERVES DURANGO!!!

The CSG meeting in question is from August 14th (arrival day) to noon on August 18th (departure after noon). Tom Bourbon will soon post exact meeting costs, but they will be well under $200 for room and board for the entire meeting.

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

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Date: Thu, 7 Feb 91 10:56:37 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: freeman, etc
In-Reply-To: Message from "CSG-L@VMD.CSO.UIUC.EDU" of Feb 5, 91 at 11:33 am

I just thought I'd share some comments on chaos, Freeman, etc. Bill and I had corresponded on this previously, and his keen questions brought me to some new understandings.

On chaos in general, first what must be understood is the conditions under which chaos proper arises. You need: 1) a deterministic, dynamical system with 2) sufficient complexity, three interacting

variables at least. I have no information as to whether neural systems are deterministic or not. I suspect not, but see below.

These conditions are crucial because we must clearly distinguish between deterministic chaos and non-determinism. A chaotic system gives the "appearance" of non-determinism at a macroscopic level because the underlying determinism at the microscopic level cannot be "resolved" at the observational level. Prime examples of random variables in physical systems that are "really" "just" chaotic include the flipping coin and thermodynamic temperature.

Now SOMETIMES properly chaotic phenomena can be described as such through appropriate statistical tests (e.g. phase-space embedding to calculate fractal dimension), and that's what Freeman thinks he's done. Other times that cannot be so, but that doesn't mean the system is not chaotic, just that we don't know. As systems approach dimension 30 (law of large numbers) their "chaoticness" ceases to become significant as they act like "pure" random noise sources. Thermodynamic systems are examples here. We don't know that they're chaotic through statistical tests, we just happen to have a good theory of the underlying deterministic dynamics. Since it displays random macroscopic behavior, we hypothesize that chaos must be present. This also raises the question as to whether any pure random sources exist that are NOT "really" chaotic (e.g. radioactive decay), but as far as I know, that has not been (and perhaps cannot be) resolved.

We should note that Freeman found dimensions between 4 and 9, a rather rough measurement. Furthermore, Freeman had more than twenty years of leading edge research invested in the idea that background EEG was purely random. For him, coming to chaos was a REVOLUTIONARY idea which rocked the foundations of all his theories. It took him some 8 to 10 years to make the transition, and was not taken lightly.

So, it seems clear to me that: 1) chaotic systems are quite common, ubiquitous; 2) the appearance of chaos may or may not be significant for a given system; 3) I agree with Bill that Freeman has NOT answered (2) for neural systems, but that doesn't mean that chaos doesn't matter for him; and 4) those who do "trendy" science will tend to forget (1) and (2).

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: Thu, 7 Feb 91 09:47:27 -0800
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: Education, Reorganization and Permissiveness

Gary (910206) -- I'm not an educational philosopher but let me try to comment on this question of "lamarkism" which seems to have to do with whether or not there is direction in learning. In the hierarchical control model, learning is, or course, reorganization -- a fundamental change in some aspect of the components of control systems; a changed perceptual or output function, for example. Reorganization starts when the existing control

structure cannot control variables that must be controlled for the sake of the organism's survival -- keeping intrinsic variables at their intrinsic references. A behavioral example of reorganization occurs in the skinner box when the rat learns to get food by pressing a bar. Pressing a bar is the only way to get food in that situation; if it doesn't learn to get food that way it dies. When it comes into the box it probably has not developed a "bar pressing" control system with which to produce the perception of food.

I think that rat's learning of the bar press control system is not directional; the rat cannot know what it has to learn to control in order to get food. The "trial and error" that appears to happen seems to be the only possible way to learn the new control system. Like evolution, reorganization cannot know what control system it should develop (how could it?) -- all it knows is what the consequences of the development of this system should be (the consequence should be "nutrient"). As Powers and I noted in the Chemotaxis paper, even though the reorganization process is random, it can be quite efficient when the interval between random changes is varied based on a comparison of the intended to the actual result of each change.

On the other hand, I think that some reorganization can be "directed" to some extent -- this is where education comes in. I think even rats, for example, are able to benefit from observing the successful reorganizations of other rats; this is imitation. People use imitation as a way of winnowing through possible random changes -- not always successfully but surely imitation can reduce the set from which possible new control systems are selected. In humans, language is, of course, a great tool for suggesting possibly successful new organizations. The problem with any of these winnowing strategies (what I think of as educational methods) is that they are too often presented as the only way to do things rather than as possibilities. People get in trouble, educationally, when they try to imitate control systems that are successful for others but won't work in the context of their own control hierarchy. Education might be more successful (and tolerable) if it saw itself as a source of options rather than edicts.

On permissiveness and Summerhill: I don't think that control theory suggests that "Walden III" would be a place like Summerhill (as you described it; I've heard of A.S. Neill but know little of the details of his community). I do think control theory makes it clear that people are autonomnous control systems. But that means all people -- students and educators. Problems arise when people start trying to control other control systems -- but how can they keep from doing it. Control systems control. If another control system disturbs a variable you are controlling you react, possibly affecting the other control system's ability to control. A.S. Neill may be perceived as more permissive than Skinner -- but he is still a control system. If he really has a community where he just let's otehr control system's control, even if this inflences the things that he is controlling, then he is just not alive any more. As long as there is more than one control system around there will be some degree of mutual influence and, possibly, control. This does not mean that things will necessarily go to hell. All control theory does is draw our attention to the fact (and theory) of control and interacting control systems. The "solution" to whatever problems may arise because of this fact is not provided by the theory itself. I do agree, however, that efforts, like Skinner's, to control behavior will likely, but not necessarily, lead to enormous conflict. But then, complete "permissiveness" in a world of limited resources is likely to lead to the development of

some pretty problematic control systems itself.

That's one of the problem's of control theory -- we don't sell well because we can't honestly sell utopia. All we can sell is quality.

Regards

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Date: Thu, 7 Feb 91 17:06:00 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Warning -- RSCS tag indicates an origin of SMTPUSER@UBVMSC
From: Hugh Petrie <PROHUGH@UBVMS.BITNET>
Subject: Re: Symbols & War

Rick Marken (910207)--Generally, it seems to me your approach to justification from a control theory perspective is correct. I do, however, have one question with respect to your application of it to the US's actions. Remember "The Test"? If what the US is doing is justifiable coercion, we should be able to ask about other disturbances to the world order and whether the US always acts to protect the agency of other societies. It appears not to many of us. e.g. Lithuania. Thus although justifiable coercion is a plausible candidate for what the US is doing, it does not seem to survive "The Test". So what are we "really" doing? This is what worries some of us.

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Date: Thu, 7 Feb 91 17:16:00 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Warning -- RSCS tag indicates an origin of SMTPUSER@UBVMSC
From: Hugh Petrie <PROHUGH@UBVMS.BITNET>
Subject: Re: Dewey as Lamarckian

Gary Cziko (910207)--I am ashamed to confess that I am one educational philosopher who has never made a careful study of Dewey so I cannot really comment on whether or not he is Lamarckian. I will say, however, that the quote from Perkinson does not convince me that he is. It seems to me that the directed action Perkinson speaks of is consistent with an interpretation from evolutionary epistemology. What looks like directed action can be understood as blind variation of already selected fairly intelligent subsystems. I think Marken (910207) is getting at

that possibility in his post on how education can help direct the levels of action.

Perkinson's other point that Dewey was authoritarian because he was pushing a method, the scientific method, is also less than convincing. All Dewey would have to do would be to admit that the scientific method is the best we seem to have at the moment, but it, too, could surely fall prey to problem-solving which might lead to the ultimate rejection of the scientific method in favor of some other approach. I think that's, in fact, what I believe.

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Date: Thu, 7 Feb 91 22:29:44 -0600
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: War and "The Test"

Hugh Petrie (910207)

>Remember "The Test"? If what the US is doing is
>justifiable coercion, we should be able to ask about other disturbances
>to the world order and whether the US always acts to protect the
>agency of other societies. It appears not to many of us. e.g. Lithuania.
>Thus although justifiable coercion is a plausible candidate for what
>the US is doing, it does not seem to survive "The Test". So what are
>we "really" doing? This is what worries some of us.

We can't do the study, but I wonder if there isn't a reference level for oil involved here somewhere, although Bush did say that we aren't in this to get anything for ourselves!

And what is Sadam controlling for. It seems the administration thought that he would act to prevent the destruction of his country and the killing and maiming of much of his army. "The Test" now says, nope, try again. There must be something else he wants. Respect from the middle east as an Arab with the guts to stand up to the U.S. and Israel? How do we test that one?--Gary

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Date: Thu, 7 Feb 91 22:30:13 -0600
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: Education and Determinism

Rick Marken (910207)

>On the other hand, I think that some reorganization can be "directed" to
>some extent -- this is where education comes in. I think even rats, for
>example, are able to benefit from observing the successful reorganizations
>of other rats; this is imitation. People use imitation as a way of
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>through possible random changes -- not always successfully but surely
imitation
>can reduce the set from which possible new control systems are selected.

Yes, I agree that imitation (and transmission-type education) may help to CONSTRAIN the variations, but it cannot eliminate the blind variation and selective retention. Think of it in CT terms. If I see you do something that I want to do (e.g., windsurfing as you commute from home to office in LA), all this gives me is a perception, and it is a perception which is VERY different from the perceptions I must control to be able to windsurf like you. I might see that that you stand in a certain position on the board, and I can try that too, but this really, at most, gives me a type of reference level, but not a control system for achieving it. This I must do myself. Nobody can tell me how to do it. Of course, I will not that one doesn't sit down to windsurf or that one doesn't try to go backwards and so this is useful. But there is no way that any perception of another person is going to give me the control system I need for windsurfing. I must learn to control my own perceptions which is not something that I can do by watching or listening to another (as is usually done in schools).

>On permissiveness and Summerhill . . .

>Problems
>arise when people start trying to control other control systems -- but how
>can they keep from doing it. Control systems control. If another control
system
>disturbs a variable you are controlling you react, possibly affecting the
>other control system's ability to control. A.S. Neill may be perceived as
>more permissive than Skinner -- but he is still a control system. If he
>really has a community where he just let's otehr control system's control,
>even if this inflences the things that he is controlling, then he is just
>not alive any more. As long as there is more than one control system
>around there will be some degree of mutual influence and, possibly,
control.
>This does not mean that things will necessarily go to hell. All control
>theory does is draw our attention to the fact (and theory) of control and
>interacting control systems. The "solution" to whatever problems may arise
>because of this fact is not provided by the theory itself.

I think the clinicians would disagree with you here. Why then do people like Ed Ford find control theory so useful in solving the interpersonal problems of his clients? Yes, we all control, but we can also control what we control. And control theory shows us what we can control (ourselves) and what we cannot (others). (This reminds me of the discussion we had during Ed's session at the last CSG meeting.)

This is the one thing I never understood about Skinner's behaviorism. If

he was right that all behavior was completely determined by the environment (plus biology), then how could one have a technology of behavior? How does the behavioral technologist get outside the deterministic system to make things better? I can't imagine that Skinner didn't consider this problem somewhere, but I have not yet been able to find him writing about it (perhaps Wayne Hershberger can help me here).

I would hope that control theory avoids this problem by its hierarchical system of levels of control. What we think at a higher level DOES make a difference in how we behave. If control theory suggests that the only way to avoid violence is to respect the freedom of others and if we want to avoid violence we may begin to respect the freedom of others. The thought it not something we induce using our senses and we do not need to be "rewarded" by the environment for such a thought. It just has to make sense at a higher level and it can change your life (and others'). In this sense, we are all "outside" of Skinner's deterministic system. And if this isn't a useful psychological theory, I don't know what is. Skinner's seems useless by definition. Control theory does seem to have the potential to make a difference. If not, I may have to pull the plug and start looking elsewhere.--Gary

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Date: Thu, 7 Feb 91 23:07:50 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Misc comments

Cliff Joslyn (910207) --

Illuminating comments on chaos, and your agreement about Freeman is reassuring. Chaos IS interesting in that apparent disorder contains order of an odd fractal kind. Something useful is bound to come out of all that stuff, but I don't see what it is yet. I got suspicious of it when it turned out that overdriven nonlinear oscillators show chaotic regions of behavior. Most of the oscillators I've ever run across were useful because they produced regular variations. You can use overdriven oscillators to produce harmonics, but usually enough overdrive to produce chaotic variations is a good sign that something's about to overheat and melt. Also the concept that the brain's behavior is based on chaos implies that behavior in general is random and unpredictable, which as I have said is probably not true. I can get a glimmer of light from the idea that chaos is involved in reorganization/evolution, but we have to keep in mind that the final product is a very systematic and precise kind of organization. Chaos could be how we get there, but it doesn't describe There.

Gary Cziko (910207) --

Mary -- who trained with Carl Rogers -- will write later on A. S. Neill and Summerhill. She's not enchanted, but she'll tell you why not.

Rick Marken (910206) --

Why you old warmonger, you. I think we have a chicken and egg problem here, just like the one between the Israelis and Palestinians. How far back do you want to keep score on who provoked whom to do what? The British screwed Iraq; Kuwait screwed Iraq; Iraq screwed Kuwait; we're in process of screwing Iraq; now Jordan and Morocco and Lybia want to screw us, etc. etc. etc.... It's been a nonstop international tag match for as long as anyone can remember. There isn't any Gulf Crisis. It's just another episode. And everybody, of course, is completely justified. Just ask.

We need some social and political scientists who can step outside this endless circle of words and show clearly how this mess is being caused by the people on all sides doing EXACTLY THE SAME THINGS to each other. We have to go up a level, not take sides. Sez I.

Gary Cziko on Dewey (910107) --

I understand that Scientific Method (with capitals) is in deservedly poor repute, but I don't agree that teaching a scientific (no capital) approach to life is "authoritarian." But my concept of a scientific approach probably wouldn't qualify as a method. I think we need to teach children the difference between reporting appearances honestly and embroidering them with interpretations. I think we have to teach them to test their ideas against real experience rather than adopting any old thing that seems convincing at first glance. I think we have to teach them about the tyranny of words. And I think we have to teach them to think quantitatively as well as qualitatively. Children beg for hints about how to be, how to grow up, how to deal with the world. They will treat us adults as authorities whether we want to be authoritarian or not. No matter what you do or what attitude you take, they're going to interact with it and either mesh with it or do the opposite. Either way they're learning from us. Sure, they have to make sense of what they learn all by themselves. But they have to start with the world they find around them, and we're part of it. We had better pay attention to what they're learning from being around us. We can be responsible for that much.

Read Robert Parker's detective novel, <Early Autumn>. It has some ideas about interacting with kids that are worth giving a thought to.

Speaking of authoritarians, the note about "Psycholoquy" was sort of interesting. How about if I start deciding who can contribute to CSGnet and who can't? Gotta keep our ideas pure, you know. Just send your posts to my personal mailbox, and I'll pick the ones that deserve going on to CSG-L. Is that OK with everyone?
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Fri, 8 Feb 91 12:12:58 -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: Misc comments

Bill (910207),

>Children beg for hints
>about how to be, how to grow up, how to deal with the world. They will
>treat us adults as authorities whether we want to be authoritarian or
>not. No matter what you do or what attitude you take, they're going to
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One of my favorite nebulous recommendations on childrearing comes from Proverbs: "Train up a child in the way he should go, and when he is old, he will not depart from it." There IS something about growing up that we've got to be responsible for, but there sure is a loophole there in the middle!

Since you throw in literary tidbits from time to time, permit me to add a couple. I've been trying to finish Oliver Sachs' THE MAN WHO MISTOOK HIS WIFE FOR A HAT. I had put it down about a year ago. It sure reads a lot differently now after thinking about control theory. The next time I have it and the computer together I'll share some of the anecdotes. The best one I remember was the night Pres. Reagan was giving the State of the Union address and a roomful of inmates were howling on the floor. It turned out that their disturbance was such that they were super-sensitive to nonverbal cues, and what the Pres. was SAYING was completely overidden by the way he was ACTING, and the inmates knew it.

And finally, since some of us live in Illinois, there was a religious group that passed through here about 150 years nicknamed "Mormons". In the early 1840s the city of Nauvoo was the largest in the state. Only five years before it had been swamp on the Mississippi. Writers from all over came to see about the people and the "Prophet" that led them, Joseph Smith. All kinds of rumors had circulated concerning this people. One writer, upon seeing how they lived and worked, asked Smith how it was that he managed to get everyone to cooperate and accomplish what they had. He replied, "I teach them correct principles, and they govern themselves." Interesting choice of words, isn't it?

Joel Judd

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Date: Fri, 8 Feb 91 10:18:22 -0800
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: Misc Replies

Gary Cziko (910207) -- you make two excellent points in your post. First, you are absolutely right about imitation and teaching -- it reduces the search space for new control systems but it doesn't tell you how the control systems actually works; you've got to try that by trial and error. In terms of the hierarchy of control systems, imitation might suggest a reference level for a perception at a high level (windsurfing) as a way to solve an even

higher level problem (getting to work) but it can't tell you too much about how to build the lower level systems that will make it possible for you to achieve that perception (perceiving yourself windsurfing). Of course, once you've gone to all the trouble of reorganizing to become a windsurfer, windsurfing might not prove to be the optimal solution to getting to work (unless you live in LA).

The other great point is the basis of a book I have been trying to write for three years. Behaviorists, like Skinner (and most other psychologists as well), claim that behavior is CONTROLLED by the environment; in Skinner's case this control is exerted by reinforcer's selecting the behaviors that produce them, but the mechanism is not important. If behavior is controlled by the environment then the behaviorist can control behavior if he or she can control the environment of the behaving organism. BUT the behaviorist him/herself must also be controlled by the environment. So how can a person who is controlled be IN CONTROL? Skinner has spoken to this problem; he talks about reciprocal control; the animal controls the behaviorist as much as the behaviorist controls the animal. So the behaviorist gives a reinforcement as a result of seeing the animal do the desired behavior. The reinforcement makes the desired behavior more probable making it more probable that the behaviorist will give a reinforcement. There are obvious problems with this analysis (it seems to predict that the animal and behaviorist will
accelerate into a frenzy of behaving and reinforcing, which is not what we observe) but the real problem is that the behaviorist is not really in CONTROL. A small disturbance to the animal's behavior could lead to a very different end result produced by the behaviorist. The behaviorist cannot intend to have the animal "make a figure 8" because this result could not be expected on each occasion because small changes in the animal's behavior would lead to small changes in the behaviorists behavior that might end up with the bird making an "0" rather than an "8". Control implies purpose -- making something happen even if circumstances are working against that end. This kind of purpose is what the behaviorist claims to have with respect to the behavior of others ("I can make you do want I want") while denying such purposivness to those very others. But both the behavioist and the organisms studied by the behaviorist are supposed to work according to the same principles.
I think this is the inherent paradox of behaviorism -- if behaviorists can control then they can't be controlled. But if they can't be controlled then neither can the objects of their control so the behaviorist can't control if he/she can control. It's like the man who says he is from Crete where all men are liars. If the statement is true, its false; if false, its true.

I know that control theory does not suffer from this paradox. Control theory has no problem explaining the behavior of the control theorist with respect to the behavior of the objects of his/her theorizing. But that's what the book was going to be about. I'm still working on it.

Bill Powers (910207) -- gee Bill, I'm not that old! Actually, I don't mean to be that much of a war monger. I don't justify this war to myself (as I said) in terms of keeping score on who screwed who the most in the region. I would be happy to "go up a level" to find a solution. But how do you do this? What's up there? How do you "go up a level" when someone has just robbed you at gunpoint? Trashed you apartment in order to get you to move? I have been involved in conflicts where I have made every effort to be conciliatory and look at things from new perspectives only to find that my "opponent" was perfectly happy to not "go up a level" but, rather, to take the simple expedient of threatening or using violence.
I believe that people can and should try to get along and respect the

fact that everybody is just trying to control what they feel they need to control. But what do you do with people who would rather kill than talk? I am not interested in past wrongs or justice. I just want to know how you deal with people (and there are such people) who consider no other option than force. I agree that it's best to not get into situations where that kind of confrontation emerges. But what's past is past. There was a rape of a country (regardless of the cause or justification). What do you do? Ignore it? Mind your own business? Non-violence and reason work if the people your dealing with actually respect life and thought. What do you think Hitler would have done to non-violent Jewish protesters suggesting that it was inappropriate to Gas German citizens? My guess -- laugh his head off before shooting them all.

This brings up one last point about control theory and solutions to problems. Gary asked why clinicians think control theory provides such solutions. I answer that it is not the theory itself that provides the solution. If the theory is a good one then it makes sense to look for solutions in the context of the theory. But the theory itself doesn't tell you what to do. It might suggest that a way to solve a conflict is to "go up a level" and you might even be able to model such a phenomenon. But what if, as I noted above, your idea of a solution is different than another person's and both of your solutions require variables being in entirely different states. Let's also say that one person is unwilling or unable to "go up a level" or has no interest in seeing anything other than one possible solution and is will do anything to get that solution even if it risks destruction of him/her self as a system. Control theory might help you understand such a situation and it might suggest ways to get to what you might consider a solution that is mutually satisfactory. But it doesn't say -- do this and all will be well.

Don't get me wrong -- I'm not pessimistic about the value of control theory as a basis for solving human problems. Part of my interest in the theory is motivated by it's optimistic, humanistic perspective. I hope that the understanding that we get from control theory will help us keep from getting into the kind of situation we are in now in the Gulf. But I have a feeling that there will always be people like Hitler and Saddam and many others of their ilk. I wish it were possible to wish them away or love them away or non-violence them away or go up a level and make everything better-- but I doubt it. Still, I would love to see some concrete proposals for alternate approaches to the current and possible future world problems based on control theory.

Have a great weekend

Rick M.

****

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Date: Fri, 8 Feb 91 13:08:52 -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: Misc comments

Bill (910207),

>Children beg for hints
>about how to be, how to grow up, how to deal with the world. They will
>treat us adults as authorities whether we want to be authoritarian or
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Since you throw in literary tidbits from time to time, permit me to add a couple. I've been trying to finish Oliver Sachs' THE MAN WHO MISTOOK HIS WIFE FOR A HAT. I had put it down about a year ago. It sure reads a lot differently now after thinking about control theory. The next time I have it and the computer together I'll share some of the anecdotes. The best one I remember was the night Pres. Reagan was giving the State of the Union address and a roomful of inmates were howling on the floor. It turned out that their disturbance was such that they were super-sensitive to nonverbal cues, and what the Pres. was SAYING was completely overidden by the way he was ACTING, and the inmates knew it.

And finally, since some of us live in Illinois, there was a religious group that passed through here about 150 years nicknamed "Mormons". In the early 1840s the city of Nauvoo was the largest in the state. Only five years before it had been swamp on the Mississippi. Writers from all over came to see about the people and the "Prophet" that led them, Joseph Smith. All kinds of rumors had circulated concerning this people. One writer, upon seeing how they lived and worked, asked Smith how it was that he managed to get everyone to cooperate and accomplish what they had. He replied, "I teach them correct principles, and they govern themselves." Interesting choice of words, isn't it?

Joel Judd

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Date: Fri, 8 Feb 91 10:56:48 -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: Education and Determinism

Anyone,

>If I see you do something
>that I want to do (e.g., windsurfing as you commute from home to office in
>LA), all this gives me is a perception, and it is a perception which is

>VERY different from the perceptions I must control to be able to windsurf
>like you.

The idea that EVERYONE has to learn as an individual makes sense, and that imitation only gives an approximation to what a given individual perceives. But I would suggest that imitation might provide one the opportunity to avoid intrinsic error and the resulting reorganization. I'm thinking of things like learning to swim before being thrown into the lake, not handling acid with bare hands, etc. We could know how to employ currently existing control systems effectively before intrinsic error builds up.

Also, is there some acronym we could use for "blind variation and selective retnetion"? It takes so long to type, maybe we could select something shorter...(b.s.?--maybe not..)

Joel Judd

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Date: Fri, 8 Feb 91 20:41:06 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Dennis Delprato <USERXEAK@UMICHUM.BITNET>
Subject: A Test

FROM Dennis Delprato

Ask respondent what the following means to them:

"They are controlling variables."

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Date: Fri, 8 Feb 91 21:39:41 -0600
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: BVSR

Joel Judd (910208)

>Also, is there some acronym we could use for "blind variation and
selective
>retnetion"? It takes so long to type, maybe we could select something
>shorter...(b.s.?--maybe not..)

One acronym which enjoys some use (if only by Don Campbell and myself) is BVSR (not to be confused with the underwear).

However, since people generally react so strongly against the notion of blindness, Campbell now prefers blind variation, selection and blind retention (just to rub it in some more) which gives BVSBR. (The reason for adding blind to retention is to emphasize that all our knowledge is tentative; there is no way to know that what has worked in the past will work in the future (this is very Popperian).

On the other hand some, like philosopher of science David Hull, think "blind variation" is redundant, like talking about "dead stones" (or how about that favorite CT expression "closed loop" and its companion oxymoron "open loop") and so sometimes you see just VSR.

I think BVSR is a good compromise (as long as you spell it out the first time it is used since we can't expect everyone to know what BVSR (or CT) stands for.--Gary

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Date: Fri, 8 Feb 91 21:39:54 -0600
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: Freeman, Chaos, & Perception

Powers (910202)

Bill: I finally got around to reading Freeman's article in the February Scientific American. And I find that there is at least one part of your reaction that puzzles me.

>He's shown that there are typical patterns of activity that change with the
>scent, but different patterns don't amount to different perceptions unless
>there is something that can respond differentially to the different
>patterns. In other words, we still need a pattern perceiver, so I don't see
>that the topographic patterns bring us any closer to understanding
>perception. I think that the patterns are probably side-effects, having no
>intrinsic significance of their own. Like the flashing lights on the
>display of a pinball machine. I think we should be watching to see what the
>ball does.

I am puzzled since I had thought that finding higher-order constancies in spite of lower-order variation was the essence of your hierarchical theory of perception. If we find a fairly constant higher-order topographic patterns in spite of great variability in the firings of lower-order individual neurons and groups of neurons, why isn't that a finding that you would embrace (if not actually do back flips)? Sure, we still need a higher-order pattern recognizer, but the job is now made easier and actually feasible. If chaos can help with the job of extracting invariants, so be it.

I just don't think you like the word "chaos" period. What do you think of that!--Gary

P.S. How's Mary coming with the post on A. S. Neill and Summerhill?

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Date: Fri, 8 Feb 91 22:37:48 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: Fuzzy Controller

Below is C source for a demo of a simulation of a fuzzy controller of an inverted pendulum. I'll discuss the details of what to do with it in a minute.

In looking this over again, I noticed a couple of things that should be said in response to Rick's initial question. First, this program makes no attempt to use CSG concepts. As far as I can tell, to my ignorant eyes, it is simply a fuzzification of an SR-type controller.

Second, it does have a formal similarity to an expert system in that it is a production system (PS). Inputs from the simulated bob (angular position and velocity) match against rules, which fire, resulting in responses that control the bob's position.

Where this differs from a "straight" controller is in the handling of uncertainty. In a deterministic PS, a unique match would trigger a unique action, yielding perhaps poor performance. In a stochastic PS, inputs would be used to calculate probability distributions of matching, and then distributions for rule firing. Finally, a Monte Carlo method would be invoked to choose one specific response according to the distribution.

In a fuzzy PS, the (computationally cheap) max-min calculus is used to calculate the "degree of matching" of each of the rules, and the "degree of firing" of each of the actions. A final (deterministic) aggregating method chooses a specific action. There are many mathematical forms for a fuzzy PS, but they use the traditional ones: max-min calculus for OR and AND, fuzzy numbers (convex real functions onto [0,1], here triangles) to represent certainty factors, and the geometric centroid as the aggregation method. These are similar to approximate reasoning methods in fuzzy expert systems. There are also ad-hoc methods available (e.g. MYCIN).

If anyone is interested in this, I can send more information or references. I see no direct relation between things fuzzy and Powers-ism, but I would be very interested in hearing from anyone who sees some, as they are both of considerable interest to me.

To actually get this thing running requires a DOS machine with EGA.
It's straight C with an MSC makefile. For those on a UN*X or UN*X-like host, cut below my signature and follow the instructions in the first few lines. For others, cut out the source code sections between SHAR_EOF by hand as appropriate. If you can't handle C, I can post a uuencoded DOS executable. I can give instructions on using uuencode, but would like to avoid actual postal mail.

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. . .

DELETED 53 PAGES. DAG JAN 31 1991. TO SAVE SPACE ETC.

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Date: Sat, 9 Feb 91 10:24:32 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Patterns: existence vs recognition

Gary Cziko (910208) --

The way Freeman gets those patterns is to place 60 to 64 electrodes on the cortex of the olfactory bulb. The contour plots are smoothed representations of the distribution of ionic currents over this surface. Therefore, in order to perceive these patterns, it is necessary to sense the local ionic currents electronically, plot them in a two-dimensional matrix, apply an algorithm that draws contour lines through the matrix, and PRESENT THE RESULT TO A HUMAN EYE AND BRAIN. The human eye and brain then report "There is a regular pattern here."

Whatever the next level of perceptual interpretation inside the experimental animal, it certainly does not look at the surface of the olfactory bulb with an EEG or with eyes. This is why I call the patterns "gratuitous." They represent a view of what is going on that the animal's brain itself cannot possibly have. The SPATIAL distribution of activity on the surface can't have any significance to the next level up, which can deal only with neural signals generated by processes in the olfactory bulb.

The regularities that Freeman finds, and their changes with different kinds of scents, are SUGGESTIVE. What they suggest is a hint about the computing processes that are going on. The spatial regularities suggest some orderly arrangement of computing functions that are physically laid side by side. It is not, however, necessary that neurons have any special position in the brain in order to communicate with other neurons. You could jumble up the locations without altering the computations, if you just preserved the connections (stretching the axons and dendrites as necessary). If you did that, all the spatial patterns as seen by an EEG would change, but the computations would not. The olfactory bulb would function exactly as before.

Well, that's probably not exactly true, because there are interactions among the ionic currents at a sub-firing-threshold level -- what Pribram has called "slow potentials." Adjacency may have some significance in that it permits interactions that bias thresholds without actually causing any nerve-firings. My point, however, is that these effects have to do with the computations going on, and that their spatial arrangement as seen by a two-dimensional visual system is irrelevant.

Long ago, Frank Rosenblatt laid down a principle that hardly anyone understood. He said that in order for any computation to have a physical consequence, its outcome had to be represented by a specific signal. It's not enough that the outcome be IMPLICIT in the computation. It must be made EXPLICIT (in the form of a physical signal) before it can have any significance in the operation of the system. It isn't enough that we have three kinds of receptors in the retina. Their output signals must be

combined by some computing function, and the result must appear as a physical signal, before "a color" can have any meaning to the rest of the brain.

Another way to say this is that it's not enough for a third party to realize that there is a pattern present. For the system in question to respond to the pattern, there must be some function that converts the separate elements of the pattern into a SINGLE SIGNAL that indicates the degree to which that pattern is present. That is why my hierarchical model is organized the way it is. Every perception exists as a SINGLE SIGNAL somewhere in the system. There is no reliance on an external observer who knows that some perception is implicit in lower-order signals. The system itself must know that, with no help from outside. Whether I have the right sorts of computations in the model or not, the principle must remain the same: anything that has an effect in the brain must first exist as a signal. Anyone who has done simulations or analogue computing knows this principle in his gut even if it's not said in so many words. The result of a computation, the value of a function, must be represented physically as a single signal.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Sat, 9 Feb 91 12:53:12 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Mary: Comments on A. S. Neill

{ This is from Mary Powers}

Gary -
Summerhill! What a liberating read that was! Really the opening gun of the 60's - all you need is love, etc.

But raising children needs a whole lot more. Two thoughts:

First, Summerhill was an isolated place. All hell could break loose and did. But we have to raise kids in a cultural context, and are their cultural context. They've got to learn to get along in our society with its range of ways to be, just as they have to learn our language with its range of sounds. And some of those ways are not things that children are spontaneously going to want to do. Fortunately, kids want to please, and are 'satiably curious, and what they must do can be offered in an enticing and interesting manner. But that takes a hell of a lot more work than just love - that's an attitude, not a curriculum.

Second, it's all very well to say that wonders occur when you give a child (or anyone else) unconditional positive regard. But who can do that all the time? It takes a very unusual person. I bet even Neill got fed up occasionally. Carl Rogers felt this was the key to therapy, but he only had to see each client for 50 minutes at a time. And I left the couseling center convinced that there was more to what was going on than UPR. The idea that you could simply reflect what the client was saying was almost a joke - it certainly lent itself to parody. But that's not the point here, which is that when you're a parent of little kids, you're with them morning, noon and night, and sooner or later you are

dealing with them on a gut level, drawing on a lot of unconscious stuff you learned from the people who raised you - and if that was kind of screwy you're going to be screwy too, however you swear things will be different. Either you'll do the same things again or you'll compensate and do the exact opposite. And by "doing" I include a lot of talking and acting that is probably very conditional indeed, and that you may not even recognize as such because it is so automatic.

I'm sort of rushing through this because I have a plane to catch. Lucky me, but I wish I had more time to get into this.

Mary Powers c/o Bill Powers <uppower@bogecnve>

Date: Sat, 9 Feb 91 14:03:02 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Fuzzy Logic Controller (??)

Cliff Joslyn (910208) --

An interesting (from several points of view) post, although incredibly lengthy. It doesn't look particularly compatible with Turbo C (what's this "FRAGMENT" thing?) but I can read it, at least the parts that count. For me, the real meat starts with "void cyclesystem" where the pendulum is modeled as a mass on a stick, and continues in the later statement of the rules.

>In looking this over again, I noticed a couple of things that should be
>said in response to Rick's initial question. First, this program makes
>no attempt to use CSG concepts. As far as I can tell, to my ignorant
>eyes, it is simply a fuzzification of an SR-type controller.

Yes, this is a fuzzification of an SR controller. It can detect only a few discrete states of the pendulum (five positions, three velocities, each counting zero) and it can respond only in a few discrete ways (motor current large or small, positive or negative, or zero). It is just barely able to control at all. Try adding a constant disturbing torque by adding a constant to TotalTorque (like someone leaning on the pendulum) or set the system up to hold the pendulum horizontal against gravity (use cos(Theta - PI/2) in the expression for MainTorque -- i.e., change the direction of gravity). This system has such a low gain that it will let the pendulum depart far from the nominal position. The reference signal is implicit in the system, in that the goal-position is assumed to be vertical (theta = 0). There's no provision for changing the reference condition.

As nearly as I can figure out from the rules, the control system is a simple proportional system with velocity damping (the rules having to do with dTheta). By the way, is Rule 17 right? It says that if the angle is negative-small and the velocity is also negative-small, the coil current should be negative-small. Seems to me that the coil is then pushing the wrong way. Where did these rules come from?

I don't want to go farther until I know why this approach to control was cited. If it was meant just to illustrate how fuzzy logic is applied, I'm

sure it does very well, although the example is unfortunate. If it was meant to show how well a fuzzy-logic controller can control an inverted pendulum in a "computationally cheap" way, I will probably have to refute that claim by writing a program to do the same thing a WHOLE lot better with about 5% of the code.

I take it that this is the same program that John Dockery showed at the Gordon Research Conference last summer. I wasn't much impressed with the performance when I saw it then. Rick Marken was right, in my opinion: this is an elaborate and clumsy way to approximate an analogue system. Or else it's a way of showing that if control is indeed accomplished by fuzzy logic, we shouldn't expect to see any very skillful control going on.

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

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Date: Sat, 9 Feb 91 17:10:35 -0600
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: Long "Messages"

Joslyn (910209):

I feel a need to suggest some policy for posting long messages and program codes as a result of Cliff Joslyn's posting of very long file today of over 100 KB.

First of all, there are probably relatively few CSGnet subscribers who know how to use such code.

Second, subscribers to CSGnet may not have the storage space for such long files. This is the case for just all about the students on my campus. Long files such as this will completely fill up their mailboxes resulting in their missing all mail sent subsequently until the mail is cleared out. As a result, my students will probably miss all messages sent this weekend. As CSG-L listowner, I have already received "filled mailbox" delivery error notices for a number of students, some of whom I had been planning to communicate with electronically this weekend, which is now not possible.

Third, very long posts take long times to transmit for those of us using Modems and may involve high phone charges for any having access via non-local calls.

Fourth, very long posts of interest to relatively few on the net may quickly discourage new subscribers from staying connected, particularly if the experience some of the problems mentioned above.

Finally, since all postings to CSGnet are logged on mainframe disk, very long posts may cause my university computing services office to think twice about continuing to offer this service.

I would therefore like to suggest the following policy: (a) that people do not send messages over 15 to 20 KB to CSGnet; (b) CSGnet can be used to announce the availability of program code and longer documents, but the code or document should be sent individually only those who express interest in receiving it.

One thing I do not want to do is to dissuade Cliff from participating on CSGnet. I have found many of his posts of great interest, most recently the clarifications on fuzzy controllers and chaos. I find the discussion among the CSG "hardcore" involving assimilation or fine-tuning and the discussion involving "outsiders" like Cliff and Bar-Kana more involving accommodation, reorganization, and paradigm shift. I think we need both types of discussion.

--Gary

Gary A. Cziko Telephone: (217) 333-4382
Associate Professor FAX: (217) 333-5847
of Educational Psychology Internet: g-cziko@uiuc.edu (1st choice)
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1310 S. 6th Street-Room 230
Champaign, Illinois 61820-6990
USA

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Date: Sun, 10 Feb 91 10:51:10 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: I am on! (I think)

For the past several days I have been trying to figure how to get on to the csg-l network. I think I made it. It certainly felt as though I was engaging in a trial and error process within limits. Hopefully, I will retain the control system. I look forward to talking to you all.

David d Goldstein
internet: goldstein%micvax.dnet@glassboro.edu

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Date: Sun, 10 Feb 91 19:27:52 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: modeling

I am glad to be on the CSG network.

I am going to start a new topic, namely, modeling, which I believe holds some interest for many of us.

I would be very surprised to see clinicians in the CSG group, or any other clinicians for that matter, become involved in the quantitative modeling we have all come to admire in Bill Powers' work. Why? For one thing, the clinical situation does not lend itself to being as experimentally controlled as a tracking task. For a second reason, the quantitative modeling does not help us carry out the practical task of helping the person bring his/her life more under control.

It seems more likely that clinicians will be interested in doing some kind of qualitative modeling approaches.

I have been interested in finding some new ways of testing whether a clinical hypothesis about a person is correct. In Control Theory, a clinical hypothesis takes the form: Perception Y is being regulated by action X. The action X is a publically observable fact about a person. The Control Theory question is: What perception is being regulated by action "X." Suppose that we come up with two possiblities: perception Y1 and perception Y2. How can we test which clinical hypothesis is correct, if any are?

I would like to share with you all one new way which occurred to me. Control Theory teaches us to expect that deviations from the reference level of a perceptual variable which is actively being controlled by a person will lead to error signals. The larger the deviation from the reference level, the larger the error signal. When the error signal is zero, a person does not change the action he/she is currently using to control the perceptual variable. The larger the error signal, the larger the urge to change the action which is regulating the perceptual variable actively being controlled.

Imagine that we obtained some rating scale data at regular intervals of time which asked the person to rate the intensity of perception Y1 and perception Y2. In addition, the intensity of "the urge to perform action X" is also obtained.

Suppose that we plotted "the urge to perform action X against time. The lowest point of the curve can be taken to be the reference level for whatever perceptions are being controlled by action X. Suppose that on a scale of 0 to 10, the intensity of perception Y1 = 2 and the intensity of Y2 = 5 at the lowest point. As a person deviates from these values, Control Theory leads us to expect increasingly stronger urges to perform action X the further we move away from these reference level values. If we do not obtain a U-shaped function around these values, then the particular clinical hypothesis may be rejected.
What do you think? David Goldstein, Internet:
goldstein@micvax.dnet@glassboro.edu.

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Date: Mon, 11 Feb 91 05:56:56 +0100
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen@ARTI1.VUB.AC.BE>
Subject: move

Gary Cziko:

I will move to Singapore from 18 Feb., please
send the messages to my new email address from that date:

Chung-Chih Chen

Institute of Systems Science
National Univ. of Singapore

Heng Mui Keng Terrace, Kent Ridge
Singapore 0511

email: issccc@nusvm.bitnet
Tel: +65 772-2075
Fax: +65 775-0938

Chung-Chih Chen
Free Univ. of Brussels

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Date: Mon, 11 Feb 91 09:48:28 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Fred Davidson,
University of Illinois at Urbana-Champaign"
<DAVIDSON@VMD.CSO.UIUC.EDU>
Subject: Two recent articles on computer networking

Two recent articles on computer networking:

(1) Pierce, et al. 1990. Computer networking for educational researchers on BITNET. **Educational Researcher** 20:1, Jan/Feb 1991, pp. 21-23. [Information on LISTSERV -- e.g. how to subscribe to LISTSERVers, how to get listst of LISTSERVers, etc. Nb. This is a LISTSERVer]

(2) Coursey, David. 1991. Riding the internet: the vast collection of networks is a mystery even to people who call it home. **InfoWorld** 13:5, Feb 4,1991. Pp. 48-49. [Overview of INTERNET w/schematic map of main nodes, information on Usenet (the information service) and commercial links to academic/research networks]

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Date: Mon, 11 Feb 91 09:01:56 -0800
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: Welcome/Reaction time

Hi everyone.

David G. -- Welcome to the net!!

Bill Powers (910209) -- Thanks so much for wading through that fuzzy logic code. It made me realize another reason why I like control theory -- the other stuff is too complicated. After reading papers by some of my favorite "trendy scientists" I realize that I couldn't do this stuff anyway; it's too complicated and my limited intellectual capacity looks for simplicity and elegance. I think that's why I never got into religion -- you really have to be clever to make that stuff make sense, and I'm just not THAT clever.

Since no one replied to my Friday post I'll just assume that you all agree with me and I'll move on to another topic. Specifically, I'd like to request some information that could help me in my efforts to write this paper on hierarchical perception/behavior. I am looking for existing reaction time studies that I could use as evidence of perceptual levels. The idea is this -- reaction time is, to some extent, a measure of transport lag in a control system. Now that we know that transport lags exist and are important in the operation of human control systems, it seems like there should be reaction time data that could help establish the relative transport

lag for perceptions at different levels of the control hierarchy. The transport lag (and, thus, reaction time) should be longer for higher order perceptions. Thus, it should take longer to make some kind of discriminative response regarding a relationship than a configuration, for example. I wonder if anyone knows of a study like the following: A subject is asked to make one response to a specific configuration -- like "A" -- and another response to another configuration -- "B". The responses should be based on discriminating configurations so the reation time should be about 250 msec. Now the subject is asked to make the discriminating response to two classes "ay" and "bee" where the exemplars of each class are very differnt configurations. Now the distinction should be severel levels up and the reaction time should be longer -- say .5 sec. Intermediate reaction times (between 250 and 500 msec) should occur for discriminations between intermediate perceptions -- ie relationships (response x if A above B and y if B above A). I have a feeling that there are reaction time studies that are like this. They are probably hidden away as Experiment 2 in some arcane JEP article somewhere, But I'm sure this type of study has been done. If it was done appropriately (that is, without confoundings -- you can respond to "A above B" vs "B above A" on the basis of the top configuration alone, for example) it could provide evidence for levels of perception based on reaction time. Does anyone (especially you grad students) know of such studies? If so, could you post the results?

Thanks so much.

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)

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Date: Mon, 11 Feb 91 10:38:53 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: e-mail correction

Gary pointed out that at the bottom of my statement about modeling there was a mistake. My internet address is:
goldstein%micvax.dnet@glassboro.edu

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Date: Mon, 11 Feb 91 11:04:10 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Imitation

General gab --

Mary, as you may have guessed, is up and about, still pretty gimpy but working on it. At this moment she's visiting our daughter Barbara just outside Durango, so she can obviously bend that knee far enough to fit in an airplane seat. She's going to look for a place for us to live (renting until we decide if Durango is IT).

All this talk about imitation (Gary Cziko and Rick Marken) has stirred up some thoughts amid the sludge. How do you imitate someone else doing something? It sounds easy until you realize that (a) you don't experience how doing it feels to the other person, and (b) you're watching the other person from entirely the wrong point of view -- yours, not the other's. Let me tackle this problem first from an angle that I don't think is very believable.

Suppose you want to imitate the way another person serves a tennis ball. The perception you have is this person tossing the ball upward while swinging the racket backward. "Up" means from the lower part of your visual field to the upper part; backward means (if the serve is to go to your left) moving in an arc from left to right in your visual field. If you now try to reproduce this perception, you will have to create the experience of a tennis ball moving upward and of a tennis racket moving from right to left in your own visual field. But that isn't what you perceive when you do it yourself. You look up at the rising tennis ball and see it going away from you and getting smaller. You don't see your own tennis racket at all except perhaps out of the corner of your eye. So what you just saw the other person doing doesn't help you at all.

The problem of imitating the tennis serve might be put this way. I want to do something kinesthetically and visually so that IF I WERE WATCHING MYSELF FROM ONE SIDE the image would be like what I saw when the other person did the same thing. An alternative is: IF I WERE SEEING THE ACTION FROM THE OTHER PERSON'S POINT OF VIEW, how would it look and how would it feel?

One implication is that we can, in imagination, move our points of view of a scene to somewhere other than where we are and construct the view as seen from that viewpoint in any direction. At least we can do this for key points in the scene, those that matter in a control process.

If this is how it works, we must carry a very extensive three-dimensional model of our surroundings in our heads. We can extract information from this model as if from many different points of view. The visual model must also go with a kinesthetic model which represents how the scene would feel (in the dark). The visual model and the kinesthetic model (of the same scene) get mapped together through long practice, so we know (for example) about how many strides it will take to reach the light switch in a familiar dark room (without counting). When we change points of view in the VISUAL model, we don't change the visual-kinesthetic mapping -- so the new visual point of view maps into a new kinesthetic point of view, and vice versa.

That's a staggeringly complicated process. Maybe there's an easier way. Rick Marken suggested it when he said something about realizing that a windsurfer stands up on the board instead of sitting or lying down, and noticing which way the board is supposed to move. When you "realize" such things, maybe what's happening is not just a straightforward (!) visual translation/rotation, but a step upward into the symbolic level where you

THINK these things to yourself. "Oh, you stand up. Oh, it moves toward the end nearest the mast." Then, when you decide to try it yourself, you remember "Stand up; it moves toward the mast end." Those symbolic phrases eventually get compared with "Ah, finally I'm standing up; Now I'm turned toward the mast end, ready to go forward." In the process of making the description of the action match the desired description, you have to achieve the lower-level relationships of which the description is a symbolic perception.

That seems a lot more possible to me. When a golfer tries to swing properly, it's not necessary to translate the pro's swing visually into the golfer's own swing. What does happen is that the golfer is told (among 25 other things) "hold your left arm straight." Then that phrase is translated into a feel of the arm that is properly described as "I'm holding my left arm straight." In fact the golfer is probably not swinging like the pro does, but that's the problem with all coaching. Coaching involves a lot of talking (most of it frustratingly uncommunicative, as in "Let's look alive there!").

Of course we're all built essentially the same way. When we manage to put together an action so that its details can be described in the same way we would describe the same details in another's behavior, we end up doing something that would look much the same to a third party. So it SEEMS that we are imitating the other's actions directly. In fact we are producing actions that fit the same description. If the key points of the description are carefully selected (where your shoulders, elbows, knees, and head are, etc.) the postures and movements that fit the description will be nearly identical no matter who carries them out.

I don't mean to tie "symbolic level" strictly to "verbal level." For some people thought is very explicitly verbal, but for others it isn't. Any kind of lower-level perception can be used as a symbol (Hugh Gibbons claims that he thinks mainly in kinesthetic symbols -- when he talks about the shape of an argument he means it literally; he feels it). The "machine language" of thought is probably not standardized. What matters, of course, is not the nature of the symbols, but the processes that structure them into programs that we call thinking. I might think "Hold that left arm straight," in words. Hugh Gibbons might have the same thought, but in stylized snippets of kinesthesia instead of stylized snippets of sound. The same sort of program-like activity would be occurring in either case, swiftly and silently.

This sounds a lot more like something that could be modelled. All we have to do is figure out how a set of lower-level perceptual signals can be turned into a symbolic description.

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

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Date: Mon, 11 Feb 91 11:05:58 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Bandura comment

Bandura et. al. ---

Well, my comment on Bandura finally appeared in American Psychologist (February, 1991). I told you it wouldn't do any good.

A friend sent me a xerox with page 153 missing, so I don't know who wrote the comment following mine, but it contains an interesting passage:

>Folk psychology is usually characterized as offering an explanation of
>why and how we do what we do in terms of mental states like beliefs and
>desires. These states are characterized by the property of
>intentionality -- that is, they are "about something." Desires, for
>instance, may be about succeeding in one's career and having beliefs
>about ways to do it. More technically, these intentional mental states
>are such because they each have a meaning or express a propositional
>attitude. (Notice that <intentional> in this technical meaning does not
>mean "done deliberately.")

So here we are in 1991 with 50-plus years of control theory behind us, and there are still people around trying to explain behavior without the concept of purpose. In this case, the ploy is to take a word designating purpose, whack off the old meaning, and give it a new one: aboutness. This is worthy of B. F. Skinner at his most exasperating. What's wrong with saying that intentional behavior is behavior that is generated to produce an intended result? Of course the intentionality isn't to be seen in the behavioral acts themselves; they vary all over the place, depending on disturbances. It's in the controlled variable that is maintained and manipulated by the variations in behavior. I wish it were possible to be saying this to people who don't already understand it. Is there ANY way to get this simple idea across to the philosophers and psychologists who still don't seem to have heard that the problem has been solved?
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Mon, 11 Feb 91 12:59:40 +0530
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: Misc Replies

Rick (910208) said:

>Don't get me wrong -- I'm not pessimistic about the value of control
theory
>as a basis for solving human problems. Part of my interest in the theory
>is motivated by it's optimistic, humanistic perspective. I hope that
>the understanding that we get from control theory will help us keep from
>getting into the kind of situation we are in now in the Gulf. But I have
>a feeling that there will always be people like Hitler and Saddam and many
>others of their ilk.

This opinion about the value of CT is precisely what has attracted me to it--CT gives us an understanding of behavior without implying that such an understanding somehow allows us to control that behavior. I get the feeling that this understanding=power thing is what underlies so much of "science". And when you present someone with a theory that offers an insight into human behavior without any gaurantee it'll give them power over that behavior, it kind of pisses some of 'em off.

Joel Judd
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Date: Mon, 11 Feb 91 16:24:53 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: JEB1@MSSTATE.BITNET
Subject: Re: Patterns: existence vs recognition

Bill Powers (910209) -

It seems to me as if the spatial distribution of patterns on the surface of the olfactory bulb, or any other neural surface, may have more than accidental significance. I am leading a readings course on neural networks this semester, and at least one basic type of artificial neural network, the crossbar network (associative memory, Kohonen map, etc.) classifies its input and stores similar "information" in physically proximate locations in the net. One of our graduate students used a Kohonen map to classify speech sounds; he devised a very nice visual display to represent the mathematical matrix constituting the neural network, and you could very easily see the "s" sound, the "t" sound, and so on - "t" sounds always produced a certain pattern in the upper right part of the matrix, "s" sounds caused a recognizable pattern to appear in the left middle, etc.

The mathematical description of how and why this happens is a bit complicated, but basically it involves an initially random distribution of predisposition on the part of each neuron to fire in response to a given stimulus. Whichever neuron DOES fire, however, enhances its own chance AND THE CHANCES OF NEIGHBORING NEURONS of responding in the future to similar stimuli, while inhibiting response by more distant neurons. (Think of the predisposition of each neuron to fire as a vector, and the stimulus as another vector; the dot product of the two will help the original vector only if the stimulus vector is pointing in pretty much the same direction.) This enhancing "close" neurons is a natural side-effect of how the stimuli are processed, not something that has to be deliberately "programmed in". Obviously, there is a lot of processing going on close to the sense organs themselves. This is true in the visual system as well.

I know this was not your main point; unless some signal gets back to the cortex, human beings don't know what is going on elsewhere in their neural system - that's what you were saying. I would probably agree with this if you define "know" to refer to conscious knowledge (but notice the word "probably"). On the other hand, there appear to be patterns produced at a lower level in the neural system that WOULD have meaning for us if we were directly aware of them. Maybe that was all the initial comment about the Scientific American article was trying to say.

- Gene Boggess

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Date: Mon, 11 Feb 91 20:50:42 -0600
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: Clinical Modeling

David Goldstein (910210)

David:

Welcome to CSGnet (CSG-L). To my knowledge, you are the first clinician to join the net. And I hope not the last. I know Ed Ford is trying to get connected a well.

In the meantime, I fear that your post on clinical modeling may not get much of a response, simply because there are no other clinicians on the net. So let me make at least some effort to comment on what you propose.

>Imagine that we obtained some rating scale data at regular
>intervals of time which asked the person to rate the intensity of
>perception Y1 and perception Y2. In addition, the intensity of
>"the urge to perform action X" is also obtained.
>
>Suppose that we plotted "the urge to perform action X against
>time. The lowest point of the curve can be taken to be the
>reference level for whatever perceptions are being controlled by
>action X. Suppose that on a scale of 0 to 10, the intensity of
>perception Y1 = 2 and the intensity of Y2 = 5 at the lowest
>point. As a person deviates from these values, Control Theory
>leads us to expect increasingly stronger urges to perform action
>X the further we move away from these reference level values. If
>we do not obtain a U-shaped function around these values, then
>the particular clinical hypothesis may be rejected.

My first reaction is that higher levels of control may well get in the way here. I can see the subject wondering, "What does the experimenter want to see; how should I react to this; do I want to please him or not." Thus other controlled perceptions may then get in the way.

Another problem is that you seem to lose a lot of the power of The Test when you can't manipulate the disturbance and see controlling behavior in real time. To use social science methodology jargon, the data then become correlational instead of experimental. If I (or my computer) generates the disturbance on a tracking task and the subject behaves to reduce the effect of the disturbance on the cursor, I can be pretty sure what is being controlled. But imagine that a person controls his feelings of loneliness by interacting with friends which he does by calling them on the phone. Now, if you asked him if when very lonely did he have an urge to use the phone, he might say yes and you might conclude that using the phone is used to control his loneliness. But it isn't the phone at all, just having somebody (a friend) to talk to is what is done to correct the loneliness error. And he might not use the phone at all if he had friends living next door or upstairs.

This isn't a great example. But at least it's a reaction. I hope that we can get some interesting clinical CT discussion going on the CSGnet. Not that I have much to add, but I have lots to learn by listening in.--Gary

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Date: Mon, 11 Feb 91 22:33:21 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: Long "Messages"
In-Reply-To: Message from "Gary A. Cziko" of Feb 9, 91 at 5:10 pm

> I feel a need to suggest some policy for posting long messages and program
> codes as a result of Cliff Joslyn's posting of very long file today of over
> 100 KB.

Apologies to this list, I was not aware that I was creating technical problems. Gary: one thing I can suggest is to make use of LISTSERV's file server facilities. You can store files (like the NOTEBOOKS of this list) and any list member can retrieve them with GET and PUT commands. Contact me directly if you'd like to talk more about it.

> One thing I do not want to do is to dissuade Cliff from participating on
> CSGnet.

Thanks, no offense taken.

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: Tue, 12 Feb 91 07:42:01 EST
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: DEWEY, REALISM AND THE WAR

Chuck Tucker (910211): Dewey, Realism and the War

On John Dewey

After I read Gary's post (910206) I realized that an error had been made on my part; I should not open up a discussion framed in the terms of "is 'x' a 'y'" with academics. This is a favorite "game" of all of us in academics and we spend too much time at it. Clark and I spent about four years arguing with people about how to classify George Mead and Herbert Blumer only to find out that such an exercise was not helpful in solving the problems that was of interest to us. [The major statement of this effort was published in 1979 and 1980 in the AMERICAN SOCIOLOGICAL REVIEW] So forgive me for embarking again on such a effort.

I propose that we focus on the utility of Dewey's statements for our own problems and concerns. Whether Dewey is a Lamarchian, Darwinian, Episcopalian, American, or "X" should only make a difference is it is useful for our problems. As others have noted [Marchen(910207), Petrie(910207), Powers(910208)] the quote cited from Perkinson's book does not support the charge that Dewey was a Lamarchian. I think we have another case here of someone who is trying to dismiss Dewey's ideas by labelling them in a way that others would find distasteful. It would be pleasant someday to read that a person simply says: "I don't agree with "X"'s ideas so I recommend that the reader of this

book not read any of "X"'s work." Or as John B. Watson was reported as saying of his course experience with John Dewey "I don't think I even understood what he was talking about." Yes, Mr. Watson we realize that you failed to understand Mr. Dewey's ideas! My suggestion is that one go to secondary sources very selectively after you have examined and studied the primary writings of an author.

Speaking of primary sources, I find that Perkinson's selection of the quote cited from Dewey's QUEST is quite odd; it is not the best quote in that chapter regarding the idea of intelligence. But rather than support my claim now I would like to encourage those who are working on the problems of experimental inquiry (which is the phrase that Dewey used more often than "scientific method") to read QUEST especially Chapter Nine. Here are some statements that I find quite useful:

"The natural man dislikes the dis-ease which accompanies the doubtful and is ready to take almost any means to end it. Uncertainty is got rid of by fair means or foul. Love for security, transulated into desire not to be distrubed and unsettled, leads to dogmatism, to acceptance of beliefs upon authority, to intolerance and fanaticism on one side and to irresponsibe dependence and sloth on the other (1929: 227-228)."

Dewey was proposing that we develop a "displined mind" or the "scientific attitude" as a way of dealing with these shortcomings. As he notes:

"The scientific attitude may almost be defined as that which is capable of enjoying the doubtful; scientific method is, in one aspect, a technique for making a productive use of doubt by converting it into operations of definite inquiry. No one gets far intellectually who does not "love to think," and no one loves to think who does not have an interest in problems as such. Being on the alert for problems signifies that mere organic curiosity, the restless disposition to meddle and reach out, has become a truly intellectual curiosity, one that protests a person from hurrying to a conclusion and that induces him to undertake active search for new facts and ideas. Skepticism that is not such a search is a much a personal emotional indulgence as is dogmatism (1929: 228)."

Dewey was not even authoritarian when describing the the features of the "scientific attitude"; he did not demand that anyone use this method but his book was an argument for its use as a way to solve problems that avoided authoritarian accounts and dogmatism. If one insisted on using other methods of "knowing" at least, if he read Dewey he would not do so not know some alternative. This is the old pragmatic way. His view of "other" philosophies is noted in this statement:

"Any philosophy that in its quest for certainty ignores the reality of the uncertain in the ongoing processes of nature denies the conditions out of which it arises. The attempt to include all that is doubtful within the fixed grasp of that which is theoretically certain is committed to insincerity and evasion,

and in consequence will have the stigmata of internal contradiction. Every such philosophy is marked at some point by a division of its subject-matter into the truly real and the merely apparent, a subject and an object, a physical and a mental, an ideal and an actual, that have nothing to do with one another, save in some mode which is so mysterious as to create an insoluble problem. Action is the means by which a problematic siutation is resolved. Such is the net outcome of the method of science. There is nothing extraordinary about this conclusion (1929: 244)." This quote leads to a comment on realism

A realist [Powers (910203) and Olson (910205)]

I don't think that the comments about KEY PERCEPTIONS indicates being a "realist" at all. What may appear to be inconsistent is probably that higher level purpose is overriding others and that those others are just "along for the ride." I think that this can be simply shown when you try to micro manage a large bodily movement like hitting a tennis ball or shooting a free throw - until the entire movement is put back together in a flow the purpose is very difficult to accomplish. We should be examining those key perception very carefully an mapping them with the others. This is very important to our understanding of human group life.

War (several comments up to 910208)

I don't think that it is clear to many people who read about cybernetic control theory that force does not resolve problems. I think that it is the word 'control' that throws people off since I always have to explain what the word means within CCT everytime I write about the approach. There may also be a contradiction that Marken has noticed in the approach - if you can not make another do anything they don't "want" to then how can you make then be peaceful, kind and gentle; someone may "want" to use force. In this war we have "just force" versus "unjust force" and the one with the most weapons will claim victory - an impossible contradiction to resolve unless you you take one side or the other as most people are doing.

I also thought in CCT terms about how this war serves the purposes (key perceptions) of many parties on both sides of the conflict and until it disturbs these purposes it will continue: Hussein sees it as gaining support for him as a political-religious leader in the area; Bush sees it as wiping out his wimp image and gaining support from the Conservatives which he needs to be reelected; the Pentagon Brass sees it as a way to justify the costs put into the military the last 10 years; the Military Brass sees it as a way to show how professional they are and as a way to finally win a war; Congress sees it as a way to assert their constitutional right to declare war; the pro-war folks see it as a way to finally get those anti-war slim and the press to realize who are the true Americans; business sees it as a way selling certain products and deflecting people from realizing that they are responsible for our economic depression; and so on. Until these purposes are accomplished or the error becomes too large for "important" people to deal with the war will continue.

What we can do is to hope these purposes are accomplished before any more people get killed and then we can work one trying to convince people force should not be used to solve problems. I don't believe this speaks to Marken's disturbance but it may help in thinking about the issue.

HOPE FOR PEACE CHUCK TUCKER N050024 AT UNIVSCVM.BITNET

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Date: Tue, 12 Feb 91 10:02:54 -0600
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: Fileserver for CSG-L

Cliff Joslyn (910211)

>Gary: one thing I can suggest is to make use of LISTSERV's
>file server facilities. You can store files (like the NOTEBOOKS of this
>list) and any list member can retrieve them with GET and PUT commands.
>Contact me directly if you'd like to talk more about it.

I would like to do make more use of the filserver capabilities of listserv, but the Computer Services Office here will not support it (other than storing the log files for us). If anybody out there is a listserv whiz and would like to take over CSG-L in order to provide services like this (e.g., having an continually updated bibliography of CT-related articles accessible to all), please let me know.--Gary

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Date: Tue, 12 Feb 91 10:45:12 CST
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: An opening query: multiple referencing in educational decision making

I'd like to make my first posting to CSG-L, a forum I have enjoyed reading over the past few weeks. Let me introduce the problem: I work in language assessment, specifically the assessment of English as a Foreign Language (EFL). Recent years have seen discussion among my colleagues of the need for greater 'pluralism' (a term coined by Bernard Spolsky) in the assessment of language learning. By this is meant that a language testing decision should involve consultation of many types of evidence, not just a single fixed-length paper and pencil test.

In educational testing this goes by the names: 'multiple criteria', 'multiple indicators' and so on.

I call it 'multiple referencing'. In educational assessment there is a proud historic tradition of tension between norm referencing, or decisions based on scores that reflect a student's rank in a group, and criterion referencing, or decisions against a point of mastery. The example I always give my students is a driver's licencing test. You sit behind a wheel (I know you all use driving as a metaphor a lot -- but this is different. Think like the **examiner**) and are evaluated on whether or not you can drive. The examiner does not care where you rank among all potential drivers; rather s/he has some criteria and looks to see if you pass.

The problem is that CRM (criterion referenced measurement) is related to NRM (norm referenced measurement) in certain unidentifable ways. When I took my first driver's test, I was told to pull over to the side of the road, wait a bit, and then pull back out. I did so, and later the examiner told me I almost failed. I had not signaled (left) or turned my head around to look. I **did** look in the outside left mirror, which he said saved my license. Somehow when we test by CRM we develop an instinct of what the relative ranking of certain skills actually is; we -- or rather the examiners -- know that good drivers at least glance in the mirror. There is a sense of NRM to that: what do good drivers do? Look in the mirror. It's better if they signal and crank their head around, but looking in the mirror defines the minimum rank. NRM is related to CRM.

What if there are other references? What if self-report is a type of reference. The scenario would be to ask a potential license holder him/herself how well s/he feels s/he drives. What role should that play in the decision? One of my students had an astute remark on other references: the difference between any two references, e.g. CRM and NRM or CRM and SRM (self-referenced measurement) is whatever one reference offers that the other does not. In a CRM turn-out driver's test, NRM guides the examiner to know that minimally good drivers at least look in the mirror but real good drivers crank their head around and signal. Perhaps SRM would aid -- perhaps the licensee could say: "Yes, I know I do that, my teacher was always ragging at me to remember to signal."

Multiple referencing is the process of appealing to more than one reference -- read "source of information" -- in an educational assessment decision. The act of integrating multiple references is quite difficult, but I suspect depends upon whatever aspects of assessment one reference offers that others do not.

What would control systems make of this? How would control systems address integration of multiple references in an educational assessment decision -- be it EFL testing, driver's ed., learning to shoot baskets (you all seem to do that, but I don't have a driveway), or whatever?

-Fred Davidson
Division of English as an International Language
University of Illinois at Urbana-Champaign

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Date: Tue, 12 Feb 91 11:04:52 -0600

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Unfuzzy controller

Cliff Joslyn, modelers --

Here is the source code (Turbo Pascal) for a control system that will hold a pendulum in any position. I used the same pendulum code that was in the "fuzzy controller" printout (except for scaling), but I moved the calculation of moment of inertia ("inertia") out of the loop because it only has to be done once.

This is a totally bare-bones program. No adjustment of parameters while running, no graphics. Also I used real numbers throughout -- lazy. The plot of theta is an asterisk scaled so the screen runs from 0 to pi horizontally.

The control system senses the angle plus first derivative of angle. This is like a sensor that is more sensitive to changes than to steady-state, a common arrangement in the nervous system. The damping parameter determines how much first derivative is added in. This provides all the damping needed for controlling a mass on a stick on a frictionless pivot.

If you comment out the line "controlsys" in the final bit of code, you can see how the pendulum behaves without damping. To do this you will have to plot -theta in the "writeln" statement instead of theta, because the pendulum will swing to negative angles (down). As set up, the pendulum just keeps swinging. In the original code there was a term " + alpha * dt2", which I commented out in the listing below. This term effectively puts in NATURAL DAMPING -- the pendulum swings less and less and stops. Try it. I don't know whether this was a mistake on the part of Infralogic or was put in because without it, the fuzzy controller couldn't ever stop the pendulum and needed a little subtle help. I suspect the latter. The control system modeled below doesn't need that help, as it puts in its own damping. Also there are no limits on the pendulum swing as there were in the original code. Not needed.

You can set the reference signal to any angle you like; it's pi/2 (straight up) as the program is set up. If you make the mass or the pendulum length too small, computational oscillations will set it, an artifact of doing an analog computation on a digital machine. Make dt smaller and it will go away.

I think this program will work better than the fuzzy controller, and is certainly "computationally cheap."

```
program pendemo;
uses dos,crt;


var current,
    maintorque,
    coiltorque,
    totaltorque,
    inertia,
    sticklen,
```

```
    mass,
    g,
    motorsize,
    alpha,
    omega,
    theta,
    dt,
    dt2:        real;
    lasttheta,
    thetadot,
    perception,
    damping,
    reference,
    error,
    gain:      real;
    ch:         char;
    i:          integer;


procedure initpend;
begin
 mass := 10.0;
 g := 9.80;
 sticklen := 10.0;
 inertia := sqr(sticklen)*mass;
 motorsize := 90.0;
 dt := 0.04;
 dt2 := 0.0016;
 alpha := 0.0;
 omega := 0.0;
 theta := 0;
 lasttheta := theta;
 current := 0;
 reference := pi/2;   { pi is predefined in compiler }
 damping := 20.0;
 gain := 100.0;
end;

procedure movepend;
begin
 maintorque := -sticklen*mass* g *cos(theta);
 coiltorque := motorsize * current;
 totaltorque := maintorque + coiltorque;
 alpha := totaltorque/inertia;
 omega := omega + alpha * dt;
 theta := theta + omega * dt {+ alpha * dt2} ;  { final term damps motion
}
end;

procedure controlsys;
begin
 thetadot := theta - lasttheta;
 lasttheta := theta;
 perception := theta + thetadot * damping;
 error := reference - perception;
 current := gain * error;
end;
```

```
begin
 initpend;
 while not keypressed do
  begin
   controlsys;
   movepend;
   writeln('*':1+round(theta* 79.0/pi));
  end;
 ch := readkey;
 ch := readkey;
end.
```

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

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Date: Tue, 12 Feb 91 10:05:35 -0800
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: Misc Replies

Joel Judd (910211) -- yes, I'm sure that many psychologists find control theory frustrating because, once they start to understand it they realize that it means that everyone is in control -- not just the psychologists. This creates an error in the psychologists who want to perceive themselves as powerful scientists who prove their understanding by being in control of their subject matter. So the solution is easy -- re-understand what control theory "really" says or ignore the theory altogether. It makes for a cozier group of control theorists.

Gene Boggess (910211) -- I probably shouldn't jump in on this because I have not read Freeman's paper. But I think you missed Bill's point about the gratuitousness of the spatial patterns emerging from the olfactory bulb. Like the differential activity in your phoneme recognition net, this information COULD be used as part of a system that, say, controls the environ-mental variables that cause these different patterns. But the patterns are only relevant if they are, indeed, represented to the system doing the controlling in a way in which the system can use them: For example, suppose I want to make a system that speaks. Then I could use your phoneme matrix as the input transducer and a sound generator as the output transducer. The effects of my sound generator are indicated to the system by the changing patterns in your phoneme detector matrix. Now suppose I want to produce the "s" consonant. This means that I want to produce a pattern in your phoneme matrix (whatever that pattern may be) that corresponds to "s". So I must specify the required pattern to the matrix.This spec could itself be a pattern -- in which case matrix pattern representation in your recognition matrix is not "gratuitous" with respect to the operation of the system that uses it. It is gratuitous if the system sends specifications for input in terms of the rate of firing in a single neuron. If this later is the case (and I think there is reason to believe that this is how information is functionally carried in the nervous system) then the representation of the phoneme must also be represented as the firing rate in another neuron (the input). Thus, even if your phoneme recognition matrix represents the phomemic transduction system, there is still another step -- the matrix pattern representing each different phoneme must be transformed into a particular neural firing rate. Of course, the nervous system might not work this way. It might work the way Freeman suggests; patterns of activity having

significance to the systems that use this information. But this has not been shown yet and until it is, the discovery of particular patterns (without establishing their functional significace) is gratuitous. Even if Freeman's patterns are shown to have functional significance, it would still be necessary to explain how the wiring of the brain is able to exploit these patterns.

Incidentally, I think this functional analysis of neural representation is somewhat independent of the value of neural network modeling. I think the latter is quite valuable inasmuch as it suggests possible algorithms for converting potentially controllable aspects of the external environment into signals that could be used as part of the control process. So don't construe what I said above as a criticism of your neural network modeling efforts, which seem quite impressive.

Gary Cziko and other possibly interested parties: I'm considering posting a rough draft of my hierarchical behavior/perception paper for comments and suggestions. It will be about a 50 kbyte document.
Is this a reasonable thing to consider given the possible impact on the listserver? I should have it ready by the end of the month (I hope).
What do ya think?

Regards

Rick M.

****

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Date: Tue, 12 Feb 91 12:43:22 -0600
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 Paper

Marken (910212)

>Gary Cziko and other possibly interested parties: I'm considering posting a
>rough draft of my hierarchical behavior/perception paper for comments and
>suggestions. It will be about a 50 kbyte document.
>Is this a reasonable thing to consider given the possible impact on
>the listserver? I should have it ready by the end of the month (I hope).
>What do ya think?

Rick: I suggest the following procedure. Anyone interested (and capable) of receiving this paper and others like it should send me a personal note. I will then combine these people into a group name and forward the paper to them after you send it to my personal email address. This is relatively easy for me to do with the software I have for email.

I will assume that the CSG "hardcore" will want a copy and will tentatively include in this group Powers, Bourbon, Hershberger, Tucker, and Delprato These people should let me know only if the do NOT want to be sent longer papers such as the one Rick is working on. (Forgive me if I've neglected someone with "CSG Hardcore" as a system principle; just let me know who you are). Students on my campus with UXA or free VMD accounts should let me know if they'd like a copy of the paper on disk.

This would prevent filling up the mailboxes of students on this campus and annoying those working via modem who might not appreciate such a long document

What do you think?

--Gary

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Date: Tue, 12 Feb 91 14:14:46 -0800
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: Conflict

Chuck Tucker (910212) -- I'm sorry to be a disturbance. Rather than carry on with my stupid opinions about the Gulf War, I'd rather just talk about conflict from a control theory perspective to see what's going on.

The simplest case of conflict occurs when two control systems have different reference specifications for the same controlled variable. The control systems can be in the same physical system (like a person) or in two seperate physical systems (like two different people): in the first case we have intrapersonal conflict (the person is in conflict with "him or herself"), in the second we have interpersonal conflict (two people in conflict with each other). War is an example of interpersonal conflict involving many persons.

It is pretty easy to model a conflict between two control systems. For example, the outputs of the two systems could be as follows:

o1(t)=k1(r1-p)+s1(o(t-1))
and
o2(t)=k2(r2-p)+s2(o(t-1))

(where t is time -- outputs are changing over time; input p is also changing over time).

assume that for both systems

p = o1(t)+o2(t)+ d

Thus, each system influences the perceptual input to the other (since it's the same input). The systems respond to the discrepency between this perceptual input and their references for the input (r1 and r2). If r1=r2 then is no conflict -- both systems want the same perception. If r1<>r2 then there is conflict. The outputs generated by one system will be a disturbance to the input to the other. The result of this conflict depends on the relative strength of the two systems. Strength is represented by k1 and k2; the amount of output generated per unit error. If k1=k2 there is a stand off. The systems match outputs until they are producing the maximum that each system can physically produce. If one system can produce more output than the other then that system will dominate but not necessarily "win" the conflict unless the residual output can completely compensate for the output produced by the other system. If k1<>k2, one system may dominate the other but, again, winning depends on the maximum output that can be generated by each system.

The intensity of the conflict between control systems depends on the relative value of K1 and K2, the maximum values of o1 and o2 and the difference between r1 and r2.

In the gulf, two groups of systems have differnet references for the perceived location of Iraqi troops. r1 = in Kuwait, r2 = outside of kuwait.

There are several ways to solve a conflict like this -- where solve means that all systems get their perceptions to match their reference states. The most approach is to simply let the conflict go and hope that the output limits of your system are greater than those of the other system -- much greater. Then one system (the stronger one) can get the perception it wants and the other system gets massive error. This is the solution call WAR. There are obvious problems with this solution 1) you can't be sure that you are the system that is going to "win" and 2) unless you completely eliminate the other system it will never stop trying to get its perception to match its reference so you will always be generating some output to prevent this (rather than devoting this energy to controlling other variables). There are other problems but that's enough for now.

The solution to conflict that is "best" and that all of us nice people want requires that one or the other party to the conflict "change their reference" for the mutually controlled variable. That is what "going up a level" is about, if you believe that the reference signals are set by higher order control systems. The higher order system could then see the lower level perception as part of a higher order controlled variable (like "being a big hero"). If this system could find, say, other lower order perceptions that would satisfy this perception than maybe r1 (troops in Kuwait) could be eliminated (so one system no longer has a reference for this perecption) and a new perception (factories in Iraq) could be substituted. The problem is that, when the conflicted control systems are in different physical systems, it is hard to get BOTH systems to solve the conflict by changing references. If just one system was always willing to change its reference in order to prevent conflict then there is the possibility that the other systems would notice this and rely on it. It could get to the point where the accomodating system becomes a doormat (which is certainly OK if it really never has any interrest in controlling any variables at levels that might cause conflict with other systems).

There is no "morality" in this view of conflict. Conflict just happen because control systems control and there are limited degrees of freedom (apparently)

available in the perceptual word of variables with which all these systems interact. Conflict seems to me to be unavoidable. But people who understand the nature of these conflicts will probably be better at dealing with them than those who are paid to do it (the politicians). Still, if one system in the conflict just refuses to change a reference for a perception which, if kept at that reference, will cause intrinsic error to another system, what can you do?

Actually, it might be interesting to try to model interacting control systems that can get out of these conflicts -- andthat do so in a way that does not destroy the physical integrity of either system.
Instead of praying for peace, we could be MODELING FOR PEACE.

Best regards

Rick M

****

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Date: Tue, 12 Feb 91 20:33:37 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Pattern recognition

Welcome to my old friend David Goldstein, and another one to Fred Davidson. Just so you'll know the ground rules: everything I say is right and if you don't agree I'll bomb you. That's the trendy thing.

Gene Boggess (910211) --

Glad to hear from you again.

When my model was first shaping up, one of the catchwords concerning perception was "encoding." The implicit model behind this concept was that of a recognizing system that received information and identified it, then emitted a signal carrying an identification code. It was basically a discrete decision system: its function was to decide which one thing among all possibilities was being perceived and to emit a code indicating the result. A single information channel was imagined, which carried differently-encoded signals.

A rival view, which I believe came from Oliver Selfridge, was the "pandemonium" model. In this model there were many simultaneously-active "demons" (probably related to Unix demons), each of which was specialized to recognize just one pattern. Each one "yelled" with a loudness that indicated how nearly the current incoming information matched the perception that the demon was specialized to recognize (hence the name, pandemonium). Multiple parallel information channels were imagined, with the signals in one channel being basically the same in form as signals in

any other channel.

In terms of a brain model, the pandemonium model is a "place" model -- a perception is distinguished by the place in the brain where it occurs, and not by anything unique about the signal itself. This is the type of model that I chose, rather than a "code" model. I chose it because all neural signals are basically alike -- if you use frequency of firing and not individual impulses as the carrier of information, as I did. With frequency as the carrier, only one dimension of variation is possible, so all that a signal can say is that more or less of something is present.

This means that when many perceptions are occurring at once, many places in the brain are active at once. Given an ambiguous wiggly line, both the 'S' recognizer and the '5' recognizer emit signals. And these signals appear in different channels, different places. So -- to reach the point at long last -- I am not surprised that in a recognizer model of the kind you describe, different inputs lead to activity in different places, topographically, in the model. You're describing a "place" type of recognizer.

What makes the difference to higher systems or to generation of behavior, however, is not the spatial location, but the fact that the channel is active. You are always going to get spatially-different signals in a "place" model of perception. But the place itself is insignificant -- it is the fact that the channel is active, not the fact that it is in a particular place, that is communicated to subsequent processes. All that can reach subsequent processes is the signal, and the signal cannot carry place information. It can only indicate how much of something is present.

I don't mean to say that adjacency doesn't have significance in the computations involved. It probably does. But that's part of the computing machinery, not part of the information being created and passed along.

My point (after Rosenblatt) wasn't just that signals have to get to consciousness. It was that there must be a specific signal representing a perception if ANYTHING that depends on the perception is to occur, even a spinal reflex. I used the example of color vision, but we could also use smell. Smells arise from stimulation of several different kinds of chemoreceptors. A particular smell, say the smell of a rose, excites those receptors in particular ratios. Can we say that the system will respond to the smell of a rose if those receptors are active in those ratios? No, because where does the signal come from that drives the response? The individual receptors are involved in ALL differential responses to smells. Somewhere there must be a neural function that combines the smells according to particular weights to yield a single signal that says how much roseness there is in the smell. This function performs the dot-product of which you speak (by the way, see p. 106 in BCOP for exactly this interpretation). The dot-product is represented by a SINGLE signal indicating its magnitude. It doesn't matter whether consciousness is involved, or even control.

This is harder to talk about than it should be. Am I getting anything across?

Fred Davidson (910212) --

A thought about a possible CRM. As Rick Marken suggests, reaction

time might be a useful way to discriminate levels. It might also be able to show whether a speaker is translating from a perception into the native-language term, then from native-language to foreign-language (instead of directly). The double translation would take longer. Flash a picture and see how long it takes for the word to come out. I know, it's been done.

I think that CRM is probably always better than NRM for determining capabilities (if not for handing out prizes). I don't see much virtue in mixing bad measures with good ones just to have "more evidence."
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Tue, 12 Feb 91 20:39:55 -0600
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: Dewey & Popper

Chuck Tucker (910211)

Chuck:

> I should not open up a discussion framed in
> the terms of "is 'x' a 'y'" with academics. This is a favorite
> "game" of all of us in academics and we spend too much time at
> it.

Yes, I agree. We should take the ideas that we find useful and not worry about classifying everbody and everything.

I do find your quotes from Dewey quite interesting. It seems to me that what Dewey does for you, Popper does for me. Based on these extracts, I note a great deal of similarity between the two philosphers. The notion that we can never be sure to have found the truth, but we can keep trying and get closer; man's fallibilty; the rejection of authority and dogmatism.

I do suspect that Dewey had more faith in the notions of the logic of the scientific method and induction (although I've have to read more to find out) while Popper rejected these in their ordinary sense. I have not seen any references by Popper to Dewey. Was Dewey aware of Popper's work? I realize that Dewey died in 1952 while Popper is still alive today, but it is possible that he might have been familiar with some of Popper's earlier works. If he has and if you can point out where, this would help me situate Dewey in the ideas and concepts I have learned from Popper.--Gary

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Date: Wed, 13 Feb 91 13:25:59 GMT

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mar@CS.ABER.AC.UK
Subject: Demos

Bill,

Just to tell you I've received the programs. I had a go yesterday on demo1, and found it most illustrative and entertaining. Thank you very much for your kindness.

I'm writing one of the final chapters of my thesis on adaptive behaviour, and I'd like to include the concepts of "blind variation and selective retention" among other things. Do you have any paper which I could make a reference to? or who will take the credit?

Best wishes,

Marcos.

Marcos Rodrigues
Univ. College of Wales, Dept CompSci, Aberystwyth, UK, mar@uk.ac.aber.cs
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Date: Wed, 13 Feb 91 09:12:43 -0600
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: Demos

Marcos Rodrigues (910213)

>I'm writing one of the final chapters of my thesis on adaptive behaviour,
>and I'd like to include the concepts of "blind variation and selective
>retention" among other things. Do you have any paper which I could make
>a reference to? or who will take the credit?

Allow me to intercept your query to Bill Powers about references to blind variation and selective retention (BVSR) (although don't allow me to cut off any comments that Bill might like to add).

The major figure related to the general application of this idea is Donald T. Campbell, now at Lehigh University in Bethlehem, Pennsylvania. You can find many references both to his work and others (over 1000 total) in :

Cziko, Gary A., & Campbell, Donald T. (1990). Comprehensive Evolutionary Epistemology Bibliography. _The Journal of Social and Biological Sciences_, 1990, _13_(13), 41-81.

Here are what I consider to be a few of the most important contributions by Campbell in this area:

Campbell, Donald T. (1956). Adaptive behavior from random response. _Behavioral Science_, _1_(2), 105-110. -- (1956b).

-- Perception as substitute trial and error. _Psychological Review_, _63_(5), 331-342.

-- (1960). Blind variation and selective retention in creative thought as in other knowledge processes. _Psychological Review_, _67_(6), 380-400.

-- (1974). Evolutionary epistemology. In P. A. Schilpp (Ed.), _The philosophy of Karl R. Popper_ (pp. 412-463). LaSalle, IL: Open Court. Reprinted in D. T. Campbell (E. S. Overman, Ed.). (1988). _Methodology and epistemology for social sciences: Selected papers_ (pp. 393-434). Chicago: University of Chicago Press.

-- (1988). A general 'selection theory' as implemented in biological evolution and in social belief-transmission-with-modification in science [A commentary on Hull]. _Biology and Philosophy_, _3_, 171-177.

-- (1990). Epistemological roles for selection theory. In N. Rescher (Ed.), _Evolution, cognition, and realism_ (pp. 1-19). Lanham, MD: University Press of America.

The 1960 paper had the greatest initial influence on my evolutionary thinking. The 1974 chapter is the seminal work on what now is called "evolutionary epistemology" (but what should instead be called "universal selection theory"). The 1990 paper shows Campbell's current thinking on BVSR.

--Gary

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Date: Wed, 13 Feb 91 09:17:38 -0800
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: Adaptation, Fuzz, Conflict

Marcos Rodrigues (910213) Here is another reference on adaptation based on random variation and selective retention.
Marken, R.S. & Powers, W.T. (1989) Random-walk chemotaxis: Trial and error as a control process. Behavioral Neuroscience, 103, 1349-1355

Bill Powers -- For the sake of the US economy I think it is important that we declare your pendulum program top-secret. I've heard that the Japanese are making a big effort in the application of fuzzy logic to control. If we act scarred, maybe they will waste as much time and money on fuzzy logic as they did on "fifth generation" computing. Trendy science can finally show its true value as a red herring.

Just a few more quick comments on my conflict post (910212):
I forgot to mention that, in the equation for each system's perception there is a disturbance that also varies over time:
p = o1(t) + o2(t) + d(t).

From the point of view of system 1, the disturbance to p is the sum o2(t) + d(t). The other system is just part of the disturbance from the point of view of system 1; the same is true for system 2. It was while working on a demo of these properties of conflict that I discovered that, in some cases, conflict can actually AID performace of one of the systems. This happens when the strength (actually gain) parameter, k, for one of the systems is much smaller than the gain for the other. The system with the larger gain, say system 1, can then control p much better with the low gain system working against it than without it. That is, the high gain system controls p better if the disturbance to p is o2(t)+d(t) rather than just d(t) alone. Bill Powers discovered the reason for this effect -- it is caused by the existence of transport lag in the control system. The response of the low gain system seems to "absorb" some of the dynamic instability that results from this lag. I am still studying this "Conflict based aiding" system (now world renowned as the "Marken effect"). I have found that the advantage of adding the low gain system response depends strongly on the difficulty of the independent disturbance, d(t). The improvement produced by the conflict based aiding system is much greater for a high frequency, high amplitude disturbance than it is for a low frequency, low amplitude disturbance. I'll bring a demo of the conflict based aiding system to the CSG meeting this summer, of course.

Hasta Luego

Rick M.

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Date: Wed, 13 Feb 91 16:10:03 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: JEB1@MSSTATE.BITNET
Subject: Gratuitous neural patterns

Rick Marken (910212)
> I think you missed Bill's point about the gratuitousness of the spatial > patterns emerging from the olfactory bulb.

Actually, I did get his point, but it also seemed to me right at the time I pressed the key to send my message as if my response wasn't very pertinent.

> Like the differential activity in your phoneme recognition net, this
> information COULD be used as part of a system that, say, controls
> the environmental variables that cause these different patterns.

I guess what I was really trying to ask was, why are these structures THERE, in the first place, if they aren't being used? The body COULD use them, presumably, and they have certainly turned out to look quite meaningful us in examining the results of our neural net as it trains itself. My bet is that they ARE being used for something

(after all, the human body had a good designer, and She had a l-o-n-g time to work out the bugs and get rid of extraneous details); we may just not know what they are being used for.

I agree with you that we humans aren't aware of this directly and, in fact aren't aware of anything at all until the signal gets somewhere back there in the cortex where perception (as opposed to sensation) occurs. On the other hand, a lot of processing gets done at a pretty low level in the neural system. I am working on a neural net to simulate human color naming behavior (a "classification" type of problem and a special interest of mine), and it is always amazing to me to see how much neural interaction occurs at the very lowest level (in the retina), and in the intermediate pathways (e.g., the lateral geniculate nucleus). In the retina, there are four different kinds of cells - besides the receptor cells (rods and cones) - that process visual signals even before you get to the optic nerve leading out of the eye, and at the very first level behind the receptors the horizontal cells immediately tie all the receptors together; talk about your neural nets! I feel very inadequate when trying to model such a complex and detailed system.

My gut feeling tells me that if different smells are represented in different spatial locations on the back of the olfactory bulb, then this is somehow used in passing the information back to the cortex. It may control the firing rate of a single neuron, as you suggest, or perhaps not; we don't know.

> But the patterns are only relevant if they are, indeed, represented to
> the system doing the controlling in a way in which the system can use
> them....

After mulling this over for a while, I have to say I think you are correct, given the definition of "relevant" that is implicit in the way you are using it. It is certainly true that we can, and do, lose information when we transform a signal; an 'A' on a computer screen may have been produced by an ASCII code (01000001) or an EBCDIC code (11000001) - we will never know just by looking at the 'A' itself. But I think it tells us something useful about the computer if we can find out which code was used. And that information may enable us to better control the computer at a later point in time. For this reason, I consider the information "relevant" even if it is not directly accessible to some particular level of the control system.

> But this has not been shown yet and until it is, the discovery of
> particular patterns (without establishing their functional
> significance) is gratuitous. Even if Freeman's patterns are shown to
> have functional significance, it would still be necessary to explain
> how the wiring of the brain is able to exploit these patterns.

Yup. I think that's exactly what we have to do; that's what I am trying to do with my mental excursions and research projects.

Gene Boggess

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Julian Eugene Boggess, III USMail: 415 Matson Road
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Mississippi State University
(601) 325-2756

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Date: Wed, 13 Feb 91 19:10:34 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Dennis_Delprato@UM.CC.UMICH.EDU
Subject: Marken Paper & Other mss.

FROM Dennis Delprato

The rapid distribution of mss. is one of the great advantages of e-mail. I look forward to receipt of Rick's draft and any other papers.

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Date: Wed, 13 Feb 91 14:18:49 -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: An opening query: multiple referencing in educational decision making

Fred (910212),

There are a lot of issues involved in (educational) testing and I hope that the post provokes some good discussion. Hugh Petrie addressed some general issues from a CT perspective in his book; perhaps he will elaborate and/or offer his current thinking.

>Recent years have seen discussion
>among my colleagues of the need for greater 'pluralism' (a term coined
>by Bernard Spolsky) in the assessment of language learning. By this
>is meant that a language testing decision should involve consultation
>of many types of evidence, not just a single fixed-length paper
>and pencil test.

One of the first things Petrie explains, and with which I agree, especially in language testing, is the need to (conceptuallly) seperate the SKILL from the MEASUREMENT OF THE SKILL. The reason this is necessary is to avoid the problems which enter into the learning environment when the two are confused. Both NRM and CRM generally look for some behavior which is "expected" or "acceptable" (as in a driver's test). This is fine when the behavior is fairly "simple" (Petrie's example is typing), but:

> "Consider skills like appreciating music, writing a poem, doing physics, engaging in critical thinking, and so on. Instead of admitting the inadequacy of our measurement tools, far too many educators simply insist that the measurement really defines the skill after all (Petrie 1981:111)."

So what happens when the skill is more complex, as in the case of language? What is the criterion, much less the norm, and how is it determined? One of the key problems, I believe, has been trying to decide what a measurement of the language skill looks like. Is it not the case that most language tests assume testee responses are the OUTPUT of the language system, or provoke an output of the system? There is nothing wrong with measuring

outputs as long as we recognize that we are doing just that, and remember that CT tells us these ARE NOT what the control system is controlling for--the control system is controlling for the perception of the inputs. The subject may be producing desired outputs (from our point of view), but they may be controlling for other perceptions (like getting a good grade, finishing the test in time for a movie, etc.).

Testing for language skills, and this I think is where the work needs to be done, calls for somehow causing error in what we think the person can control for (after having "learned" it). Maybe we want to know if they utilize 3rd person -s in English, or create a passive construction. The point is to see if the subject counteracts the disturbances we create. We are not trying to produce outputs, we are concerned with how the subject perceives the inputs.

Petrie mentions doctoral orals as approaching the kind of testing implied by CT; I think some kinds of oral language interviews get at language skills also. FSI interviews or others which expect certain kinds of outputs are not thinking along CT lines. Gary has mentioned some thoughts along these lines to me before.

Which brings up one other point. Testing industries which hold up standards as requirements for yet other valued goals (such as government jobs or university entrance) will likely have great influence over the learning environment. This can be explained in CT terms when, for example, the reference level "attending an American university" is what the person is controlling for. Getting 650 on the TOEFL is simply a disturbance to that perception. The learning situation may become one where the evaluation becomes the emphasis of the process. Even when the teacher may be attempting to help students value a skill, the learner realizes that what counts in the end is not the skill itself but the measurement of that skill. TOEFL is probably the greatest villain in language learning. In too many cases learning language skills has been replaced by learning language measurement skills, and knowing a language means how you score on a test.

> "So what I am urging is an entire conceptual change, a shift in perspective, on the part of educational testers from measuring outcomes to checking for corrections to disturbances of inputs... The shift from a focus on outcomes to a focus on correcting disturbances in inputs is a shift toward a more individualized view of learning and teaching. We will now have to justify standardization rather than the individual's creative correcting of disturbances (Petrie 1981:112)."

And then of course there's the statistical assumptions underlying standardized tests, sampling, norming, etc.

In short, testing from a CT perspective becomes an investigation into what perceptions a person can control for. The Test itself is not biased--it does not care (necessarily) why the person is controlling for something, only that he is. It does, however, point out the robustness of one's (language) skills; that is, how many ways can I adapt to disturbances? Put bluntly, if I can only control for disturbances to "English past perfect tense" on a multiple choice test, what kind of language skill is that?

Joel Judd

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Date: Wed, 13 Feb 91 22:03:51 -0600
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: Comparative Control Theory

Here's a new topic I thought about in trying to explain the "evolution of behavior" for my book.

I remember reading somewhere how the first explorers to visit North American brought back tales to Europe about the intelligence of beavers. They were impressed by their engineering ability in constructing dams and lodges and elaborate networks of canals for moving timber. If a dam sprung a leak they would furiously begin to repair the hole to maintain their pond. I've even read that when there is a thick enough covering of ice over their pond, they will let some water out of their pond creating an air space between the ice and the water creating the equivalent of an indoor pool!

Much much later, research was done to see just how smart Mr. Beaver really was. In one study, a loudspeaker playing a recording of rushing water was placed on the dam. Instead of being amused by the setup, the beavers started to pile more sticks and mud in the vicinity the loudspeaker to repair the nonexistent leak. So, it seems they were not so smart after all.

But what does "not so smart after all" really mean? One interpretation is that they were controlling what might be considered a lower-order perception whereas we would be controlling a higher-order perception. I suppose the beavers' system usually works fine, but not when devious researchers play recordings of rushing water.

Animals and insects display all sorts of amazing, complex behavior and control theory provides a way to understand this. But do psychologists and ethologists even have a clue as to what is being controlled by various organisms? What is the spider actually controlling in constructing a web? What about the ritualistic mating behaviour of many birds? What is the newborn cuckoo controlling for when it dumps the eggs of its unwilling foster mother out of the nest? And what about the complex social behavior of the social insects (bees, some wasps, termites, ants)?

The cuckoo example brings up another point. The cuckoo as a system might be controlling for the absence of spherical objects in the nest. But this in itself is not why this system evolved. It evolved because cuckoo chicks who did this in the past were more likely to survive and reproduce. So in some larger sense, the cuckoo is controlling for more food and parental attention than it would get if it had to share it's nest with other chicks. So evolution may result in control systems which control for immediate, simpler perceptual variables which then has the long-term effect of controlling other, more important variables (e.g., food).

It is also interesting to speculate about how there might be evolutionary pressure to evolve higher-order control systems. Higher-order systems would appear to be better able to maintain control in the face of more serious and complex disturbances (many caused by the behavior of other animals).

Such a comparative approach might reveal interesting cues as to the human hierarchy of perception and control. Are there organisms which can only control intensity? Others intensity and sensation? Others these two levels plus configuration, etc.? If so, what do their sensory and nervous systems look like? What are the minimum necessary requirements for a certain number of levels of control? This info might also be useful for people working on AI topics.

And finally, since higher organisms are not born with all the control systems they will need later on, how does the ability to reorganize control systems compare across different organisms. I think we know something of the sense modalities involved and how this differs (you can teach rats certain tasks involving smell that pigeons will never learn, while pigeons can control visual variables which rats apparently can't) but I haven't seen this explained in control theory terms.

--Gary

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Date: Wed, 13 Feb 91 22:04:49 -0600
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: Bugs in Your PC (Williams)

Here is an announcement from Greg Williams about some interesting software at a rock bottom price which may be of interest to some people on the net.--Gary Cziko

==========

PUT A BUG IN YOUR PC!

It isn't dangerous -- just use our Nervous System Construction Kit for IBM PC/XT/AT/386/486 compatible computers. Based on the ideas of Dr. Randall Beer
at Case Western Reserve University (see his book INTELLIGENCE AS ADAPTIVE BEHAVIOR: AN EXPERIMENT IN COMPUTATIONAL NEUROETHOLOGY, Academic Press, New

York, 1990), the Kit simulates a simplified cockroach with a network of quite
realistically modelled neurons. The six-legged bug (with nearly 80 neurons)

walks around its "world" with appropriate gait patterns, "wanders" pseudorandomly, follows edges, moves toward "food" when "hungry," "eats," and
manages to avoid conflicts among its various behavioral modes. You can

monitor
activity in up to five neurons at a time, and you can inject current into any
neuron. On a 12MHz 80286 computer, the simulation runs at about 1/12th real-
time. An animation option allows simulation runs to be recorded and
subsequently played back at much higher speeds (real-time on a 12MHz 80286
computer).
  The Kit programs also allow easy "fill-in-the-blank" user-specification of
ARBITRARY CONNECTIONS OF UP TO MORE THAN 100 NEURONS OF VARIOUS TYPES WITH
ARBITRARY PARAMETERS, including pacemakers and random bursters, and (by
modifying the included source code and recompiling with Turbo C or C++)
arbitrary specification of modelled "organism" and "world" physics, so you can
design your own networks and even entire organisms/environments. Great for
student projects -- all programs and source code may be distributed and
modified freely. Note: Full understanding of the programs requires reference
to Dr. Beer's book.
  For a copy of the most recent version of NSCK, send $10.00 (U.S. $15.00 for
delivery outside North America). NSCK requires EGA or VGA graphics; for
compilation of source code, either Turbo C version 2 or Turbo C++ version 1

is necessary. Full documentation is included on-disk; please specify 360KB,

1.2MB, or 720KB format.
  Order from: Pat and Greg Williams, Rt. 1, Box 302, Gravel Switch, KY 40328
U.S.A. (Telephone 606-332-7606.)

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Date: Wed, 13 Feb 91 23:04:11 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: Questions for Bill/Rick

I'm pleased to say that I've acquired copies of Powers '73 and Marken's '88 /Behavioral Science/ article. Very compelling, and filling in the gaps from this list.

Some questions/comments:

1) In my used copy of /Behavior/ (from Harold Blum's library, anyone know who he is?) is a copy of a review from /Science/ 184, pp. 455-457. An excellent review, comparing Powers to Wiener, Hebb, Ashby, von Neuman, Pribram, and other greats. No doubt correctly. I'd recommend it to anyone.

2) I have a listing in my bibliography for /Living Control Systems/ from CSG Press. I assume that's you guys. How can I get a copy? Is it cheap?

3) Marken gives an argument for distinguishing the stability of equilibrium from the stability of control: the stability of control is achieved even in the face of a continuous disturbance, not just a distinct pertubation. Doesn't this yield a QUANTITATIVE definition of

"control": if a fluctuation is sustained "enough" and the variance of the controlled variable is small "enough" then control exists. Is there then any problem with this "fuzzy" definition, or the possibility of a gradual move from equilibrium to control? If not, how do you account for the origin of control from equilibrium?

4) It seems quite clear to me that the intent of the CSG program is to identify all living systems (and their productions, e.g. engineering systems, social systems) as control systems. Thus the existence of control appears to be both a necessary and a sufficient condition for life. In my studies (cybernetics, autopoeisis, the Pattee/Rosen school of biological philosophy) I have come across many ideas for similar definitions: "cybernicity", or "semantic closure", or "autopoeisis" being a "definition" of life. Is my observation correct? How are these other suggestions related to yours? Better/worse? Is Gaia a control system?

5) If I'm correct about (4), then I'd like to suggest an argument that Pattee makes which is also compelling: given a vast, difficult phenomena like control (or life, or mind, or semantics) theory is best advanced by studying the simplest examples we can find. In evolutionary systems, the simplest will tend to be the earliest. Thus, don't study mind in the highest systems (humans), but rather in the lowest neural creatures; don't study semantics in human communication, but rather in the simplest coding systems, genetics; so: don't study control in the most vast human socio-cultural systems, but rather in the simplest, and most primitive control system. I would presume that this would be in the biochemistry of the earlist proto-cell. Yet the work of CSG (at least the practitioners) seems devoted almost purely to issues of human psychology and engineering. Is this simply an historical accident, or a reflection of various researchers interests/specialties? Why is CSG rooted in psychology, and not biology? Where's the application to control in non-human systems, plants, bacteria?

6) If I am correct about (4), then the CSG theory faces the same problem of all theories of life, accounting for the ORIGINS of control systems (living systems, cybernetic systems, semantic systems, autopoeitic systems) from a universe without them. All of the above do better at explaining the increase in complexity (evolutionary depth) of such systems given that they exist at all. In New Hampshire I heard Bill's strong ideas about this, relating CT w/Haken's hypercycles and other kinds of thermodynamic emergence. Could you please (again) discuss them?

Thanks again for all the stimulating conversation. This list is truly a superb example of the capabilities of this medium.

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: Wed, 13 Feb 91 23:02:11 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: more clinical modeling

I would like to continue the discussion of clinical modeling which I started with my last posting. For those CSG networkers who responded to it, thanks.

As a clinician gets to know a patient, the clinician builds up a system of information about the person which is based on a variety of kinds of information. This systems level perception is used by the clinician to help the patient reorganize dysfunctional control systems or acquire new control systems. I would like to argue that this systems level perception is a model. Sometimes, this model is referred to as the case formulation. In the February 1991 issue of the American Psychologist, Persons describes a way of doing psychotherapy outcome studies based on a case formulation approach.

Unlike the models of the person created for tracking tasks, I don't believe that we will be able to write equations to describe the model. Perhaps, the model will be describable in some form of expert systems language. Testing the model might consist of asking the clinician versus asking the model the same questions. The model is correct to the degree that it answers the questions in the same way that the clinician answers them. Testing the model might consist of seeing the kinds of questions the model versus the clinician asks when presented with a given statement. The model is correct to the degree it asks the questions in the same way that the clinician asks the questions.

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Date: Thu, 14 Feb 91 07:38:18 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Clinical modeling

David Goldstein (910210) --

[David and I have been conducting an argument for several years. David tends to win many of the rounds because he is working with clients who have both real and severe problems, and I often have to admit that when you're faced with solving such problems, you have to do what's possible. If a person is so depressed as to be on the verge of suicide, you give the person a pill that takes the edge off, and you're glad that such a pill exists. Afterward, you can think about trying something else. Even control theory can't cure a dead client.

A lot of our arguments are conducted in the context of such practical limitations. But I don't have David's responsibilities, so I can argue against conventional methods even if I don't have an immediately applicable alternative to propose. One of these arguments has to do with the utility of testing, particularly testing that involves questionnaires and other means of self-description such as Q-sorts. Basically, I argue that verbal tests are too imprecise to do much good, and that they inevitably put us in the position of applying statistical methods to individuals. I argue that we should be trying to apply control theory directly, trying to find out what individuals can and can't control, and trying to find out why they are having trouble. This means abandoning old diagnostic categories and old attributions of traits and conditions in

the attempt to explain what's wrong. I claim that we must make a conscious effort to break free of cultural assumptions, which always steer us back toward the conventional categories. David doesn't exactly disagree with me, but -- well, he can speak for himself. That's enough of an intro to this argument, which may as well go public now.]

David, you're proposing "qualitative modeling," which I rather like the sound of. You say

>Suppose that we plotted "the urge to perform action X against
>time. The lowest point of the curve can be taken to be the
>reference level for whatever perceptions are being controlled by
>action X. Suppose that on a scale of 0 to 10, the intensity of
>perception Y1 = 2 and the intensity of Y2 = 5 at the lowest
>point. As a person deviates from these values, Control Theory
>leads us to expect increasingly stronger urges to perform action
>X the further we move away from these reference level values. If
>we do not obtain a U-shaped function around these values, then
>the particular clinical hypothesis may be rejected.
>What do you think?

I think that the method as stated predetermines too many variables. The first objective should be to see what perceptions are under control. To do that you have to allow the action-variable to be free. If the perception is "People like me," the action that will contribute to that perception will be different under different circumstances (meaning, different disturbances of the sense that people like me).

Under the conventional approach, we would be most concerned with the action, because that is what other people experience. But to understand the acting person, we first have to understand what perceptions are under control. A given perception can be controlled through many different actions, so no one action is significant by itself. Furthermore, we might see both an action and the opposite action being taken as a means of controlling the same perception, depending on whether disturbances are pushing the perception above or below its reference level. The object of control theory can't be to explain one particular action.

So I would propose backing up a step or two, and starting by testing Y1, Y2 .. Yn to see if they are controlled variables. This is hard to do using a verbal test, first because while taking the test the person isn't experiencing the perception but only a description of the perception, and second because the only way to apply disturbances is hypothetically, by describing them and asking how the described disturbance would affect the described perception (and, presumably, what the person would do if the perception changed). I much prefer direct interaction in real situations, with perhaps a discussion afterward if you want to cast the interaction in verbal terms. Maybe role-playing would be a compromise that allows setting up hypothetical situations while still allowing real perceptions and direct interaction with disturbances (supplied by the experimenter).
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Thu, 14 Feb 91 11:35:40 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Misc. comments

Great gobs of meaty mail this morning. It's really nice of all you people to spend so much time just to keep me busy while Mary's gone.

Gene Boggess (910213) --

Sounds like very ambitious and important modeling that you're doing. We control theorists need good models of the perceptual systems -- they're the heart of any control process. I hope you'll tell us about any publications, past or future, of your work so we can learn about it in more detail.

The question of consciousness gets into some strange territory. You make a distinction between sensation and consciousness, and you may be right to limit conscious perception to the cortex, but that may not be the whole story. I've distinguished quite a few levels of perception, so many that a single dichotomy doesn't seem adequate. In the course of doing this, I've had to look pretty closely at perception as a phenomenon, and one of the real jolts was to realize how much I had always taken for granted. I think we tend to accept most perceptions as just being "out there" in the real world, not realizing that the brain has to be involved in ALL that we experience, not just special pieces of it here and there. The computer I'm looking at right now is basically a perception, and only hypothetically something more than that. So are my fingers on the keys, and the pressures I feel with each keystroke. As I look at the screen, even the brightness of the characters must be a perception (or sensation, if you like). My point is that we can be aware of things that really don't belong in the cortex. The experience of INTENSITY of any sensation or perception correlates with the frequency of signals emitted by sensory receptors! This makes it seem that we can be aware of signals in the very first level of sensory processes.

In my modeling I've always followed the principle that the brain never does the same thing more than once. If the intensity of a sensation is adequately represented in peripheral sensory signals, the principle says that it's not represented AGAIN at a higher level. Higher levels do something else with the information -- interpret it as sensations, configurations, transitions, etc. If imagination seems to involve perception-like phenomena, then it uses the SAME perceptual machinery that is used for real-time perception too. The principle may be wrong, but if so it errs on the side of parsimony, which is probably good.

Anyway, the implication is that consciousness connected with the cortex would probably be associated with more abstract perceptions, while consciousness could also be involved in low-level perceptions which you call sensations. In other words, awareness can participate at any level of the nervous system (even the retina?). Another implication is that signals can be present in afferent channels (at any level) WITHOUT participation of awareness -- the associated control systems work either with or without awareness. So consciousness is a combination: awareness plus perceptual signals.

I suppose you hadn't seen my post concerning spatial locations of signals when you replied to Marken. Want to comment on it? I think it is compatible with your idea that these structure DO something, even if the location information isn't what is passed on to higher systems.

Joel Judd (910213) --

>Both NRM and CRM generally look for some behavior which is
>"expected" or "acceptable" (as in a driver's test).

The control theory approach adds another alternative: testing as measurement of characteristics rather than in comparison with a standard. An analogy would be the measurement of reading speed. You can measure how many words per minute a person reads without saying that a particular speed is desired. In skill terms, you can measure how a person controls variables of different kinds, and from such measurements determine how the person will perform in tasks involving similar variables.

Long ago I was playing around with the concept of "styles of control," which meant determining whether a person tended to be a leading, lagging or proportional controller. A leading controller corrects for the rate of change of error, a lagging controller corrects for cumulative error (a positive error is required to compensate for a previous negative error), and a proportional controller corrects for the present-time error without regard to future or past. Nobody is a pure instance of any of these styles, but you can do some personality descriptions by asking questions that reveal which style predominates in a given person.

Assuming that such characteristics can usefully be measured, we could come up with some conclusions that don't seem like NRM or CRM usages. We could say, for example, that if A is an integral controller and B is a leading controller, they should not be put in a position of having to do a task together, because the combination will tend to oscillate.

So I'm thinking in terms of using measurements to match a person's characteristics to the requirements of a task. This is a little different from meeting a criterion or passing a test, isn't it? Or is it?

Gary Cziko (910213) --

Some lovely thoughts on smartness. You've basically pointed out why the Test is so necessary. We can easily read organization into behavior at too high a level, just because WE can perceive at the higher levels. This mistake is akin to the post hoc, propter hoc error: after, therefore because of. In this case it's attributing purpose to an outcome that is a byproduct of controlling some lower-level variable. Your example of the cuckoo is perfect: the young cuckoo acts in a way that gets it more food and parental attention, but I'll bet that if you gave it all the food and parental attention it needs, it would still push the other eggs out of the nest. It's just controlling for "no eggs," and the other consequences are side-effects. It doesn't know why getting rid of the eggs is a good idea. This is a very nice way of contrasting evolutionary effects and purposive behavior of a single organism.

I've always wanted to see someone do a comparative study to see where the control capacities cut off for various organisms. This might provide a new basis for ordering organisms on an evolutionary scale. Survival doesn't seem to be a very good basis; on that basis, cockroaches and sharks are way ahead of most other vertebrates, including us. Somebody ought to take up Gary's suggestion here in a big way. Maybe if Dennis Delprato is able to work up some good control-system experiments with his animals, he could think about branching out.

Cliff Joslyn (910214) --

I agree that CSGnet is lively and active, and am glad that you are still helping to make it so.

(2) "Living Control Systems" can be obtained from Greg Williams, Rt. 1, Box 302, Gravel Switch, KY 40328 ($16.50 pp.-- cheap). Greg did a superb job with editing and typesetting this book. He and his wife proofread everything with one person reading aloud from the original and the other following the typeset text -- twice.

(3) There really can't be a continuum between passive equilibrium and active control, because active control requires amplification, which in turn requires going from a thermodynamically closed to a thermodynamically open system. Once you have a control system with amplification, you can always turn down the gain and approximate an equilibrium system, but the basic organization will still be totally different. Another way to define an equilibrium system is to say that the energy needed to correct the effect of a perturbation is exactly the energy put into the system BY the perturbation (a pendulum). In a control system, the energy needed comes from a power supply, which in general can supply many times the energy put in by the perturbation.

(4) You touch on a sensitive point: are we control theorists doing the same thing that behaviorists do when they call all actions "responses?" Are we trying to force the control-system interpretation onto systems that could equally well be seen in some other way? I try very hard not to do this, but I probably don't always avoid it.

I think we have to stay in close touch with our roots. We're not basically interested in control systems. What we're interested in are systems that are able to maintain variables outside them near internally-specified states, resisting disturbances with great effectiveness under a wide variety of conditions. When we say that organisms are control systems, we're not just classifying them; we're saying that they have exactly this critical capability, which few scientists have recognized but which is easy to demonstrate. It doesn't matter to me what you call such systems: cybernetic, autopoetic, closed-loop, or whatever. What does matter is to understand how they can accomplish what they do.

When I propose that we look at certain biochemical systems as control systems, I'm not just suggesting that we try on this point of view as a general way of talking. I'm proposing that we investigate biochemical systems to see if they have the required properties. Our control models derived from more general experiments show us the kinds of things we need to look for: input, comparison, output, and amplification (loop gain). I have found, in the literature, a few examples of closed-loop enzyme-catalyzed chemical systems that fit the bill. I'm willing to say that these are examples of control systems, because they have the properties that are needed. But this doesn't lead me to say that we now know that all biochemistry is organized around the principles of control. We have an existence theorem that now makes it worth while to investigate as many systems as possible, using the hints we get from those that have been identified. I have a hunch that we are going to find many control systems in a hierarchy of control. But I don't know that yet and wouldn't try to sell it. I only want to sell the idea that this is a fruitful way to go.

By the way, at the Felton meeting (cybernetics) I managed to wring out of Maturana a grudging admission that control theory might explain how autopoeisis works (I am NEVER going to be sure I spell that word right).

As to Gaia, no, I don't think that Gaia is a control system. I do think that all the organisms on earth are probably control systems, and that in acting to stabilize their local environments in states that they prefer, they end up stabilizing (to a lesser degree) their common environment. Each plant acts to maintain a certain oxygen concentration in the stagnation layer around its leaves. Any disturbance that tends to raise the concentration results in the plant's lowering its own oxygen output. When trillions of plants do this, the effect has to be to stabilize the general global oxygen concentration. But if you disturb the oxygen concentration 1000 feet up in the air, the plants aren't going to do anything about that. They care only about what's happening to them, individually. Yet Lovelock is still generally right: the environment is stabilized in many regards by the massive resistance of uncountable organisms to disturbances of their own local environments.

(5). I agree that we must study the simple stuff first. Unfortunately the simple stuff is not of much interest to most scientists: they want something with pizzazz. I have found studies of human beings wiggling sticks to be of much interest as well as educational, and I've tried to get people interested in applying control theory to biochemical systems, but most people want something either more practical or less practical than that. I think that progress in the more global applications will be in direct proportion to the amount of time we spend laying foundations. But I've stopped bitching about this, because people are going to be interested in what they're interested in. What I can do is show how to apply control-system principles in general, so others can try them out. My own interests are at a pretty low level, so I won't participate much in the high-level stuff (except in forums like this where we're more relaxed). Nor do I think that everyone ought to just wait until we have all the fundamentals nailed down. Nobody will go far wrong if the principles are applied consistently and everyone remembers that every idea needs testing every time it is used. All sciences need both theory and application.

Also, Cliff, there really aren't many control theorists doing basic modeling. I think a revolution is under way, but it's horribly understaffed. We know of lots of fields where we'd like to see real work done, but we just can't do it all.

(6) Yes, the origins of control systems are important. I've had some thoughts on that, which are now buried in the archives of CSGnet. I'll try to find them or maybe someone else who's better organized will. We don't have any particularly new ideas about this that other's haven't come across before. The only exception, maybe, is in applying principles of control to processes that have a random element in them, as in bacterial chemotaxis. Negative feedback can add powerful selection effects that might explain how those first molecules got started so quickly. But before we can go farther than that, somebody has to do a job on control processes at the biochemical level. So far nobody's doing that the way I would like to see it done. Everyone's still enchanted by oscillations, looking for strange attractors and chaotic patterns, etc.. What I would like to see is much simpler than that, to go back to (5).

Why don't we just try to identify a lot of controlled variables and see if they suggest levels of organized control? I don't think that good biochemical control systems are going to be in a state of constant chaotic oscillation!
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Thu, 14 Feb 91 12:12:45 CST
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: MR; meningitis and motivation

Thanks much to Joel Judd and Bill Powers for thought-provoking reactions to my queries about educational referencing.

All references (norms, criteria, self-report, background data) are really searches for criteria -- for benchmarks and bona fide evidence of learning. I agree that is a complex and challenging task.

Multiple referencing is essentially a politically motivated concept. Whatever you call it, if you can convince an educator to consult more than just one (or a few) sources of assessment information, you then **elevate** previously taboo data to the role of almighty measures; and I do agree that TOEFL needs to be critically questioned, constantly!

I guess my motivations and purposes are to set up systems where it is easier for an educator to consult previously taboo references. And they are taboo only because they are inconvenient. We have this marvelous norm machine working now, why muck it up?

But we are all, at our hearts, applied linguists in the language teaching field and we want to know what language acquisition really is. We search for criteria. So that is my question: what are 'criteria' in CT? Are they determined solely by the individual's desire to defuse error? Does CT speak to shared criteria across individuals -- for example, how would CT react to the universal grammar people who are desperately searching for the basics of syntax necessary to learn any language (this is sort of where Chomsky's non-political work is now...)?

-=-=-=-=-

One more thought. Those who doubt that human behavior is purposeful should talk to the students waiting in line outside the student health center here. Two students have died in the past week from a meningitis-related disease. As of yesterday, some 8,000 had been prophylactically treated -- They are scared of dying. If that aint a desire to balance an internally imbalanced system, I don't know what is.

-Fred Davidson

P.S. Joel: thanks for that book reference. I'll pursue it. I am spooling this thread into a separate notebook file

and can share it with anybody who wants.

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Date: Thu, 14 Feb 91 14:44:01 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: clinical modeling 3

Bill said: " The object of control theory can't be to explain one particular action. " Sometimes, however, an action is significant enough so that we want to explain it. Two examples from the news: (1) Why the Iraqi actions in Kuwait? (2) Why the Israeli actions in response to the SCUD missiles? I am only using these examples because it is a common experience for all of us.

For each of these actions, we can all generate several possible perceptions which might be controlled by the action. Bill said: " So I would propose backing up a step or two, and starting by testing Y1, Y2...YN." What is stopping us from doing this with either of the above two actions? I think the answer is: (a) that we do not have a way of disturbing each of the proposed controlled variables, (b) that it would be immoral/unethical to apply the disturbance in some cases, or (c) that the people we are disturbing may not like it, after a while, even if we do it in very subtle, indirect ways.

[Control Theory does offer a few procedures to help specify the meaning of an action. There is the how/why technique. Other than action X, how could you have gotten the same result? What result did action X produce for you?

There is the procedure of relative levels. You said that action X produced result Y1. What is it like to experience result Y1? You said that it was like Y2. What is it like to experience Y2?]

Let me throw out a challenge using either of the above two actions. In the tradition of a "thought experiment" , using the concepts and methods of Control Theory, explain in detail the way that you would find out what perceptions were being controlled by the above two actions.

Clinicians face the challenge posed above every day. I am not even talking about life or death situations. It is frustratingly hard to figure out what perceptions people in everyday situations are controlling.

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Date: Thu, 14 Feb 91 18:11:01 -0600
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: some thoughts

I've been trying to send messages but am having difficulties. Does this get through?

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Date: Thu, 14 Feb 91 18:22:01 -0600
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: another try

OK I'm getting very frustrated here, but I think this might be working finally. I've missed your discussion on Freeman and sit here now with all of it on paper to read. Sorry to have brought up a topic and not interacted since--not my intention.

I am happy to report that the solution proposed by Bill on my "attractive woman/I didn't know I wanted it till I saw it" problem, is a part of my thinking--it's such a nice feeling to know that a month ago it was a huge question and now its seemingly explainable.

My tip-of-the tongue question got some discussion today in my Cognitive Science class. I'm still working on whether the responses in this class to the question relate at all to your responses. But I'm not going to bring that up again given that I'm so far behind on this Freeman stuff.

One thought: "temptation" can be defined as a choice between two outputs which both satisfy higher reference levels, one (the temptation) having a higher loop gain than the other, (and the other might satisfy higher levels on a different hierarchy path). How's that sound?

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Date: Thu, 14 Feb 91 15:56:26 PST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joss Marlowe <joss@SOH.ENG.SUN.COM>
Subject: some thoughts

yes, that reached me. Sorry -- my entry is somewhat messed up in the YP map. the real address should be:

joss@Eng.Sun.COM

The above address should work tomorrow or whenever they get my problem fixed. I cannot guarantee that the address you used (joss@SOH.Eng.Sun.Com) will always work...

--joss

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Date: Thu, 14 Feb 91 21:41:17 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Criteria, Challenge

Fred Davidson (910214) --

>So that is my question: what are
>'criteria' in CT? Are they determined solely by the individual's
>desire to defuse error? Does CT speak to shared criteria across
>individuals -- for example, how would CT react to the universal
>grammar people who are desperately searching for the basics of
>syntax necessary to learn any language (this is sort of where
>Chomsky's non-political work is now...)?

Not sure what you mean by 'defusing' error. Error in control theory doesn't mean "mistake." It's just the difference between what you want and what you have. It is normally a very small difference, just enough to keep enough action maintained in the right direction. There's always error in a control system; in a good control system, not very much. But it's what drives action, so it's essential.

A lot of terms like criterion, aim, goal, objective, desire, and the like are just alternative ways of saying reference signal. They might imply somewhat different contexts -- for example, when people say "goal" they tend to mean that it's a state that hasn't been reached yet and might take some protracted maneuvering to get to; a "desire" is sort of an optional reference condition; an "objective" is a reference condition that's laid out in rather formal terms; a "criterion" is a reference state that is more or less kept in mind all the time. But these distinctions are very fuzzy and inconsistent. Also, they're usually projected into the objective world, where CT would analyze them as intended or wanted perceptions. Part of the job of applying control theory in existing fields is to re-analyze the situation that's being described by such words to see if it can be recast less ambiguously using the CT model.

Control theory also raises the question of whose reference signal is meant. If you are told that the criterion for passing this typing course is 100 words per minute for three minutes with no errors, you know whose reference level that is. Applying the criterion is basically a logic or program-level action: IF you type to criterion OR better, THEN I give you a passing grade.

A lot of terms succumb to analysis if you just ask in some detail how you can tell whether they apply. You can probably do better than my hasty examples.

My reaction to the universal-grammar people is that what they're searching for isn't linguistic in nature. They're really asking how the brain works no matter what kinds of perceptions it is handling. I think that linguistic and nonlinguistic activities are examples of one brain doing different things with the same equipment. I think that Chomsky has said something similar.

Sharing criteria (or reference signals) can't literally be done, can it? Just ask how two people could share any criterion -- and know that it is the same in both of them. Some pretty elaborate communication is needed even to try to do this, and you never know if you actually succeeded. Just consider the mechanics of it: how information gets out of one person into the air or onto paper, and how it gets from there into a second person, and then what's required for it to be understood, accepted, and put into practice.

David Goldstein (910214) --

>(1) Why the Iraqi actions in Kuwait? (2) Why the Israeli actions in
>response to the SCUD missles?

First we ask WHAT are the actions. Iraq sent soldiers into Kuwait. They shot people. They stole things and shipped them home. They dug fortifications, which they presently occupy. And of course they said a

lot of things while doing this. Those actions don't need to be explained: they happened; they speak for themselves. At one level, the goal was to produce those actions, and it was achieved.

What we do need to understand is what those actions were intended to accomplish, which is something other than the actions themselves. That's where we will find potential controlled variables. If we listen to the given reasons, we find that the Iraqis want better access to the Gulf, want Kuwait to stop tapping their oil fields, want their war debt to Kuwait cancelled, want a general discussion about peace in the Middle East, and so forth. You may say that the actions are a pretty dumb way of achieving control of those variables, and you may guess that there are unspoken variables like influence, power, and riches that may also be intended effects of the actions, but none of those things are the actions, either. The actions are only interesting because of their side-effects on us and the coalition countries. Under other circumstances, attempted control of the same variables might have led to using different actions.

The point I've been trying to make is that when we understand the outcomes that the actions are intended to produce, we can explain not only the actions that did occur, but those that will occur as circumstances change. We can understand other situations in which different actions occur, but in order to achieve the same results. Once we know what results are desired, we can explain the action taken by seeing how circumstances made it a reasonable or necessary way to reach the objective, given the state of the world at the time. According to many sources, for example, Saddam Hussein had reason to think that the action of invading Kuwait would not evoke any objections from the United States. This undoubtedly helped to make it seem a feasible way of reaching his goals. Of course this also reveals something about Saddam's system concepts and principles.

As to Israel's reaction, I won't go through the same litany again. I do wonder, however, how a dozen or so 500-pound bombs could do 3 billion dollars-worth of damage, especially considering the misses. Maybe Israel's actions are a pretty good way to get three billion dollars. Or maybe someone ought to investigate their building contractors. What I really think is that we're watching a big poker game for high stakes.

I'll think more about your challenge before saying more. It's a good one.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Fri, 15 Feb 91 09:08:33 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: be a Control Theory therapist for Gail

Let me start to present a case that I have now to illustrate the difficulties of applying the Control Theory approach in clinical practice. You can be my (unpaid) consultants. I will only present a little information now. I will answer any questions you pose if I have the answer. Otherwise, I will find out in the next session.

Let us call her Gail. She is in her early thirties. The major symptom of concern to her is a physical symptom, namely, a lump experience in her throat. She can distinguish two different kinds of lump experiences. She has been to physicians who tell her that it is not the result of any physical disease. She is not concerned that it means she has some dread physical disease.

This symptom is not present all the time. When it occurs it lasts for days. There is nothing she can do to make it go away. She feels angry and then depressed at this symptom. She has had it for years. She believes it is ruining her life.

What do you want to know in order to apply the Control Theory approach to this case?

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Date: Fri, 15 Feb 91 02:59:30 est
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: Pattern recognition: Place vs temporal codes
In-Reply-To: UPPOWER@BOGECNVE.BITNET's message of Tue, 12 Feb 91 20:33:37 -0600

Bill Powers mentioned this issue of the coding of signals in the nervous system. It does seem to be the tacit assumption among neuroscientists that the neuron only sends one signal (the instantaneous firing rate) down the axon and that this signal is distributed unchanged to all axonal branches and synapses. There is however evidence to the contrary-- that there can be multiplexing of signals in single neurons. The three papers I know that show this most convincingly are:

Bittner, GD (1968) Differentiation of nerve terminals in the crayfish opener muscle and its functional significance. J. Gen Physiol 51: 731-58

Chung, SH, Raymond, SA, and Lettvin, JY (1970) Multiple meaning in single visual units. Brain Behavior & Evolution 3:72-101
(see also Raymond & Lettvin (1978) Aftereffects of activity in peripheral axons as a clue to nervous coding. Waxman, ed. Physiology and Pathobiology of Axons, Raven Press, NY.)

Opticon, L and Richmond, BJ (1987) Temporal encoding of 2-dimensional patterns by single units in primate inferior temporal cortex. I, II, III. J. Neurophysiology 57(1): 132-178

Basically all three of these papers present evidence that 2 or more independent (orthogonal) signal dimensions are being utilized by the neurons in question. Lettvin and Raymond propose a coding by temporal interval and show possible mechanisms (via membrane threshold oscillations following each action potential) by which any patch of excitable membrane could function as a resonant filter. Raymond has subsequently shown that threshold changes are activity-dependent; thus this theory could provide the mechanism for implementing temporal coding and decoding and a model for learning via modulations of the time constants of neural oscillations.

There are many attractive aspects of such a model, and I believe that they are all compatible with control theory (as I understand it):

1) multiplexing allows non-locality of message passing (to get from A to B a signal does not need a dedicated pathway)

2) multiplexing means that signals need not compete with each other
   Place models are like telegraph networks where each node is receiving
   many inputs but can only transmit one output. This necessarily makes
   the propagation of the signals mutually-dependent. In a temporal code
   (like interspike intervals) each signal can have its own characteristic
   interval, so that the various signals need not be in a zero-sum
   competition. Multiplexed temporal codes are more like radio--another
   carrier frequency can be selected which will not interfere (much)
   with pre-existing signals.
3) Place models, having one signal per element, thus have to degrade
   previously stored patterns to store new ones. In the time/frequency
   domain new patterns can be stored in frequencies/temporal sequences
   which are orthogonal to previously existing ones. (Cliff Joslyn
   recently sent me a paper which uses this idea in optical information
   storage.(Marcus Cohen, J Applied Optics, 1986))

There is quite a lot more to this story (which we can take up for discussion
if there's interest). These temporal codes are still analog codes and the
same kinds of control-theoretic mechanisms can apply, despite the different
kind of signal encoding. (I am currently investigating these sorts of
coding mechanisms in the auditory system where (to me) the rate-place
models look completely inadequate.) If these temporal models are borne
out by the empirical evidence, new signal types would be generated by
the tuning of membrane threshold dynamics. This would be (in my language)
a semantic emergent event, or (in CSG lingo) structural reorganization
to form a new control structure.

Peter Cariani

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Date: Fri, 15 Feb 91 10:16:42 -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: be a Control Theory therapist for Gail

David,

>What do you want to know in order to apply the Control Theory
>approach to this case?

OK, I'll try something new--

How about 1) does she remember when the lump first appeared, and when does
it appear now?
2) What does she do when her perception is negated, ie. You are just
imagining it, there's nothing there, etc.?

Joel Judd

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Date: Fri, 15 Feb 91 11:40:09 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Therapist; Multiplexing

David Goldstein (910215) --

Happy to serve as an unpaid consultant. My contribution is worth every penny.

As a professed CT therapist, I would assume that this symptom, if psychogenic which I take to be established, is part of some effort this person is making to control something. Because the symptom is recurrent and persistent, I would assume that this effort is unsuccessful. If an effort is unsuccessful, there may well be some counter effort: in other words, a conflict is present. There are other possibilities, but they can be discovered.

I would not take her interpretation of her feelings at face value (as opposed to her report). She says she is angry and depressed AT having this "lump experience." From that I can glean that she is angry and depressed and is having a lump experience. The term "at" is an interpretation, which I would ignore. Emotions arise from a blocked desire to do something. I would try to find out what she wants to do when she is angry, and what she wants to do when she is depressed. I would persist until she tells me. I would ask her why she doesn't do these things (even if the answer seems obvious). I would continue to ask for details until the time appeared right to ask her what her attitude toward these details she had been describing is. As soon as she begins to answer that question, according to the hierarchical model, she has begun operating from a conscious point of view one (relative) level higher. Now she is in a position to begin talking about things closer to the reasons for which she is trying to control these things. I wouldn't venture to guess what those reasons will turn out to be: ask her. Nor would I venture to predict how many times this process would have to be repeated before the operative conflict would be revealed. But I'm pretty sure it would be revealed. And when she is in a position to see both sides of the conflict, the conflict will resolve. Or you will find out (and more important, she will find out) why not.

The doctor is --- x Out
In

Peter Cariani (910215) --

Welcome to CSGnet, Peter. You arrive bearing interesting materials.

We could do this several ways. I could start the machinery for getting those papers through my local public library and we could continue this in three or four weeks. Or you could send me Xeroxes of them and we could continue after I've studied them. Or I could ask some questions and you could do all that hard work for me. I'll blithely assume that the third alternative appeals to you as much as it does to me.

First, let me describe how I see neural computations as being carried out. I assume that the basic signal is a variable rate of firing (frequency modulation). When a signal reaches a synapse, it releases jolts of neurotransmitter in synchronism with the arriving impulses. The neurotransmitter diffuses across the synaptic gap, its average concentration being a function of the rate at which impulses of transmitter are released, the rate of diffusion out of the gap, the rate of metabolism of the transmitter, and the rate of uptake by the dendrite receiving the signal. Inside the dendrite (I'm a little hazy on this), signal molecules are released by the interaction of transmitter with the

membrane. This signal substance has a concentration that reflects the mean rate at which incoming impulses arrive.

All incoming signals work the same way. The signal molecules inside the receiving cell diffuse toward the hillock, probably with some interactions, and their net smoothly varying concentration affects the postsynaptic potential there. We are now working with continuously-variable concentrations and potentials. The potential at the hillock then determines the rate at which the neuron fires, and hence the signal frequency leaving via the axon. There are also some connections at the axon end, which can further modulate the output frequency.

So I see this whole process as an analogue computer, a converter of frequency inputs to frequency outputs. There are other types of neurons but I believe this arrangement is by far the most common.

OK, now to the idea of "multiplexing" incoming signals. In order to multiplex, two things are needed: a way of combining the multiple input signals such that they retain their independence, and a way of demultiplexing at the other end that sorts out the independent signals into separate channels again. This leads to my first set of related questions, which may be enough to consider for now.

Do these papers consider that individual impulses are significant? In other words, are they interpreting impulses as bits in a digital signal? If so, multiplexing can be done three ways that I know of: time-division, phase division, or amplitude division. For time or phase division to work, the receiving end must have some machinery for deriving a reference phase or a clock from the signal. For amplitude division to work, the receiving end must be able to distinguish impulses on the basis of their size (one detector would ignore impulses over a given size, the other would ignore them below a given size (for two channels)).

I confess that if neurons work the way I described above, it's very hard for me to see how any demultiplexing could take place. Do the authors discuss this problem? Have they found signals downstream that correspond individually to the separate input signals before the multiplexer?

The other main possibility is that the authors are treating signals as analog signals, as I do. If that is the case, "multiplexing" no longer has any meaning, and I would have to ask what they intend by this term. I have no difficulty in seeing the processes in the soma as generating a single output signal that is some function of multiple input signals. The outgoing signal then represents the value of the function, while the various input signals represent the values of the arguments (all in terms of a frequency variable). This is not multiplexing, even though the outgoing signal's frequency depends on several incoming frequencies at the same time. Once the output is expressed as a single frequency, there is no longer any way to work backward to deduce the states of the individual inputs that contributed to it.

Can you find answers to these questions in the articles?

Again, welcome aboard.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Fri, 15 Feb 91 10:24:34 -0800

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: Words & Models

I just read Bill Powers' nice, clear reply to Bandura in American Psychologist. (It was also nice to see a letter, on a completely different subject, by CSG's own David McCord -- how are you doing David?). I also read Bandura's reply to Powers' comments. What the entire exchange shows to me is the difficulty of having a fruitful verbal discussion that is not firmly anchored in modeling. Bandura, like Skinner, uses language as a model. If it "sounds right" then it is right. The only way to deal with this is to ignore it. It's like a story Richard Feynman told about arguing with some orthodox jews about some of their explanations of why one should and should not do certain things. As I recall, Feynman said they "mopped the floor up with him" or something like that. I feel this way when I argue with my mother, who can make up verbalisms that seem to be explanations faster than Skinner could have dreamed. I think many people feel like they understand stuff when they hear words the sound good. This is particularly true of many psychologists. So the Banduras and Skinners and Jungs and their ilk will be the winners in the standard psychological forum for some time to come. I don't mind this any more. It's just that reading it and listening to it gives me a headache. So I try to avoid it. Kudos to Bill for having the patience to try to give linguistic expression to a model that is really not linguistic (though, like Bandura, the model can talk).

Bandura's comments actually did give me an idea. I think a common complaint about control theory is that it doesn't take planning, anticipation or "feedforward" into account. Of course, it does, but in a way that is quite unfamiliar to those who say this. The model controls higher order variables that are to some extent defined over time. Thus, what looks like anticipation of the next curve in the road is just the control of the perception of a well know route from point A to B. My idea is to incorporate the "temporal" variables into my spreadsheet control model. The columns of the matrix could represent the values of variables at different times rather than different spatial positions. The hierarchy of control systems is again represented by the rows of the matrix. Time changes in the environmental variables are represented by a shift of all values to the right, say. So a disturbance is just a new value of the termoral waveform at the left. The perception of the time signal (over columns) is now a temporal rather than a spatial integration and outputs will be affecting a variable that is changing over time. I'll work on this a bit this weekend but if anyone gets the gist of what I want to do and has any suggestions (especially in terms of possible experiments) please feel free to give them. The goal is to design a control system that controls a representation of a variable that is defined over time.

Best regards

Rick M.

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213 336-6214 (day)
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Date: Fri, 15 Feb 91 14:14:52 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: Case of Gail

Joel Judd wants to know: (1) Does she remember when the lump first appeared, and when does it appear now? and (2) What does she do when her perception is negated, i.e., You are just imagining it, there's nothing there, etc..?

Answers: (1) The lump first appeared when she was in her early twenties. When it appears now, it always surprises her.

In her early twenties, there were two episodes she recalls which is linked to it. On a trip with a girlfriend down South someplace, she jumped out of a second story window to escape a group of men who were coming after her in order to rape her. The second episode occurred when she was smoking pot which, unknown to her, was laced with LSD. These episodes were followed by a period in which she experienced anxiety symptoms. The anxiety symptoms have plagued her from the early twenties.

The lump experiences now can be triggered by a variety of circumstances such as: (a) Her son yelling at her., (b) A car almost hitting her car., (c) A fellow worker saying something to her like " Couldn't you take that call?," (d) A man she is dating not calling her., (d) The person who does her nails saying " Gail, Gail, where are you!, (e) Gail going on a trip someplace, (f) A female boss calling her "dear", (g) Her ex-husband demanding her to speak to the woman who carpools with their son about the unsafeness of the woman's car.

(2) I have not negated the perception. I would expect that she would become very upset and say things like: Don't you believe me. I am not making it up. I don't expect much out of life, all I want is the lump to go away.

Gail has elevated levels of skeletal muscle tension as measured by surface EMG readings with electrode placement on the forehead. She has some TMJ symptoms as well. All of this supports her report of feeling a lump in her throat.

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Date: Fri, 15 Feb 91 10:40:54 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mar@CS.ABER.AC.UK
Subject: Neural feedforward

Gary (910213) and Rick (910213) --

Thanks a lot for your references.

To all --

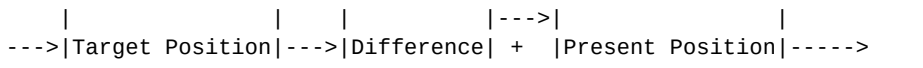
What about neural feedforward? I'm very intrigued for several weeks
on this. At one moment I accept it; next I reject it.
It might be just a special case of feedback, but I cannot see it
quite clearly.

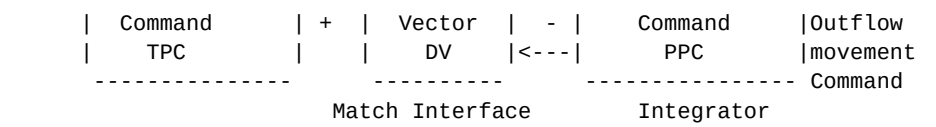
Stephen Grossberg (Center for Adaptive Systems, Boston University)
"proves" the existence of neural feedforward based on some
experiments with monkeys. He also proposes a model which I will try to
describe.

Grossberg describes the experiment like this [some bits deleted/edited]:
"... monkeys were trained to move their forearms without visual feedback
of hand position from a canonical starting position to the position
of one or several lights. The monkey's arm movements were studied
both before and after a dorsal rhizotomy was performed to remove all
sensory feedback from the arm. Before deafferentation, so long as
the spatial conditions of training were maintained -- in particular
the canonical starting orientation and position with respect to the
known target array -- the animal remained able to move its hand to
the target position. However, if the initial position of the upper
arm and elbow of the deafferented arm was passively shifted from the
position used throughout the training, then the animal's forearm
movements terminated at a position shifted by an equal amount away
from the target position. Thus the movement of the forearm did not
compensate for the change in initial position of the upper arm.
Instead the same final synergy of forearm-controlling muscles was
generated in both cases. ..."

He continues on arm movements: "... We hereby provide an explanation
of the compensatory effect that avoids invoking a special mechanism
of ``error correction'' for a movement which does not generate an
error in achieving its target...
... When a new target position command (TPC) is switched on, its
relationship to the current present position command (PPC) can be
arbitrary. Any realizable pair of positions can be coded by
TPC and PPC. In order to track TPC, the PPC needs to change in a
direction determined by the difference between TPC and PPC. In addition,
the _amount_ of required change is also determined by this difference.
An array which measures both the direction and distance between a pair of
arrays TPC and PPC is called a _difference vector_, or DV.
At any given time, the DV between the TPC and PPC -- namely, DV=TPC-PPC --
is computed at a match interface [diagram below]..."

"...How does such a DV updates the current PPC? Clearly PPC must be
updated in the direction specified by the DV. Hence we assume that
PPC cumulatively adds, or integrates, through time all the DV's which
arise at the match interface. Due to this arrangement the PPC gradually
approaches the TPC. At a time when the PPC equals the TPC, the DV
equals zero; hence, although the PPC may continue to integrate DV's,
it will not further change it until either the switching on of a new
TPC creates a non-zero DV, of the PPC is updated by inflow [feedback]
information during a passive movement..."





"...A match interface within the motor command channel continuously computes the difference between the target position and present position, and adds the difference to the present position command."

This is his model of neural feedforward (outflow). I don't know why, but I'm having difficulty with something that should be straightforward: the simple mathematics of this scheme.

There are a few more things which are not quite clear to me: suppose the monkey's arm is at its usual initial position (of training).
How does it know where to stop the arm with no afferent information or visual feedback? Does neural feedforward exist or not? Is it a special case of feedback?

Marcos.

Marcos Rodrigues
Univ. College of Wales, Dept CompSci, Aberystwyth, UK, mar@uk.ac.aber.cs

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Date: Fri, 15 Feb 91 18:45:48 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Deafferentation, Feedforward

Marcos Rodrigues (910215) --

There seems to be a concerted effort going on to "prove" that behavior can take place without feedback. At lot of it (particularly the work of Taub) seems to be aimed at refuting the idea that behavior -- action -- is impossible without feedback, a thesis which as far as I know has never been advanced by a control theorist. I don't know why this is going on, but I have a fuzzy guess about it. All real feedback control models have inside them a reference signal. Because of this signal and all that it implies, feedback control models suggest that there is autonomous agency inside of organisms. As soon as agency appears on the scene, many old issues that some people consider to have been settled once and for all suddenly become viable again: animal awareness/consciousness, animal intelligence, purposefulness, and of course the whole question of who controls whom or what to meet whose goals. I don't think that the biologists/neurologists involved have enough understanding of control principles to see clearly what all the implications are, but I think they get a sense of opening a can of worms, and would rather not do so. From their point of view, it would be much more convenient if experimental animals remained as "preparations" that can be treated as if they shared no part of human experience, but were simply objects available for use. I don't know if this is any part of the motivation. But it seems a not-unreasonable hypothesis to keep in mind as you wonder why such effort is being devoted to proving something that is obviously already believed.

I speak of a motivation here (in this context: unrevealed reference

criteria) rather than reasons, because the arguments concerning deafferented behavior are vague, sketchy, and ill-conceived. Neither the observations nor the models offered are complete enough to support the conclusions that are given. Something else must be driving this effort.

There are many problems with deafferentation studies of the kind you cite. In the first place, a dorsal rhizotomy probably does not cut off all kinesthetic feedback paths. There are "auxiliary" paths for both central and autonomic input signals that pass through the ventral roots (to cut them you'd have to cut the motor nerves, too). In the recovery period after an operation, animals can learn to use the auxiliary feedback paths (which are probably used normally anyway). Tom Bourbon can say more on this subject.

In the second place, nobody ever said that cutting feedback paths disables the efferent neural pathways. Just consider how a spinal reflex works as a control system. The "command" signal reaching the spinal motoneuron (and, via the gamma efferents, the muscle-spindle comparators) is really a reference signal. From this reference signal is subtracted the perceptual signal (either neurally in the motoneuron or mechanically in the muscle spindle). The difference signal is what normally operates the muscles, as the error signal in a very cleverly-designed control loop that is highly stable.

Now if you cut off the feedback paths, all you are doing is removing the subtractive signal from the comparator. The reference signal still remains, and it will result in an error signal -- a very large error signal, because there is no sensory feedback signal subtracting from it. The initial effect of loss of kinesthetic feedback is, I believe, well-known to be a gross exaggeration of movements. Far from losing the ability to produce movements, the system begins producing far too much movement.

But there are higher-level systems that are using this spinal loop as an output function. Those higher-level systems use kinesthetic information from joint receptors, skin receptors, visual receptors, and even receptors in other parts of the body that are indirectly affected by reaction forces from limb movements. As an animal recovers from loss of negative feedback in its peripheral control systems, its higher-order systems will reorganize. Their amplification will become less, to compensate for the overdriving of the lower systems that lack negative feedback. New uses may be made of other feedback information. Eventually the higher-level loop that remains, now using the un-fed-back lower level systems for output, becomes stable again and even somewhat competent. But it has lost the ability to make rapid, precise, and disturbance-resistant movements, because those facilities are provided by the peripheral kinesthetic feedback.

Many of the demonstrations of deafferentation effects (or their absence) are basically qualitative. If the animal can somehow get its limb from one position to another, no matter how slowly or awkwardly or imprecisely, the result is reported as "no effect." In some instances, the apparatus itself (the limb support that is often used -- why?) introduces damping that would stabilize a system that has lost its stability.

If the objective is to prove that motion is still possible in the absence

of feedback, then perhaps a qualitative demonstration is enough. But that proof would be offered against a straw man. To show that peripheral feedback has no important function, it is necessary to do real control experiments, in which the properties of the control systems (loop gain, dynamic stability, resistance to disturbance, etc.) are measured. If such measures proved to be unchanged by deafferentation, I would be astonished and thoroughly taken aback, and would have to search for a new model. But if they were done competently, I would have to accept them. So far I have not seen anything approaching a competent measurement of control properties in such experiments.

Perhaps it's just as well that these experiments are being done by people who are basically amateurs in the field of control system analysis. A real control-system expert probably wouldn't even think of doing them, because it's so obvious that negative feedback is essential in skilled behavior. The obvious is always worth a second look -- but once the second look is taken, it needs to be supplemented with a more critical investigation by people who know what they are doing. So far that hasn't happened.

As to the Grossberg model, I'd have to see a working version of it before I could comment on it. It looks sort of like the Mittlestadt "re-afference" model. It also looks like the design of the Three-Mile-Island control system for controlling the water flow to the reactor: the feedback signal shows what the command was, but it doesn't reveal whether the effect of the command actually happened. Such a system couldn't handle variable loads or obstacles. Grossberg proposes a lot of entities -- his TPC, DV, and PPC -- but he doesn't say how they work. As far as I can see, they're just defined to have whatever properties are needed to make the result come out right, without spelling out those properties. Show me.

You're quite justified in having difficulties with the simple mathematics of Grossberg's model. And don't worry about feedforward. It's just output from a higher-level feedback system. Even that old standby the vestibular reflex has been shown to adapt to disturbances and changes in eye movement response by increasing or decreasing as appropriate -- under control of a slower higher-level system. Takes 20 minutes or so. Maybe somebody else has that reference handy -- I don't. It was in Science.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Fri, 15 Feb 91 23:06:54 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: case of Gail

Bill Powers says that Gail's symptom...is part of some effort she is making to control something. If he had put this in the form of a question, it might have been: What function is the symptom playing? What desired outcome occurs as a result of the symptom? In non-Control Theory approaches, this kind of question often comes under the heading of " secondary gain ."

Answer: Some guesses I have made to date are: Gail distracts herself with the symptom. Gail punishes herself with the symptom.

Gail stops herself from expressing herself with the symptom. Gail uses the symptom to confirm her low self-image. Gail uses the symptom to get help from doctors which substitutes for the caring person who has been missing in her life.

It is always possible that the symptom does not have any specific function now. Gail becomes stressed and has bodily stress reactions. The lump experience in the throat is the part of this stress reaction which she notices. It is the " weakest link " in the chain. The use of a special form of electrical stimulation on her throat, provided by a device called the Pain Suppressor Unit, has reduced the intensity of the lump experience on four out of four occasion to some degree. This is the only thing which has reduced the symptom intensity to any degree whatsoever. Anti-anxiety medicine has not. Anti-depressant medicine has not. Acupuncture has not. General relaxation has not.

The symptom might have had a function in the past but does not have a function in the present. The old way of describing this is to say " it is just a habit. "

Bill says that the symptom is an unsuccessful effort because...the symptom is recurrent and persistent. I don't follow this exactly. Does this mean that breathing is an unsuccessful effort because it is recurrent and persistent? The corresponding question is: To what degree does the symptom reduce an error signal?

Given that I don't know for sure what the function the symptom serves, I can't answer the question of how successful it is in accomplishing this purpose. I assume that if Gail had some better way of functioning then she would use it and give up the symptom. She really gives all appearances of hating the symptom.

Bill, in a move designed to warm the heart and minds of psychodynamically oriented therapists, concludes that the symptom may be a sign of a conflict at the next higher level in the perceptual hierarchy. He then proceeds to tell us how to identify the nature of the conflict. Once the conflict is identified at a conscious level, ...the conflict will resolve or ...we will find out why not.

Put in the form of questions: Is this symptom a sign of a conflict? What does Gail want to do when she is angry? What does Gail want to do when she is depressed? Why does she not do what she wants to do? What is her attitude about what she just said?

A conflict can be followed by the symptom. There is one instance I can remember in which this occurred. Fear feelings can be followed by the symptom. Angry feelings can be followed by the symptom. Rejection feelings can be followed by the symptom. I don't have any way of knowing for sure what percentage of the time the symptom is a sign of a conflict.

When Gail is angry she usually keeps it to herself. The only people she allows herself to yell at are her son, her mother and her twin sister. She does not throw things. She does not curse.

She does not punch or hit. She has been the target of abuse at the hands of her ex-husband (verbal and physical). She has been abused by her father (verbal).

When Gail is depressed she does not feel like doing anything. Like many depressed people, she stays home. She escapes into sleep. She withdraws from people. She thinks self critical, negative thoughts.

I do not believe that Gail has any strong urge to take some kind of action when she is angry or depressed which she is stopping herself from doing. The only thing that I can think of is that Gail wants to be more spontaneous. She wants to have more fun. However, she cannot enjoy herself because she is so self-focused on the symptom and she feels so bad.

The conflict has not revealed itself up to this point in therapy. The symptoms continue. Once the symptom starts, it seems to have a life of its own. It will last until it decides to go away. She feels as though she has no control over it. Even if she resolves the interpersonal situation which started the symptom, it continues. Her attention is riveted to the symptom. She is reminded of the situation which seemed to precipitate it. She obsesses about the symptom. She thinks that no man will ever want her because she is flawed in this way and has emotional problems. Insight into the situation does not seem to help her.

What does Gail want to do when she is depressed? She want to stay home and sleep and do nothing. She wants to give up. She wants to go back in life, be reborn again and start over again.

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Date: Sat, 16 Feb 91 10:14:45 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: CT Therapist

David Goldstein (910215) --

At my consulting rates I can't get so complex. Psychodynamics, yet.

>Answer: Some guesses I have made to date are: Gail distracts
>herself with the symptom. Gail punishes herself with the symptom.
>Gail stops herself from expressing herself with the symptom. Gail
>uses the symptom to confirm her low self-image. Gail uses the
>symptom to get help from doctors which substitutes for the caring
>person who has been missing in her life.

I would put together simpler guesses, for which you have supplied some evidence:

>Fear feelings can be followed by the symptom. Angry feelings can be
>followed by the symptom. Rejection feelings can be followed by the
>symptom.

AND

>When Gail is angry she usually keeps it to herself. The only
>people she allows herself to yell at are her son, her mother and
>her twin sister. She does not throw things. She does not curse.
>She does not punch or hit. She has been the target of abuse at
>the hands of her ex-husband (verbal and physical). She has been
>abused by her father (verbal).

I theorize that "fear" results from wanting to get away from something but preventing yourself from doing so, that "anger" results from wanting to attack something but not doing so, and that feeling "rejected" results from, or in, desiring to seek acceptance but not doing so. (Feeling rejected doesn't fit the pattern, because that doesn't describe a feeling -- when she's rejected is she glad, indifferent, disappointed, fearful, grief-stricken, or what? This is an example of leaving a goal implicit and describing the external disturbance instead. So you were rejected, that's just a fact. What did you want to do about it?).

If you want to get away from something but aren't doing so, then I presume there is a second goal that is incompatible with getting away, such as not showing fear, needing to stay where you are for some reason, wanting to be near the person you fear, and so on. If she feels like getting away, why doesn't she get away? Her muscles still work. The answer will be the other side of one conflict. Ditto for anger, the desire to attack. If she desires to attack and doesn't do so, there must be some reference level that would be violated if she attacked. What is it? She can probably tell you what her reasons are for not running away and for not attacking (except her son, her mother, and her twin sister: does she have the lump experience while she's attacking them by yelling?).

Of course the result of wanting to attack or flee and also wanting not to attack or flee is a net lack of action, isn't it? The goals cancel. The body feels aroused (the feeling of anger or fear) but the muscles are not doing anything energetic. You're just stuck with the feeling of arousal. You call it by the emotion-name appropriate to the circumstances.

>What does Gail want to do when she is depressed? She wants to stay
>home and sleep and do nothing.

When you're locked up in conflict, the only thing left is to stay home, sleep, and do nothing. Depression is the natural outcome of conflict. I think, however, that she has the beginnings of the right idea:

>She wants to give up. She wants to go back in life, be reborn again and
>start over again.

I agree: she needs to give up one side of these conflicts or the other, or both, reorganize, and start over. Does she understand that giving up and starting over is the whole idea?

I was thinking very simply about the lump in the throat, guessing that it might result from wanting to scream and not daring to scream. For example, while you're trying to sneak silently away from a gang of rapists, wanting to scream for help but not wanting them to know where you are. Ask her to scream very loudly and to see if the feeling of screaming is in the same place as the lump experience.

My guesses aren't very "psychological." But I think they're good control theory. I think that plenty of conflicts are in evidence. If you followed them up in detail, they would probably show the way to higher-level conflicts. But the physical symptom suggests a low-level conflict.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Sat, 16 Feb 91 12:51:25 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Jay Mittenthal <mitten@UX1.CSO.UIUC.EDU>
Subject: Re: Comparative Control Theory

Gary, the issues you raise are interesting; they cover much of comparative neuroethology. This is too far from my knowledge for me to contribute anything constructive, except that your ideas seem reasonable. Aside from some work by Mittelstadt on control systems in praying mantis strike, and maybe some other work from the control theory viewpoint (on flight of flies, & walking stick behavior) I don't know how much ethological problems have been studied from a control theory viewpoint.

I'm off to Europe for a meeting, back March 3, so can't converse more before then.
best, Jay.

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Date: Sun, 17 Feb 91 00:33:53 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: case of gail

As is evident from Bill Powers' comments, he sees signs of " plenty of conflicts " BUT " the physical symptom suggests a low-level conflict " in the case of Gail.

Gail has other physical symptoms which originated during times of stress. She has stomach problems which started during the stress period when she was abused by her husband. This has resulted in the necessity to monitor what and how much she eats and drinks. She will experience stomach pain and discomfort if she eats or drinks the wrong things. She hates the limits this puts on her. She feels like " a prisoner in her own body. " In recent months, with the help of a nutritionist, she has relearned that she can gain some control over the stomach problems if she monitors food and drink carefully. She resents having to be so careful. It spoils her from being " sporadic " and having fun.

Gail is angry at the fact that she has these stomach problems. Does she want to attack someone or something? Her ex-husband maybe because he was the source of the stress. Her body maybe because it stops her from eating and drinking at will. It would not make sense at this point to attack her husband in some way or attack her body in some way. So these angry feelings can never really be released at the source of the disturbance. Every time her stomach acts up, the angry feeling starts again.

The stomach problems, as well as the lump in the throat experiences, have consequences for Gail's social life as well

for her eating/drinking. Gail does not want to go out with guys because of it. She would have to watch what she eats and drinks with them. Her breath smells when her stomach is acting up. She would be afraid that if she kissed someone, he might taste/smell it. Why would a guy want a girl with emotional problems when he others are available, she thinks. This kind of thinking discourages her from wanting to date at this point. Yet she recognizes that she wants a man in her life.

How does Control Theory explain the fact that some people with higher level conflicts (unknown at this point in the case of Gail) will develop lower level symptoms? The conflict between two control systems at level n, results in the failure of those control systems at level n-1 which feed perceptual signals into these two control systems from maintaining their reference signals. This scenario repeats itself and gets passed down the control system hierarchy to the control systems at level 1 which result in excessive skeletal muscle tensions. This analysis points to the conclusion that the specific muscles which are tight relate to the higher level conflict. Freud would have loved this analysis! Gail's lump symptoms probably point to the conflict which Bill talks about.

Chronic error signals result in a chronic aroused state which is commonly called stress. The non-skeletal muscular components of the stress response result in idiopathic bodily symptoms. Some people develop ulcers. Some people develop bowel symptoms. Some people develop skin conditions. These symptoms, it seems to me, do not relate directly to the conflict. They simple indicate that this person has been under stress for a long time and this has resulted in problems in the body physiology or anatomy. Gail's stomach problems are probably an example of this nonspecific effect.

If the above ideas make any sense, then I should follow Bill's suggestions for the lump symptoms which should relate to a specific conflict. The stomach problems probably do not relate to a specific conflict.

I suspect that the conflict will center around the issue of expressing angry feelings. I have given Gail two " affirmations " or thoughts to repeat to herself often: (1) I will no longer let anyone abuse me in anyway. (2) I will no longer remain silent when someone abuses me.

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Date: Sun, 17 Feb 91 14:40:26 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: gail4

I would like to summarize the general statements which Bill Powers has made during discussing the case of Gail.
"Emotions arise from a blocked desire to do something." "The body feels aroused (the feeling of anger or fear) but the muscles are not doing anything energetic. You're just stuck with the feeling of arousal. You call it by the emotion-name appropriate

to the circumstances."

"Fear results from wanting to get away from something but preventing yourself from doing so."

"Anger results from wanting to attack something but not doing so."

"Depression is the natural outcome of conflict."

When the obstacle for achieving a desire is the output of another control system, this defines conflict. The therapist should ask a person what they want to do when they feel a certain emotion. The therapist should ask a person why they don't do it. The therapist should repeat this procedure as many times as necessary.Then the therapist asks the person what is the attitude s/he has to what s/he is saying. This may result in going to the level above the one where the conflict exists.

"She needs to give up one side of these conflicts or the other, or both, reorganize and start over."

Some general observations which I have made along the discussion pathway are: Bill Powers' ideas are very close to the psychoanalytic approach to psychosomatic symptoms. [This is OK Bill, you are allowed.] There is a linkage between the specific symptoms which are observed and the specific error signals which contribute to the chronic error signals. The therapy implication is that the therapist should make an effort to discover the nature of the error signals through talking. The person has to reorganize the control systems involved in the chronic error signals. I speculated that skeletal muscle tension symptoms may follow this line of thinking more closely. This assumes that the skeletal muscle tension symptoms are not the result of an accident or injury. In Gail's case, maybe her lump symptoms can be traced to a specific conflict.

A different perspective, more popular in current behavioral medicine circles, is that psychosomatic symptoms are a nonspecific anatomical or physiological result of chronic stress reactions. There is no linkage between the specific symptoms which result and the error signals which contributed to the error signals. The implication is that the therapist should teach the person relaxation skills to reduce the chronic error signals. The body's natural healing processes will do the rest once the chronic error signals are reduced. I speculate that smooth muscle tension symptoms (for example, cold hands) and, in general, autonomic mediated symptoms (for example, sweaty palms) may follow this line of reasoning. In Gail's case, maybe her stomach pain, nauseous symptoms, etc.. can be traced to general stress.

Where does this leave the case of Gail? I will pursue trying to identify the perceptions being controlled by the lump symptoms in her neck area. Reorganization must then follow the insight. It cannot be assumed that reorganization will automatically occur after the insight. Teaching Gail to express appropriately her

angry feelings in interpersonal situations, before the lump occurs, will be a general kind of intervention strategy. The general problem is that the lump experiences occur so quickly. And once it occurs, Gail does not know how to turn it off.

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Date: Sun, 17 Feb 91 14:41:48 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Comments from Mary

[From Mary Powers]

Back from Durango with various thoughts:

The Fort Lewis College campus is very nice - much more compact than IUP. We'll have next door meeting and computer rooms, with a conversation pit in the lobby a few steps away and an outdoor amphitheater we can also use. No hills, once you get up on the mesa where the college is (joggers - the rim road is 1.8 miles around). More when we send out registration forms...

The Grandchild (21 months) is in high gear for language acquisition - uppy uppy to be picked up, juicies for a drink, and so forth. But why, when he can repeat almost any word in a fairly comprehensible way, is he adamant that a greenhouse is a tosano? Does he think he is saying greenhouse? It's a word he hears all the time, since that's the business his parents are in and the darned thing is right outside the back door and he is in it every day. Does he hear it inside out? He seems to have no error between what he hears said and what he says.

Peculiar phenomenon #2. While out there I treated my battered bod to a massage. When she got to my traumatized leg I started to feel cold, and got colder and colder and shivery, even when she turned up the heating pad on the table and piled covers on the rest of me - and I lay there with tears leaking out realizing that this was how I had felt in the ambulance, the emergency room, and so forth, when I was in shock and bleeding internally. When she got done with that leg and did the other, I warmed up again. The price of admission to this interesting experience was rather high, but it was fascinating and even delightful, in retrospect. I haven't the faintest idea how to integrate it with anything else I know or think about, including control theory - except (dare I say it in this refined company?) dianetics. Any other ideas? Larry? Are you on the net yet? (To those on the net at UIUC, Larry Goldfarb has recently joined the Kinesiology Dept. as an instructor and PhD candidate, and Bill and I have been privileged at various ASC meetings to participate in his demonstrations of the Feldenkrais method of physical therapy - which he explains in largely control theoretic terms).

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

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Date: Sun, 17 Feb 91 20:43:53 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Gail4

David Goldstein (910217) --

[Now I'm going to go out on a limb regarding the Gail case. David is an experienced practicing therapist and he knows this woman; neither is true of me. In the following I am not arguing from authority, but simply trying to follow out the principles of control theory as I understand them and as I think they might apply. If I dispute David's approach, it is only to say that control theory, as I understand it, would recommend doing something different, and not that David is wrong. It would be pleasant to see a few others sticking their necks out, too.]

>It would
>not make sense at this point to attack her husband in some way or
>attack her body in some way. So these angry feelings can never
>really be released at the source of the disturbance. Every time
>her stomach acts up, the angry feeling starts again.

Neither attacking her husband nor holding herself back would release any feelings. She is hurting herself by trying to do both at once. Succeeding at either one, while the conflict still exists, would intensify the conflict. In control theory, angry feelings are not like an inner pressure that has to be released. That is a metaphor, but a false one. Feelings can be "released" only by time (allowing the adrenaline and so on to be metabolized away after the conflict disappears), by acting energetically to use the physiologically-prepared state in the normal way (which requires, of course, that there be no conflict to prevent action), or by removing or revising the conflicting goals that are creating the feelings in the first place. The approach I would recommend entails the third choice, which deals with the source of the feelings rather than their consequences.

It is not what Gail is doing but what she wants to do that is causing the problem. She has incompatible goals that keep her from acting at all. It might not make sense for Gail to want to attack her absent husband or oppose her own wish to do something, but that might very well be what she is doing anyway. Finding a higher-level viewpoint would permit her to realize that she DOES want to do something that is not sensible. Realizing that it isn't sensible WHILE OPERATING FROM A VIEWPOINT THAT HAS THE POWER TO MAKE AND UNMAKE SUCH DESIRES should suffice to alter what she wants, and resolve the conflict. She may decide to look up her husband and yell at him. Or she may decide to stop wanting to do that. That's her business. Of course I would admit that intervention might well be called for, temporarily, if she reorganizes in a way that is too destructive. That's YOUR business.

If you could lead Gail to a state of awareness from which she can say "Yes, I want to smash that son-of-a-bitch's face" and "No, I mustn't do that because he'd kill me," she might realize all by herself that this scenario no longer applies, as she is not with him. If that is truly the basis of the conflict, the conflict will simply disappear at the moment that she see that NEITHER goal is needed. But she can't do this while her conscious attention is identified with the system that simply accepts a goal and tries to carry it out, the goal she then feels as HER goal.

>I suspect that the conflict will center around the issue of
>expressing angry feelings. I have given Gail two " affirmations "
>or thoughts to repeat to herself often: (1) I will no longer let
>anyone abuse me in anyway. (2) I will no longer remain silent
>when someone abuses me.

If the conflict is about the issue of expressing angry feelings, what makes it an issue is that she wants to express them and she wants not to express them -- for different reasons, of course. She can't simply decide which she wants to do because her awareness is not operating from the level that chooses these goals, but from the level that carries them out. These affirmations tell her to pick one side of the conflict and throw more effort into it. She may indeed do so, at least temporarily. If she does, the other side will experience greater error and will increase its efforts, thus making the conflict worse than it is now. Nothing has happened to change the other goal. If the conflict becomes severe enough, she may begin to reorganize at a higher and higher rate until she becomes disorganized or picks an extreme behavior as an alternative. The expectation, if I am applying control theory consistently, is therefore that these affirmations will increase her level of stress and will increase the severity of physical symptoms.

The going-up-a-level approach, on the other hand, does not require any effort on Gail's part other than directing her attention to background thoughts and attitudes that are there anyway, and paying attention to them instead of to how much she wants to win the conflict. In my experience with applying this method experimentally, in non-clinical situations, the result of success in going up a level is an immediate drop in stress and a striking change in affect for the better. The scope of awareness seems to broaden -- if a conflict is in question, it is not even neccesary to point out that it exists. I have seen the "other side" become visible simply through the shift in viewpoint. This method does not require the experimenter/therapist to control the subject's behavior or to urge the subject to take any particular action with or against a conflicted goal. It simply puts the subject's awareness in a position from which more effective choices of goals can be made. It is still "intervention," but of a kind different from what you suggest.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Sun, 17 Feb 91 21:41:36 -0600
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: Language Acquisition

Mary Powers (via Bill Powers) (910217)

>But why, when he can repeat almost any word in a fairly
>comprehensible way, is he adamant that a greenhouse is a tosano?
>Does he think he is saying greenhouse? It's a word he hears all
>the time, since that's the business his parents are in and the
>darned thing is right outside the back door and he is in it every
>day. Does he hear it inside out? He seems to have no error
>between what he hears said and what he says.

Joel Judd have been agonizing over trying to apply control theory to

language acquisition for the last several months. It ain't easy, but potentially very fruitful (we hope). Here's a few thoughts about your grandchild (you should have given his name).

He wasn't sensitive to error between what he says and what he hears, you wouldn't be able to understand anything he says. He's come a remarkably long way in 21 months, and the only way to explain it is reorganization driven by error. He's converging on the language he's hearing, but the particular path itself is not completely predictable. I will make one prediction, however--that he will use the world "greenhouse" like everyone else around him before he's 12 years old--unless he's able to get everyone else to use "tosano" in the meantime!

There is some interesting research on the availability of negative feedback (called feedback in language acquisition circles) to children acquiring language. It used to be thought that parents provided no feedback to the chlid that what he or she said was ungrammatical. Recent research (in _Child Development_) has found that parents are much more likely to "recast" ungrammatical sentences in a grammatical form which is often repeated by the child.

Also, did you ever think that a greenhouse is usually neither green nor a house (what color is his)? This may be one reason why your grandchild is reluctant to use it--it causes internal error used that way. It may be easier to him to learn compound words when he doesn't know what the individual morphemes mean if the resulting compund word is not a clear semantic combination of the individual morphemes.--Gary

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Date: Mon, 18 Feb 91 11:57:35 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: gail5

The discussions I have had with Bill Powers about the case of Gail brings me back to the original issue. How does Control Theory say clinical hypotheses should be evaluated? I agree that the rating based procedure which I suggested has some fatal flaws. I hereby declare it dead! The problem remains of how to test out the Control Theory based guesses in a psychotherapy situation.

I can agree that one can be a lot more confident in a clinical hypothesis which is based on "in vivo" data. If I could observe Gail in the actual situations in which lumps develop, then I might have a much better idea of what is going on. If I could introduce disturbances and observe her reactions, then I would feel a lot more confident in what is going on.

This reminds me of times when my car needed repair. If the mechanic could experience the problem, the chances of the car being fixed the first time was pretty good. If he could produce the problem at will, then the car was well on its way to being fixed. If the mechanic had to rely solely on my verbal report of symptoms, the chances were not as good that the car would be repaired. Usually, I would have to bring the car back several times.

In individual psychotherapy, we have to rely on verbal reports most of the time. There are nonverbal behaviors which are less subject to conscious manipulation. Of course, the therapist is a person, so that the way the patient relates to the therapist might be informative of the way that the person relates to other people. In marital, group or family therapy we can observe the way that the different people relate in the session. While people are usually on good behavior in a therapy session, it does provide the therapist with some chances to make direct observations. These are the realities of the psychotherapy situation.

Let us guess that Gail's lump experiences are the way that she stops herself from saying something back to the person who has just criticized her out of fear that the person will stop caring for her. I devise therapy interventions, of which the affirmations are one component, which weakens or eliminates one side of the conflict. The therapy interventions plays the role of disturbance which is followed by reorganization in the person.

The expectation would be that Gail would start to speak up more after being criticized. She would stop having the lump experiences. Gail would report these things to me. I would conclude that my guess was probably correct.

If Gail did not report that she was speaking up more, then the therapy interventions did not result in the weakening or elimination of one side of the conflict. I would have to think of new therapy interventions to eliminate the fear that people will stop caring about her.
If Gail did not experience a reduction of the lump experiences after speaking up, then my guess was probably wrong. I would have to go on to the next guess of what function the lump experience was serving.

If Gail did not report back to me in a reliable way, then I would have no good way of evaluating the clinical hypothesis. With Gail's permission, I would talk to other people who share her life situations.

All of this does not seem very different from evaluating clinical hypotheses within more traditional psychotherapy approaches. All of this obviously takes time.

So what is new in the Control Theory approach? [This was the topic in the symposium that Dick Robertson organized at the last meeting.] Symptoms are looked upon as regulating perceptions.

Symptoms are not taken at face value. Unlike behavior modifiers, and like psychoanalysts, the CT therapist will not think that it is possible to increase or decrease symptoms directly. Unlike behavior modifiers and like psychoanalysts, the CT therapist would expect that some kind of symptom substitution would take place. [Several years ago, before behavioral approaches became accepted, there was a big controversy about the symptom substitution hypothesis. The behavior modifiers won. I plan to go back and look at this in more detail.]

In spite of what I just said, I think it is sometimes worthwhile to apply the behavior modification approach to see if it works to reduce symptoms and doesn't produce any new worse symptoms. In Gail's case I did not do this. What would I have her do? Snap a rubber band on her wrist after the lump symptoms developed to punish the lump response? Would I have her reward herself in some way every time the lump response did not occur in circumstances which have " a high probability of a lump response . "

The clinical hypotheses in CT are "simpler" than in psychoanalysis. Many of the guesses which I expressed were deemed too complex by Bill. " At my consulting rates I can't get so complex. Psychodynamics yet. " Even though the symptoms are not what they seem to be, the CT therapist curbs his imagination of what they could mean.

Symptoms are looked upon as regulating perceptions in present time. In this way, the CT therapist is more like behavior modifiers than the psychoanalysts. Looking into personal history is important in CT because it may provide clues for what perceptions are being regulated in the present. Knowing the circumstances under which Gail's lump symptoms started helps to come up with a better guess of what it might be regulating.

All this has to do with symptoms. Any psychotherapy which does not result in symptom reduction would not be considered successful. I am going to interrupt myself at this point. I fear that the message is becoming too long.

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Date: Mon, 18 Feb 91 14:35:23 -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: gail5

The last message from David (910218) ended,

>Symptoms are looked upon as regulating perceptions in present
>time. In this way, the CT therapist is more like behavior
>modifiers than the psychoanalysts. Looking into personal history
>is important in CT because it may provide clues for what
>perceptions are being regulated in the present. Knowing the
>circumstances under which Gail's lump symptoms started helps to
>come up with a better guess of what it might be regulating.
>
>All this has to do with symptoms. Any psychotherapy which does
>not result in symptom reduction would not be considered

>successful. I am going to interrupt myself at this point. I fear
>that the message is becoming too long.

I'm trying to catch up on all this after the weekend. I appreciate the opportunity to consider clinical issues since I think they have general applicability insofar as they offer ways of assessing what another is perceiving and controlling. I think these issues apply to education and learning as well. One of the "gail" messages defined conflict as when another's output is an obstacle to a desire. Most of us could probably remember a teacher who seemed to fit that bill.

On a related point, what about internal conflict? I am thinking about Gail's ulcer. Did I understand correctly that unresolved conflict will eventually directly result in skeleto-muscular symptoms, and only indirectly in higher order symptoms like ulcers. There was mention made about her feeling frustrated because of limitations imposed on her by her body. Are you expecting these to go away or become unimportant once the external conflict is resolved?

This brings up something I don't remembers seeing addressed on the net yet--physiological limitations of the organism. Maybe the topic is just too straightforward and self-explanatory in CT. But there seems to be a point where error is tolerated (or maybe not) simply because the system physically can't do anything about it any more. Even in my heyday, I couldn't do a 100 meter dash in less than about 15 sec. Closer to my interests, almost everyone gives in to the assertion that no matter how well you produce the syntax or how well you carry on a conversation in a L2, almost no adult learner will be able to speak another language without an accent. Among those who consider the problem seriously, most attribute this to the maturation of the speech tract in the first few years of life.

So are there any gross generalizations about the limitations of a human control systems hierarchy? (I am not looking for arguments for genetic, racial, or other segregation) How might we determine that it is no longer possible for the system(s) to further reduce error?

Joel Judd

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Date: Mon, 18 Feb 91 14:59:22 -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: Comments from Mary

Mary (910217),

>The Grandchild (21 months) is in high gear for language
>acquisition - uppy uppy to be picked up, juicies for a drink, and
>so forth. But why, when he can repeat almost any word in a fairly
>comprehensible way, is he adamant that a greenhouse is a tosano?
>Does he think he is saying greenhouse? It's a word he hears all
>the time, since that's the business his parents are in and the
>darned thing is right outside the back door and he is in it every
>day.

A quick thought: The first two communications are important for keeping intrinsic error at a minimum--tosano/greenhouse probably isn't.

Joel Judd

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Date: Tue, 19 Feb 91 10:44:52 -0600
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: paper draft

(CZIKO_Gary_A.:_U_Illinois_at_Urbana:_Bitnet:cziko@uiucvmd)g-cziko@uiuc.edu

>Gary
>I have a copy of the rough draft of a paper tentatively
>"The Hierarchical Behavior of Perception". It still needs work, of
>but I think I need a break and I also would like to get people'
>suggestions about possible reference materials, other related facts that
>might put in, and whatnot. I think the file is about 30 K bytes right
>Should I send it now?
>
>Best Regards
>
>Rick Marken

Rick:

Why not? I'll send it out to the CSG "hardcore." If anyone out there is not sure if he or she is considered hardcore and would like to interact with Rick on his paper, please send me a not to my personal email address.--Gary

Gary A. Cziko Telephone: (217) 333-4382
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of Educational Psychology Internet: g-cziko@uiuc.edu (1st choice)
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1310 S. 6th Street-Room 230
Champaign, Illinois 61820-6990
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Date: Wed, 20 Feb 91 08:13:22 -0800
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: Hierarchical Behavior/Perception Paper

To those who are willing to read and comment on my paper, here are a few quick thoughts I had while re-reading what I had posted. First, the paper is more a work in progress than a rough draft. It needs quite a bit more data and what is already there needs to be described in a bit more detail. I would appreciate any comments or suggestions about other data that might be relevant or how to clarify what I've already got. I will probably extent the section on the "control of sequence" experiment and present a graph of the data (once I get it) . That's the experiment where I have a subject press a key when the sequence changes. The key press restores the original sequence or not. If not, another press is required. The data would be the proportion of time that the "correct" sequence is maintained as a function of the speed of the sequence.

A second point that I should mention: I am thinking of this as a possible Psychological Review type article but any suggestions regarding an appropriate forum for such an article would also be appreciated.

All constructive (or destructive) comments are welcome. I see this as an experiment in using the net not only for peer review but for help with creating articles -- and the only place that I can find peers is here on CSGNet. Thanks in advance.

Rick M.

****

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

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Date: Wed, 20 Feb 91 11:38:36 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: Misc. comments
In-Reply-To: Message from "CSG-L@VMD.CSO.UIUC.EDU" of Feb 14, 91 at 11:35 am

Thanks for your reply to my comments, Bill. Sorry I took too long to reply in turn.

> I think we have to stay in close touch with our roots. We're not
> basically interested in control systems. What we're interested in are
> systems that are able to maintain variables outside them near internally-
> specified states, resisting disturbances with great effectiveness under a
> wide variety of conditions.

That sounds just like "Life" to me. That's certainly what the amoeba does, and I wouldn't even object to saying that that's what the amoeba is TRYING to do.

> When we say that organisms are control
> systems, we're not just classifying them; we're saying that they have
> exactly this critical capability, which few scientists have recognized
> but which is easy to demonstrate.

Yes, so we agree that being a control system is a property of all organisms, which means that being a control system is necessary for being an organism.

But what about the other way? Is it a sufficient condition? In other words, are there any control systems that are NOT living organisms (or, like the thermostat, embedded in living systems?)? Further, COULD THERE BE ANY natural control system which is not an organism? What would one look like?

> When I propose that we look at certain biochemical systems as control

> systems, I'm not just suggesting that we try on this point of view as a
> general way of talking. I'm proposing that we investigate biochemical
> systems to see if they have the required properties. Our control models
> derived from more general experiments show us the kinds of things we need
> to look for: input, comparison, output, and amplification (loop gain). I
> have found, in the literature, a few examples of closed-loop enzyme-
> catalyzed chemical systems that fit the bill. I'm willing to say that
> these are examples of control systems, because they have the properties
> that are needed. But this doesn't lead me to say that we now know that
> all biochemistry is organized around the principles of control. We have
> an existence theorem that now makes it worth while to investigate as many
> systems as possible, using the hints we get from those that have been
> identified. I have a hunch that we are going to find many control systems
> in a hierarchy of control. But I don't know that yet and wouldn't try to
> sell it. I only want to sell the idea that this is a fruitful way to
go.

I appreciate your modesty. Over-reaching is a danger of all visionaries.

But aren't I correct in saying that we know that all organisms are
control systems because living itself is a kind of control? Being alive
is a reference level that is maintained in the face of continuous
pertubations? This seems to be the case whether we have a good
biochemical THEORY of how that control acts or not.

And modesty aside, here, in this forum of friends, perhaps you can
speculate, or rather elaborate on your hunches, about how that might
play out.

> As to Gaia, no, I don't think that Gaia is a control system. I do think
> that all the organisms on earth are probably control systems, and that in
> acting to stabilize their local environments in states that they prefer,
> they end up stabilizing (to a lesser degree) their common environment.

As far I as I understand Gaia, it seems like a definition of control:
the "conditions that make life possible" are maintained within a small
margin at a "reference level" in the face of continuous pertubation from
solar flux, etc. Are you asserting that that's an equilibrium, and not a
control? I suppose that that's an empirical question anyway?

> Yet Lovelock is still generally right: the environment is
> stabilized in many regards by the massive resistance of uncountable
> organisms to disturbances of their own local environments.

So this is equilibirium maintained by an underyling "social" dynamic of
many individual control systems? vis. a vis. my earlier comments on
society being a control over people's thoughts, is that a model for
social systems in general?

> Also, Cliff, there really aren't many control theorists doing basic
> modeling. I think a revolution is under way, but it's horribly
> understaffed. We know of lots of fields where we'd like to see real work
> done, but we just can't do it all.

A shame, really. The biochemical stuff really seems ripe.

> (6) Yes, the origins of control systems are important. I've had some

> thoughts on that, which are now buried in the archives of CSGnet. I'll
> try to find them or maybe someone else who's better organized will. We
> don't have any particularly new ideas about this that other's haven't
> come across before.

I'd be interested anyway: it's the perrenial problem.

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: Wed, 20 Feb 91 11:20:35 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: <Parser> E: "From:"/"Sender:" field is missing.
From: Undetermined origin c/o Postmaster <POSTMASTER@UIUCVMD.BITNET>

(CZIKO_Gary_A.:_U_Illinois_at_Urbana:_Bitnet:cziko@uiucvmd)g-cziko@uiuc.edu
From: UPPOWER@bogecnve.bitnet (by way of (Gary A. Cziko) g-cziko@uiuc.edu)
Subject: Mittenthal on structure-function constraints

Gary: I'll leave it to you to relay this to Jay directly, or you can broadcast it to CSG-L in general if you think it will be understood. My interface to Bitnet allows one TO: field, and I have no instructions on how to do multiple sends, CCs, replies, and so on. The same will have to apply when I send comments on Marken's paper, probably later today.

Jay Mittenthal --

I have read and quite possibly (to an extent) understood the paper by Clarke and Mittenthal, "Modularity and reliability in the organization of organisms," which you so kindly sent to me. I believe that it may be the starting point for a bridge between control theory and your approach to biological subsystems. Whether you will agree may depend on whether you can show that blind variation and blind retention, which we could call "weak" (Darwinian) selection, is sufficient to account for the meeting of constraints, or whether we can show, together, that a stronger selection principle is required: blind but regulated variation and systematic retention through negative feedback control processes.

The concept of satisfying a constraint could be criticized as being teleological. As you use the term, there seems to be little difference between "constraint" and "consequence." For example, you say "At the highest level in the hierarchy there is one module, the entire organism. It meets the constraint that organisms must survive and reproduce if their lineage is to persist." Put this way, a constraint implies a condition that is set up beforehand, which the organisms must then somehow match. Simply inverting the sentence removes that implication: "If organisms survive and reproduce, their lineage persists." Now persistence of the lineage is not a constraint in the sense of affecting the processes in question; it is merely an outcome that may or may not occur. The statement becomes unequivocally true.

Each time you describe a constraint that is matched by the function of a module, the same inversion could be used. Doing so removes the causal

implications attached to the constraint, because then the constraint merely becomes the consequence of a certain behavior of a module. But this leaves unexplained the fact that certain functions of great value to the organism tend to appear preferentially, implying that the constraints met by their behavior happen to be those that are beneficial. To explain that "lucky coincidence," the traditional biologist would simply invoke Darwinian selection, saying that organisms containing functions that once matched different constraints or less "optimal" constraints are no longer with us.

I would be unconvinced by that explanation unless someone could demonstrate that for the process in question, Darwinian selection would suffice to bring it about in a finite number of generations. The fact that the near-optimal process DID arise is not proof that Darwinian selection would suffice (although biologists often employ that non-sequitur, in effect merely reasserting their faith in Darwinian selection). If it could be shown that there is a stronger selection process possible, the control-theoretic elaboration of Campbell's "blind variation and selective retention" principle, we would be at least one step further toward explaining cases in which weak selection would be unlikely to produce a detailed and complex answer to the problem of meeting a critical constraint.

In control theory, we have the same general kind of problem. A behavior is "purposive" if its outcome meets some predetermined criterion (very much like your "constraint"). That alone, however, is not actually a condition sufficient to prove purposiveness. What is needed is to show that the outcome is bought to the criterion state REGARDLESS OF UNPREDICTABLE DISTURBANCES THAT TEND TO ALTER THAT STATE BY ACTING DIRECTLY ON IT, and that it is brought to that state FROM A RANGE OF STARTING CONDITIONS. We must show, in other words, not just an effect of behavior, but control of that effect. Ideally, we should be able to demonstrate that all the processes required for control do exist and do work as needed. Practically, of course, we often have to settle for showing that such processes are likely to exist in the real system.

It seems to me that you have a similar problem with regard to the matching relation between structure and function. A constraint can be thought of as a boundary condition. In designing physical systems, the boundary conditions are generally given first; they define what the system is to accomplish. The mathematical problem can be stated, "given these boundary conditions, what is the equation of the system behaving over time?" In other words, "How does the system get from the initial conditions to the final conditions?" So the constraint or boundary condition is like a purpose: it is an outcome that is in some way predetermined. But boundary conditions can also work the other way: given the forms and the coefficients, what are the states of the variables at some initial time ti, and some final time tf? With the solution already known that becomes a trivial problem.

To show that your principle of matching is not simply an unfortunate inversion of a tautological statement of cause and effect, you must show that there is some reason to claim that a constraint precedes the operation of the module whose behavior meets it. This could be done by showing that when the constraint is not met, there is a consequence of the failure that alters the organization of modules that fail to meet it (over successive generations). One such consequence, as mentioned, is

weak or Darwinian selection, which "alters" modules by eliminating them. But another possibility is that the constraint has physical existence in the form of a reference signal for a control system, and that the action of the control system can modify the organization of the modules without destroying them. This is what I term "strong selection:" blind but regulated variation and systematic retention through negative feedback control processes.

Of course "strong" selection processes must arise out of "weak" ones. They represent the operation of a control system of a primitive but powerful type; Darwinian selection is the bridge between unorganized matter and matter organized to exercise this control process on itself. So the control-theoretic explanation of behavior that meets constraints or satisfies reference conditions is continuous with the traditional biological explanation; there is no conflict.

I suspect that the mechanism we control theorists call "reorganization," as it is embodied in models, could be represented as a Markov process. It is basically a statistical process, but biased by being embedded in a control loop. It has the same property of "stationarity" that you speak of in your paper, in that the final state is a continuing re-convergence on a definable state of the system. Perhaps your mathematician-colleague Clarke could look into that idea. I don't know enough about Markov processes to make a judgment (I barely know what the word means).

Best regards -- Bill Powers
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Wed, 20 Feb 91 13:27:29 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Replies to Joslyn; mouse problem

Cliff Joslyn (910220) --

>are there any control systems that are NOT living organisms (or,
>like the thermostat, embedded in living systems?)? Further, COULD THERE
>BE ANY natural control system which is not an organism? What would one
>look like?

I'd say that there are no naturally-occurring control systems other than organisms. Artificial ones have always been designed in the attempt to imitate control by living systems, and of course they're the products of living systems.

>I appreciate your modesty. Over-reaching is a danger of all visionaries.

I do agree that I probably do better at being modest than most other people. You should see what I delete.

>But aren't I correct in saying that we know that all organisms are
>control systems because living itself is a kind of control? Being alive
>is a reference level that is maintained in the face of continuous
>pertubations? This seems to be the case whether we have a good
>biochemical THEORY of how that control acts or not.

Rick Marken will approve: a theory of control doesn't make sense unless there's a phenomenon of control that needs explaining.

The question is, "What is the difference between living and non-living systems?" I think we can say that the universe is divided between controlling systems and non-controlling systems. But to be sure that is the difference between living and non-living systems, we'd have to have some independent way of defining living and nonliving to compare with our distinction between controlling and non-controlling, wouldn't we? I think that what control theory does is finally to give a meaning to what we've been trying to designate with the word "living." Of course there are other characteristics as well, such as reproduction or thermodynamic openness, but they're not as clear-cut -- crystals can reproduce, too, and the processes that keep stars shining are thermodynamically open, and so on. But I don't know of any system we would call nonliving (other than artifacts) that controls. It's rather surprising, considering that natural nuclear reactors, masers, and synchrotrons have been found. Maybe natural control systems would just require too many functions of specific kinds, hooked up to each other in just the right way, to occur by chance. Or maybe you need CNOH chemistry to get sufficient complexity.

I don't think that "being alive" is a reference level for organisms that don't think in symbols. For lower organisms, being alive is a consequence of controlling the variables essential for life, but isn't an explicit goal (i.e., a goal that has physical embodiment as a signal in a real system). I'm not sure that even symbol-using critters have any real goal in mind when they talk about staying alive. After all, if you can't say what "alive" means ...

>As far I as I understand Gaia, it seems like a definition of control:
>the "conditions that make life possible" are maintained within a small
>margin at a "reference level" in the face of continuous pertubation from
>solar flux, etc. Are you asserting that that's an equilibrium, and not a
>control? I suppose that that's an empirical question anyway?

It's the term "a reference level" that tells us that Gaia isn't a control system. Where would the reference signal be embodied? This is a lot like the problem of looking for "social control systems." In order to have a control system, a real one, you need all the components hooked up in an embodied system that does real things with signals and physical variables. I think there's a difference between having a lot of independent control systems all trying to achieve more or less similar goals, and having the same systems receiving their reference signals from a single specific superordinate system that coordinates them. There isn't any superordinate control system hovering over Gaia -- only the individuals are controlling. Same for social systems, which are created by interacting but independent individuals. Nobody sets the reference signals for the individuals, or even controls their actions. The mechanisms that would be needed just aren't there. Social mechanisms consists of interactions among individuals. That's probably how we should link of Gaia, too. "Interactions" can be as complex as you please, although I suspect that we will be able to derive some laws of interaction based on the peculiar ways control systems -- as opposed to atoms and molecules -- would interact. You know what "we" means.

Have you had a chance to look into that pendulum control system program yet?

Anybody --

David Goldstein is having a peculiar problem trying to get my Demo 1 to run on his Compaq 286 using a mouse. The program runs on his 286 at home using a mouse, but it doesn't read the Compaq mouse (a serial mouse in both cases). I'm baffaloed (buffaloed + baffled). Anybody else out there with a similar problem?
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Wed, 20 Feb 91 14:16:22 -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: Refs for studies of higher processes

This is a call for any published or readily available dissertations, theses, or other papers anyone knows of suggesting or actually carrying out some type of research dealing with high levels of the hierarchy (especially PROGRAM and above).

I am at that moment of truth in finalizing a dissertation proposal where it is necessary to outline an empirical study. Since I am applying CT to language, and particularly L2 acquisition, Gary Cziko and I have been trying to understand what the investigation of language acquisition from a CT viewpoint might look like. There seems to be two general possibilities: 1) to intervene with someone in the learning environment (teacher, administrator, student) along clinical lines--perhaps identifying a student who is not satisfied and determining what he wants and how he might resolve his frustration a la Ed Ford-type counseling; or, 2) try to apply The Test by hypothesizing a controlled variable and applying disturbances to see how the system reacts. In a learning situation, such as the English Institute here on campus, there doesn't seem to be a real ethical way to apply 2); disturbances like providing false test scores or unnerving teachers through student behaviors don't seem appropriate. 1) seems more appropriate when dealing with human subjects at high levels of the hierarchy, but counseling techniques of course depend on use of language. A study less holistic and more concerned with levels of the hierarchy might investigate particular linguistic aspects (pragmatics, phonetics, etc.), and how they are controlled in the L1 but not the L2.

The difficulty is that "research" along CT lines is conceptually simple, but when applied to something like "language learning" the simplicity is a little deceiving. Part of the problem is that I've been thinking about this too much, and part of the problem is that the CT perspective does away with so much of what used to make social-science research (seem like) a straightforward proposition. So before I put my foot further into my mouth...any references for investigating higher level behavior?

Joel Judd

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Date: Wed, 20 Feb 91 14:20:03 -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: Refs for studies of higher processes

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The difficulty is that "research" along CT lines is conceptually simple, but when applied to something like "language learning" the simplicity is a little deceiving. Part of the problem is that I've been thinking about this too much, and part of the problem is that the CT perspective does away with so much of what used to make social-science research (seem like) a straightforward proposition. So before I put my foot further into my mouth...any references for investigating higher level behavior?

Joel Judd

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Date: Wed, 20 Feb 91 14:26:39 -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: apologies

Excuse me, I think my last message was sent twice.

Joel Judd

==========

Date: Wed, 20 Feb 91 18:42:02 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: Gail6

I just received Bill Powers (910217).

While the people I talk to have had insights during the therapy discussion, they have not had the "... drop in stress and a

striking change in affect for the better" which Bill says goes with the going-up-a-level approach.

The psychoanalytic approach emphasizes the patient achieving insight. The going-up-a-level approach seems very similar except the kinds of insights which the patient is to achieve are described differently. The psychoanalytic approach lost some of its dominance among psychotherapists because (a) insight is not enough for many problems, (b) not everyone is capable of the kind of self-observation to achieve the insight, (c) the symptom substitution hypothesis was not supported by research, and (d) miscellaneous reasons.

I detect an inconsistency in the CT approach. When it comes to helping a person deal with a conflict, the therapist plays a passive role. The therapist job is to help a person focus on the control systems which are in conflict. However, when it comes to evaluating whether a perception is being controlled or not, the therapist takes an active role in which disturbances are introduced, imposed on the innocent, unaware patient.

Bill assumes that the magic light of awareness will result in one side, or both sides of the conflict changing by itself. Why should it? A person can understand that they are in conflict for the first time. What principle in CT says that two control systems in conflict will start to reorganize as soon as a person becomes aware of the conflict? And why is awareness necessary for this to occur?

When two children (two control systems) want to play with the same toy (perceptual variable), there are several ways that this can be resolved. One child can give up the goal of playing with the same toy. Usually, the physically weaker child would do this. The two children can time share the same toy. The two children can agree to put the toy away and each choose different toys. The two children can find a way to play with the toy together.

A parent (therapist) in the next room hears the children arguing and fighting about something (the symptom). The parent goes to where the children are located. The parent points out to the children that they are fighting over a toy . Do the children say "Aha, so that's what we're doing." Does the parent then observe the children solve the conflict all by themselves?

Being more serious, the parent could find out from each child why they wanted to play with that toy at that time. They could ask the child how s/he could get what s/he wanted in a different way. One or both children may decide to choose a different way of getting what s/he wants. Why? As soon as an alternative pathway towards a goal opens, which is not blocked, the child prefers that pathway. I guess I answered one of my own question. Never mind Bill. But why is awareness necessary for this to occur?

Back to the case of Gail. The conflict is: Gail wants to be treated fairly and justly by people; she doesn't want to be abused. Gail also wants to be cared for by people. Gail wants to

and doesn't want to answer back when people sound angry with her. By discussing both sides of this conflict with Gail, she may find alternative means to the goals and the lump symptoms will not develop.

I don't see what is anti-CT to advocate one side of the conflict if one of the two sides is clearly wrong, mistaken in some way. Not all of our wants are rational. How did our wants get there in the first place? Did we, by means of our own will select them? Or, through socialization did we adopt some of them without much thought? If we take one side, or make a specific suggestion to take some action, and this creates error signals in the person, the person will let us know or will fail to accept our suggestion. People can be persuaded or influenced through verbal means. Why is it anti-CT to do so?

In surveys of therapists who were asked to describe the qualities they would seek in other therapists, it is interesting that they want therapists who are active not passive in therapy. They don't want a therapist, who like the classic psychoanalysts, just sits there and listens. They want a real person. Other mortals want the same thing from their therapists.

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Date: Wed, 20 Feb 91 23:03:03 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: symptoms;L2

Joel Judd (910219)

I was trying to point out some alternative possible interpretations for a physical symptom which is not the result of a physical disease process. Namely, medical factors are ruled out.

One possibility is that the symptom has a specific functional meaning. The lump experience, for example, could be the result of an approach/avoidance conflict for talking back after being criticized which results in skeletal muscle tension in the throat area. Gail wants to but doesn't want to talk back. You can't talk and not talk at the same time. The conflicting instructions to the speech producing equipment create excessive muscle tension. A second example, you can't flex and extend the same arm at the same time.

Bill Powers was saying that the CT interpretation of a symptom was of this variety. He came to this conclusion from the idea that the symptom was Gail's unsuccessful effort at producing some specific outcome. The reason her efforts were unsuccessful was the presence of a conflict. This interpretation of physical symptoms is the one taken by classical psychoanalysts.

If a symptom has a specific functional meaning, then the use of CT Therapy makes sense, or any other kind of psychotherapy, to discover the function being achieved by the symptom.

A second possibility is that a symptom has a more general meaning with no particular function. The symptom could mean that Gail is experiencing some stress. Which body system is most sensitive to stress will vary from person to person and within the same person, may vary from time to time. At one time in the past, Gail's stomach was the site in which the stress showed up first. Now, the most sensitive body system seems to be the throat area most of the time [although her stomach is still a problem area if she doesn't watch her intake of food and drink very carefully].

I can see that the idea of reorganization may result in symptoms of the nonspecific functionality kind. When in a state of reorganization, one sees random activity. It would be a mistake to read any specific functional meaning into the randomly generated results of the reorganization system other then recognizing that the person is reorganizing. Maybe, Gail is easily thrown into a state of reorganization and the first sign of this are the lump symptoms.

If a symptom is a sign of reorganization, then what is the most helpful therapeutic approach from the viewpoint of CT? (1) Reassuring the patient that they are reorganizing which is a good thing even if the symptoms are unpleasant. This is to help the person avoid becoming disturbed by the symptoms., (2) Reducing the error signals within a person by addressing life problem areas., (3) Encouraging the person to take an open, experimental attitude towards trying new things., (4) teaching a person some self-calming techniques which reduces overall body arousal., (5) Encouraging the person to go on with his/her life in spite of the symptoms.

I have probably tried all these approaches with Gail. Numbers 2, 3 and 5 have been of some help. They don't satisfy Gail. She wants the symptoms removed. This makes me think that the interpretation of Gail's symptoms as having a specific, functional meaning may be correct.

Of all the things that I have tried with Gail, the ones which have been most successful have been: (a) encouraged Gail to take a nurturing attitude towards herself, (b) allowed Gail to express her frustrations at the symptoms, at the lack of control over them, at me and other therapists for not helping her without becoming rejecting or angry. I am one of the only males in her life who she trusts. At first she felt very uncomfortable with me.

Joel Judd (910220)

I have done some reading research in young children. This has been described as "language by eye" and may related to L2 acquisition.

One idea for you to consider is that the emotional reactions of people while they are learning a language may be an indicator of error signals they are experiencing. If you videotaped a person while they are having a language lesson, and you examined the videotapes of a session, you may be able to spot the points at

which they showed some strong emotions.

If you then showed the person these points in the videotape and asked them to recall as much as possible what was going on at these points, you may be able to learn what they were trying to control but having some difficulty doing.

I have done a research study like this when children were being giving there first reading lessons. I would be glad to send it to you.

Just let me know if you are interested.

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Date: Thu, 21 Feb 91 02:07:03 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: Replies to Joslyn; mouse problem
In-Reply-To: Message from "CSG-L@VMD.CSO.UIUC.EDU" of Feb 20, 91 at 1:27 pm

Bill Powers (910220)

> I'd say that there are no naturally-occurring control systems other than
> organisms. Artificial ones have always been designed in the attempt to
> imitate control by living systems, and of course they're the products of
> living systems.

Right. This doesn't means that control systems = living systems. The question is: let's say you came across a natural control system that at first glance wasn't an organism. Would you be forced to say it was, and thus include it in the class of organisms? What about viruses, which, like a disembodied thermostat, control only when included in part of a larger living system?

> I think we can say that the universe is divided between
> controlling systems and non-controlling systems. But to be sure that is
> the difference between living and non-living systems, we'd have to have
> some independent way of defining living and nonliving to compare with our
> distinction between controlling and non-controlling, wouldn't we?

This is the problem: there cannot be an empirical test for being a living system (although Marken claims there is for being a control system). What we're doing in this little game is going about the business of coming up with a DEFINITION of life. Perhaps being a control system is a good definition; perhaps not. But the decision is OURS what we call life; it is not nature's. The stuff is unchanged. It's epistemic, not ontological.

But I agree with you that control is a very good candidate. As you suggest, the other properties, thermodynamic openness, circular chemical networks, and all the consequences for "metabolic activity" of the Prigogine school and its successors, could follow from that.

> Have you had a chance to look into that pendulum control system program
> yet?

Regret not. I should say that I'm not really into fuzzy control, but

had the code around and passed it on as a public service. My uses of fuzzy are rather different: about the kinds of uncertainties that arise in natural complex systems, like many-body physical systems and natural networks. I'm also not sure that that was intended as a prime example of a fuzzy controller, either, but rather just to show you how it's done. I certainly appreciate your argument as described, and as am little interested in a fuzzified SR controller as in a crisp SR controller.

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: Thu, 21 Feb 91 10:02:26 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Hardware, thesis, levels & conflict

Fred Davidson (910220) --

Thanks, Fred, I think you're right. David got a similar answer from a local guru. He will try starting the Compaq without the mouse driver in AUTOEXEC and then install his own mouse program from home where the mouse works. If that doesn't work --- ???

Gary Cziko (910220) --

It's a color problem. I wrote the original version for a black and white system with 2 colors: color 0 and color 1. Color 1 in an EGA or VGA system is dark blue, not white. In later versions I substituted the compiler variable WHITE which adjusts properly for the type of graphic system, but evidently you don't have that version. I'll send a replacement set. In the mail tomorrow.

Joel Judd (910220) --
>trying to understand what the investigation of language acquisition
>from a CT viewpoint might look like. There seems to be two general
>possibilities: 1) to intervene with someone in the learning
>environment (teacher, administrator, student) along clinical lines--
>perhaps identifying a student who is not satisfied and determining what
>he wants and how he might resolve his frustration a la Ed Ford-type
>counseling; or, 2) try to apply The Test by hypothesizing a controlled
>variable and applying disturbances to see how the system reacts.

What does language enable you to do that you can't do without it?

How would you use language to persuade somebody that you're right? Would it make any difference if you couldn't see the other person's face?

If you're talking to someone and that someone's attention strays, what do you say? What if the other expresses disbelief? Changes the subject? Doesn't understand? Misunderstands? Deliberately misinterprets? Objects to your attitude? Doesn't want to listen?

What will it be like to have a conversation between Earth and Mars when

the time lag is several minutes? (Sort of like having a conversation on this network as opposed to directly). Do people talk differently?

One place to look is at Newell, Simon, and Shaw's work on the General Problem Solver. They did extensive work with real people who were trying to prove theorems and such, recording running reports on what the people were trying to do and why. Then they developed programs that worked in similar ways with goals and subgoals. You might warm up for your thesis by interpreting their protocols in CT terms. Their approach was non-deceptive and non-intrusive.

Dick Robertson and L. A. Glines published a work on "the phantom plateau" -- Dick, will you supply the correct references? This involved a complex learning task in which the subject had to learn, at one stage, which of 4 keys (V,B,N, and M) to hit to turn off one of 4 lights that appeared on the screen (in a fixed sequence). I discovered accidentally that learning the required sequence of moves (even for a completely new set of relationships) could be done in one complete trial just by paying attention to the symbols on the keys and saying them to myself. I would try each key until the first light went off. If the "V" turned the light off, I said "V" to myself. The next light came on when the previous one was turned off. Then I tried the remaining keys until the next light went off. If the effective key was "M", I said to myself "VM". After all four lights had been turned off, I had the complete sequence, for instance "VMNB." At that point I knew that the key sequence was VMNB, and the sequence was learned after the first pass. The task could also be learned without this silent naming of the letters in sequence, but the learning took much longer (dozens of trials). Dick's subjects evidently did not use my method, because some of them took 50 trials or more and some never learned the sequence. When I didn't use it, I also took many trials to learn the sequence. I don't know what this tells us about the role of symbolization in the learning of complex motor control tasks, but maybe you can figure it out.

Robertson and Goldstein also did a study on self-image. Perhaps Dick R. can send a copy directly to your bitnet address. Dick/David, I'm thinking of the report in which you go through all the fancy methods you tried first, before you describe the simple one that nearly always worked (25 of 26 cases, wasn't it?). There's something to be learned from this study about how to explore the higher levels of control: Keep It Simple.

Another approach would be to set up some learning task and try to lay it out to the student as a problem in learning perceptions rather than actions. Instead of teaching someone to DO something, try teaching how to PERCEIVE the right aspects of the task, the ones that need to be brought to specific reference levels if the effort is to succeed. You'll see some hints about that in the Newell et. al. work. I suppose you could do a statistical study comparing the learning of some material that way and some in more standard ways (with different people). Your Committee would appreciate seeing something familiar.

Could you describe some language-learning tasks for us? Maybe they will suggest some more approaches to CSGnetters.

David Goldstein (910220) --

>While the people I talk to have had insights during the therapy

>discussion, they have not had the "... drop in stress and a
>striking change in affect for the better" which Bill says goes
>with the going-up-a-level approach.

>The psychoanalytic approach emphasizes the patient achieving
>insight. The going-up-a-level approach seems very similar except
> the kinds of insights which the patient is to achieve are
>described differently. The psychoanalytic approach lost some of
>its dominance among psychotherapists because (a) insight is not
>enough for many problems, (b) not everyone is capable of the kind
>of self-observation to achieve the insight, (c) the symptom
>substitution hypothesis was not supported by research, and (d)
>miscellaneous reasons.

I've known lots of people in psychoanalysis (some in their 6th or 7th year) and they had all sorts of insights into their behavior but seemed just as messed up as ever. I came to think that what they call an "insight" I would call an "explanation."

I'm not sure what you mean by "insight" here. It may not be what I mean by going up a level. Can you give some examples of a patient going up a level (or having an insight)? I don't count it as insight when a therapist explains to a patient what is really going on, and the patient says, "Yes, I see, that makes sense."

The method of levels involves getting the person to notice and describe background attitudes, thoughts, feelings THAT ARE ABOUT WHAT IS BEING DISCUSSED. There isn't any attempt to solve a particular problem or steer attention toward any particular subject-matter, nor is there any interpretation of what the person is saying. It's an iterative present-time process, so that as the person notices and then begins to describe the background attitude, etc., the therapist listens for still another level and when appropriate asks the person again to notice and describe background attitudes, thoughts, feelings, and so on -- now about what was previously the background. The process doesn't stop until no more levels easily appear.

If you can keep the method of levels going for 20 minutes, I think you'll see that change of affect. It doesn't work with everyone every time but in my informal tests it has almost always had interesting results. It's hardest to use with someone who is highly verbal and doesn't believe there is anything else going on. You have to notice when the person is turning it into an "I'll bet you can't guess" game, and obviously doesn't want you to know what secret thoughts are going on. You tell such people "If you want to try this, you should do it with someone you trust." And of course you can't pounce on things the person says just because they suddenly reveal the truth to you and you can't wait to show how smart you are. All you care about is going up more levels. If the person says it isn't working and nothing is happening, don't get defensive -- just ask what the patient thinks about that fact. The route to the next level is unpredictable.

>I detect an inconsistency in the CT approach. When it comes to
>helping a person deal with a conflict, the therapist plays a
>passive role. The therapist job is to help a person focus on the
>control systems which are in conflict. However, when it comes to
>evaluating whether a perception is being controlled or not, the

>therapist takes an active role in which disturbances are
>introduced, imposed on the innocent, unaware patient.

Why evaluate whether a perception is being controlled, in therapy? Suppose you found out. Would you tell the person "Here's what I think you're controlling for?" There are dozens of perceptions being controlled all the time -- why pick one for discussion rather than another? It seems to me that CT therapy is an attempt to enable the client to do these things, not the therapist. The therapist's insights don't help the client make progress, they just help the therapist feel wise.

Anyway, the therapist is not passive in the method of levels. The therapist is an implacable force acting in the direction of going up a level.

>What principle in CT says that two control
>systems in conflict will start to reorganize as soon as a person
>becomes aware of the conflict? And why is awareness necessary for
>this to occur?

In Basic Control Theory, there isn't any reason. The reason comes from Elaborated Control Theory, which is a figment of my imagination. The proposition is that awareness is the means by which reorganization is directed to the right place instead of occurring in random places -- at least when brain functions are reorganizing (this obviously wouldn't apply to the immune system. I THINK it's obvious...).

>Do the children say
>"Aha, so that's what we're doing." Does the parent then observe
>the children solve the conflict all by themselves?

You can't get a child to take the viewpoint of a level that doesn't exist yet. If the children don't understand sequence, for example, you can't get them to take turns. So you say Peter, do you want the toy? (Peter says yes). You say Mary, do you want the toy? (Mary says yes.) You say, You can't both have it, can you? All right, here's how we're going to do it. Peter, you can have the toy to play with now. Then Mary can have it to play with. Tomorrow, Mary can have it first. Mary, let's find you a different toy to play with. Peter will give this one to you when he's through. OK, Peter? (Sure). Come on, Mary.

In other words, you substitute your own level for the one they don't have. Arbitrarily. There are lots of possible solutions. You could try David's idea, which would probably work, too. Lots of things will work. You just have to be aware of what you're teaching. As Mary (Powers) said, I wish I'd thought of all this 30 years ago.

>I don't see what is anti-CT to advocate one side of the conflict
>if one of the two sides is clearly wrong, mistaken in some way.

Conflicts aren't "won." They're dissolved. BOTH sides of the conflict are right, for different higher-order reasons. What matters is what the client knows, not what you know. If you advocate one side of the conflict, you will be causing error in all the higher-level systems related to the other side. Those systems will fight back. Reference levels, no matter where they come from, aren't effective until they are voluntarily accepted, translated into reference-perceptions, and used.

Objective correctness or rationality don't come into it. Both sides of a conflict make sense to a person in some way or in some context and were established, individually, on purpose. Usually, though, they won't both make sense when they're experienced simultaneously. People tend to identify, at a given time, with one side of the conflict, treating the other as some "urge" or "habit" outside them -- some kind of character defect. When they reach the right viewpoint they can see themselves as responsible for both sides. That's when something happens.

>In surveys of therapists who were asked to describe the qualities
>they would seek in other therapists, it is interesting that they
>want therapists who are active not passive in therapy.

Therapists, like everyone else, want to be in control. Some of them want to have the power to cure people, like a doctor. Many of them dream of saying JUST THE RIGHT THING so the patient's jaw drops, the patient's eyes bug out, and the patient cries, "Oh, thank you, Doctor, that's exactly what's wrong, I understand everything now! You're so smart!" If a therapist doesn't provide insight, diagnose problems, give people good advice, administer treatments, and cure the patient, what's the point in being a therapist? That's what it's like to want to be in control of the client.

Did anyone ask the patients what they want?

I don't think that the therapists in that survey (the ones who agreed with the outcome, that is -- how many didn't?) would be able to stick with the method of levels very long. It isn't passive, or "just listening", but it doesn't afford an opportunity to solve the client's problems for the client. In fact, it leaves the client largely in control and the therapist may be the last to understand what's happening. In the method of levels, a perfectly acceptable response is "I don't want to tell you." (You say, "What are you thinking about not wanting to tell me something?", not "Aw, come on, you can trust me.").

From the CT viewpoint, the goal IS for the client to be in control, isn't it?

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

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Date: Thu, 21 Feb 91 11:27:14 -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: symptoms;L2

David (910220),

Thank you for the responses and explanations. They are very helpful and give me a clearer picture of clinical applications and interpretations. The comment in your other post about people's desire for active therapists makes sense, especially when considering learning. Gary Cziko has shared a couple of Ed Ford's examples and I like what he said about his role being one not only of making others aware of their functioning but providing "plan" suggestions for continuing action. This seems to go along with your unwillingness to simply accept the fact that simple awareness of conflict will automatically trigger solutions. I see the same role for a teacher in

language (or any other kind of) learning: one who can bring the student to an awareness of why he is having problems, AND provide ways to possibly solve the problem and continue attempting to reach his goals. This, of course, is not the typical stereotype of a teacher.

>One idea for you to consider is that the emotional reactions of
>people while they are learning a language may be an indicator of
>error signals they are experiencing. If you videotaped a person
>while they are having a language lesson, and you examined the
>videotapes of a session, you may be able to spot the points at
>which they showed some strong emotions.
>
>If you then showed the person these points in the videotape and
>asked them to recall as much as possible what was going on at
>these points, you may be able to learn what they were trying to
>control but having some difficulty doing.

This is one of the possibilities Gary and I had discussed. I had thought about it in regards to taping a teacher and comparing classroom behavior with the teacher's verbal claims about classroom goals. I would be interested in knowing what you did. If you have any written protocols for the proceedures you followed, those might be less troublesome than sending a tape--whatever is more convenient for you. Thanks again.

Joel Judd

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Date: Thu, 21 Feb 91 11:27:22 -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: Refs for studies of higher processes

Charles (910220) and Clark (910221),

>One of the most plausible descriptions of L1 learning that I have read is
>found in Jerome Bruner's book, Child's Talk. If you are a student of
>language acquisition and have not read that, I would urge you to do so.
>Perhaps you can conceive of a way of recasting what is reported there into
>hierarchical learning problems and therefrom take a step toward the
>solution of your own dissertation problem.

Sounds like 2 out of 3 control theorists agree: Bruner and Plooij have important perspectives on language! Unfortunately I have come up through L2 programs at a time when most people studying L2 feel that it is different enough from L1 to be sort of a world unto itself, and so they are concerned mostly with L2 literature and I would say most ESL/FL teachers don't get a grounding in general acquisition literature. If anything, they read Brown's 70s work with acquisition and Leopold's case histories of his children. One thing CT seems to be saying is that we're all in this together, and L2 learning is inseperably connected to L1 learning, and L1 learning is a part of learning in general. Talk about the Big Picture. Thanks for the references.

Joel Judd

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Date: Thu, 21 Feb 91 12:46:40 -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: Hardware, thesis, levels & conflict

Bill Powers (910221),

I probably should have prefaced the first research alternative with some explanation about "language." The "clinical" alternative for research would be using language as a medium to find out about problems (what is the learner controlling for/what is his goal) in language learning. This method is subject to all of the objections you raised:

>If you're talking to someone and that someone's attention strays, what do
>you say? What if the other expresses disbelief? Changes the subject?
>Doesn't understand? Misunderstands? Deliberately misinterprets? Objects
>to your attitude? Doesn't want to listen?
>
>What does language enable you to do that you can't do without it?

Encode experience to communicate (it) to others.

>How would you use language to persuade somebody that you're right?

I think teaching has often become this; I want to argue that learning is NOT obtaining a knowledge of what is "right". The language teacher is not "right", but (hopefully) knows "right" ways of doing things which he can help the student develop in a safe environment.

>Do people talk differently?

In a strict sense, what everyone SAYS is different since everyone's experience is different, though we use the same words/expressions.

>Another approach would be to set up some learning task and try to lay it
>out to the student as a problem in learning perceptions rather than
>actions. Instead of teaching someone to DO something, try teaching how to
>PERCEIVE the right aspects of the task, the ones that need to be brought
>to specific reference levels if the effort is to succeed.

This sounds a little like "Silent Way" approaches which allow the learner as much time as necessary to silently "perceive" (different use of the word) the L2 before he actually says anything. This idea comes from the competence/performance distinction raised by Chomsky and others in language generally. Many people can listen to or read in another language, but can't say anything, or very little. A question of interest then is "What are these 'passive' learners perceiving?" Your comment seems to suggest that if one can be brought to a key perception (the [th] sound or adjective order) then both comprehension and production should be facilitated, is that right?

>Could you describe some language-learning tasks for us? Maybe they will
>suggest some more approaches to CSGnetters.

See if this is what you had in mind:
Verbs are important in English. They tell us about what's happening and the time frame. They also are placed in a certain relationship to subjects and complements. From a CT perspective, learning verbs and their usage involves

al kinds of levels in the hierarchy. Some of the more salient are 1)naming the "action" involved (think, eat, drive, own), 2)conjugation 3)object requirements 4) modals/helping verbs 5)"verbals" ('SWIMMING pool'), etc. There are many aspects to this. Where do you start? Give L2 equivalents to L1 verbs (dictionary)? Memorize lists? Name pictures? Perform the actions yourself? What is error for the learner? What is done about it? Etc., etc.

>Why evaluate whether a perception is being controlled, in therapy?
>Suppose you found out. Would you tell the person "Here's what I think
>you're controlling for?" There are dozens of perceptions being controlled
>all the time -- why pick one for discussion rather than another? It seems
>to me that CT therapy is an attempt to enable the client to do these
>things, not the therapist. The therapist's insights don't help the client
>make progress, they just help the therapist feel wise.

Substitute "language learning" for "therapy" in the above. It seems that in learning there would be a reason for the teacher selecting certain perceptions at a given time. The trick is knowing which ones, and when?

>In Basic Control Theory, there isn't any reason. The reason comes from
>Elaborated Control Theory, which is a figment of my imagination. The
>proposition is that awareness is the means by which reorganization is
>directed to the right place instead of occurring in random places -- at
>least when brain functions are reorganizing

Seems to be a function of the teacher.

>If a
>therapist doesn't provide insight, diagnose problems, give people good
>advice, administer treatments, and cure the patient, what's the point in
>being a therapist? That's what it's like to want to be in control of the
>client.

Seems to be a temptation for a teacher.

Joel Judd

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Date: Fri, 22 Feb 91 08:35:45 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: L1, L2, etc.

Joel Judd (910221)

I guess I should also have explained myself at more length (or at all). Sometimes I leave too much of what I mean inside my head and it comes out rather obscure. I asked:

>If you're talking to someone and that someone's attention strays,
>what do you say? What if the other expresses disbelief? Changes the
>subject? Doesn't understand? Misunderstands? Deliberately misinter-
>prets? Objects to your attitude? Doesn't want to listen?

These questions weren't "objections" but sketches of research manipulations that could be used to demonstrate and explore control that

is carried out verbally. If you're talking to someone and that person's attention strays, do you interrupt yourself and wait, or speak differently in some way to get the attention back? If someone expresses disbelief while you're talking, do you edit what you're saying to make it more forceful, add corroborating evidence you hadn't originally planned on, all while continuing to talk? If someone changes the subject do you try to change it back? If someone doesn't understand, how do you modify your verbal behavior to deal with that? And so on.

All of this is predicated on the assumption that there are higher level systems concerned with transmission of meanings of many different kinds, systems that use and modify the control of sentence construction, word choice, and so on as a means of perceiving some EFFECT of the words on another person. The only way in which such systems can control for transmission of meaning is to perceive the effects that successful transmission would have. And the visible signs of such effects require hypotheses about how another person would visibly or verbally behave if the meaning were understood as intended. I think we have many ways not only to control for those effects, but to cross-check the hypotheses: we don't settle for just one bit of evidence. We even test the other person to see if understanding really occurred -- sometimes by asking questions, sometimes just by watching what they do: i.e., if you understood my directions, you're going to turn left at the next corner.

It seems to me that language is uniquely concerned with producing effects on people -- getting them to show agreement, understanding, approval, affection, cooperation, gratitude, fear, and so on. We don't make words just to make them; we make them in order to have effects on our own perceptions of their recipients. The only thing that words can affect (linguistically) in the environment is a living system that understands words, even if the words are only "Heel, Rover."

I don't know what this has to do with L2 and L1, but if I get the drift, it would imply that L2 uses L1 as a means of controlling perceptions appropriate to L2. Or maybe there are systems that use L2 as well. Maybe you'd better explain L2 and L1 in more detail to those of us with limited resources.

At any rate, my suggestions have to do with ways in which language systems are used as subsystems to help accomplish higher-level purposes. I should think that there would be research possibilities there, maybe even pertinent ones. My previous post didn't result in the kind of reply I desired to perceive, so I'm modifying my output to see if I can't get that there little old error a little smaller.

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

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Date: Fri, 22 Feb 91 09:26:56 -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: L1, L2, etc.

Bill Powers (910221),

>These questions weren't "objections" but sketches of research
>manipulations that could be used to demonstrate and explore control that

>is carried out verbally.

I think I took those questions the way you meant them--I was just understanding them as problems with using language as a means to determining controlled variables; they are also aspects of using language, period.

>I don't know what this has to do with L2 and L1, but if I get the drift,
>it would imply that L2 uses L1 as a means of controlling perceptions
>appropriate to L2. Or maybe there are systems that use L2 as well. Maybe
>you'd better explain L2 and L1 in more detail to those of us with limited
>resources.

I think this is the stage where your beliefs about L1/L2 differences need to rely on a theory of behavior. If you believe that a new language is just slapping new symbols onto existing perceptions, then your teaching methods, or your research, or your L2 abilities will reflect that belief. What most people have "discovered" in the last twenty years or so is that to be able to "communicate in" as opposed to just "speak" another language one is somehow learning new perceptions of life and the world. As one researcher put it, I can speak like a native speaker but still have a strong non-verbal "accent." I think that is a result of failing to control for higher level perceptions in an L2 milieu. As a language teacher put it, learning a L2 means that when I'm in Spain and I hit my thumb with a hammer and say 'ouch' in Spanish, I mean what a Spanish speaker means when he says 'ouch'.

Joel Judd

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Date: Fri, 22 Feb 91 08:20:12 -0800
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: Hierarchy Paper

Dear Dr. Meyer

Sorry about posting this to CSGnet but I was unable to send it directly to your bitnet address.

Thanks for your interest in my paper. I don't have a copy on my mainframe at the moment. The paper is being distributed via email by the CSGnet manager, Gary Cziko at U of Illinois Urbana. I don't know his email address offhand but you can request a copy from him by posting your request to CSGnet. If you have a problem let me know and I will be happy to send a copy to you myself. The file is not that big -- about 35KB. As I said in an earlier post to the net, the paper should be considered a work in progress -- I am posting it in order to get suggestions regarding the topics that are most interesting, which should be expanded, clarified or dropped. So I would be most grateful if you would read the paper and let me know what you think.

Thanks again

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: Sat, 23 Feb 91 13:00:09 -0600
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: teaching control theory

After a number of weeks and a lot of missed messages, I am now once again able to receive and send mail. Just in time to make some comments about my attempt to teach CT to undergraduate Ed Psych students (two classes of 16). I'm not sure what should be my measure of success, here, but if a basic understanding of the model is a sign of success, I think we were successful. However, teaching to 35 students who have only had a chapter's worth and one or two lectures of behaviorism and/or cognitive psychology is not the same as teaching those ingrained in the S-R mentality. The task was difficult because it was so easy. The students said, "Well, of course" to CT. I guess that's good but I wanted them to understand that psychologists have not and are not saying "of course." This was puzzling, especially when I told them that CT has been around for quite a while. A lot of the students said "Operant conditioning seems the same as CT; you get reinforced by reaching your goal." I said "Exactly!" but you aren't allowed to say "goal" in operant conditioning. This puzzled them more. The idea that the environment would have a simple causal effect on behavior just didn't make sense to them. Seeing the envirionment as a disturbance did.

So there's hope for the college students who haven't been engrained in other models but I still don't know the best way to teach students new at learning models. It's also difficult since I am supposed to be finding applications for the models we teach, but applications for CT aren't as straight-forward as previous models. It's a necessary evil, as you all know, but I'd appreciate any suggestions for applications of control theory in the high school classroom, specifically related to classroom management. Maybe I need to find a new word for "management." (It would have been nice to have read Power's Skinner's Mistake article in the Newsletter BEFORE I taught last Thursday--Bill states it clearer than I did).

By the way, did my thought on "temptation as the choice with higher loop gain" get through a few days or weeks ago? Did anyone respond to it?

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Date: Sun, 24 Feb 91 15:26:36 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: <Parser> E: "From:"/"Sender:" field is missing.
From: Undetermined origin c/o Postmaster <POSTMASTER@UIUCVMD.BITNET>

(CZIKO_Gary_A.:_U_Illinois_at_Urbana:_Bitnet:cziko@uiucvmd)g-cziko@uiuc.edu
From: (Gary A. Cziko) g-cziko@uiuc.edu
Subject: Especially for Newcomers

Newcomers and Oldcamers:

Since we have had more than just a few new subscribers over the past few weeks, I thought I should once again send out some basic information about what this network is all about.

So here is the introduction to control theory that Bill Powers put together last fall. While control theory goes well beyond what Bill presents here, his introduction does give the new subscriber some idea of the "Weltanschauung" shared by the vast majority of people on CSGnet (CSG-L).

Let me remind you that Bill saw this as a first draft and so anyone who wishes to suggest changes should communicate with Bill or perhaps share his or her ideas with the network.

My apologies to those who have seen this introduction before. If there were an easy way for me to send this to only new subscribers, I would do it.--Gary Cziko

P.S. Anyone wanting to see a more "hardball" introduction to control theory should let me know and I will send him or her Bill's "Manifesto."
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INTRODUCTION TO CONTROL THEORY
William T. Powers
November 1990

Control theory, as we use the term, means engineering control theory adapted for use as a model of the behavior of living systems. Those who already understand engineering control theory therefore already know part of the story. The rest of the story lies in the way we organize a model of control to explain organismic behavior. Sensors, comparators, and effectors appear in this model just as in ordinary models of nonliving control systems. Where we understand enough of real behavior, the models are set up much like models that others use and for the same purpose: to analyze behavior through simulations. But there are some critical differences.

In a living control system, the reference input is not accessible from outside the system. Engineering diagrams commonly show the reference signal as an input from the outside world, which it is in artificial systems: it's the means by which the human user tells the control system the level at which to keep its controlled variable. In a living control system, the "user" is the whole organism. Reference signals are set by higher systems that are also control systems (the higher systems act by adjusting reference signals for lower systems). In some cases the reference signals are derived from genetically-specified information (for example, the reference signal for body temperature). In the majority of the control systems that exist in the brain, however, the organization is learned within a general matrix of preorganization, and reference signals derive from the operation of a multi-leveled, "massively parallel" system.

One of the basic insights behind our uses of control theory is that all control systems control their own inputs, not their outputs. In engineering, this fact is obscured because the inputs are arranged so as to represent an external variable of interest to the user of the system, generally a variable directly affected by the actions of the system:

position, temperature, acceleration, pressure, and so on. But a little thought will show that such variables can be known to the system only as signals generated by sensors; in every case it is the signal, not the external variable, that is under control (just picture what happens when the sensor drifts out of calibration). Our model must be understood from the viewpoint of the system itself, not that of an external user.

The human system knows the external world through millions of sensors. It affects the external world, and thus its inner world of sensory signals, by its actions. The sensory signals also play a part in the production of action: we propose, specifically, that it is the same role played by the sensory signals in control systems. This leads to a new understanding of behavior, in which action and perception are part of a closed control loop, the action serving to maintain the perception at whatever level is currently specified by an inner reference signal. External disturbances tending to alter the signals, the perceptions, result in actions that oppose those effects, thus leading to the spurious appearance that the

system senses the disturbances and simple reacts to them.

This picture is very different from a stimulus-response model, and it is also very different from a cognitive or command-driven model. One level in the model does not tell a lower level what act to perform: it provides an example (in the form of a signal) of the state to which the lower system is to bring its own sensory signal. The lower system itself provides the action needed to match perception to the reference. A sensory signal entering a control system does not cause any particular action to occur; the action is based not on the perception but on the DIFFERENCE between the perception's state and state currently being specified by the reference signal.

This model is very tightly interconnected. A perceptual signal in a given control system is derived from the perceptual signals in a set of lower-level systems. The derived signal is of a new type; it is a function of the set of lower perceptual signals. This higher-level perception is compared with a reference signal, and the difference is converted to a set of output signals. These output signals enter THE SAME SYSTEMS FROM WHICH THE LOWER-LEVEL PERCEPTIONS CAME, serving as reference signals that specify the states of the lower-level perceptions. All loops are closed: all behavior at all levels is purposive. Every effect generated by any system is controlled in terms of the perception that represents it: nothing organized ever happens open-loop.

The evidence in support of this model ranges from excellent at the lowest levels to sketchy at the highest. Where we know how to do experiments, we construct quantitative working models and match them to behavior by adjusting their parameters. We're trying to expand the scope of these experiments to higher levels, but the going is slow. One factor that encourages us is that all control loops, in this model, can be detected and tested from outside the system, because all loops are closed, ultimately, through the environment. Where the model is wrong we can find out that it is wrong.

The model is also approached in another way, as an organizing principle for reinterpreting phenomena of behavior. Given the basic organization of control as we see it at the lower levels, the question is whether higher levels of organized behavior also make sense in these same terms -- more

sense than when interpreted in conventional ways. So far the answer seems to be a unanimous yes. We are trying, however, to extend the method of modeling so it can be useful in areas where quantitative experiments are difficult. In this way we hope to test and buttress the insights of our clinician-members and real-life investigators by linking their work to that of our computer modelers. Both contingents will learn from this interaction. But all have a long way to go. There are more than enough research problems awaiting us at all levels of analysis.

While our uses of control theory have many roots in the past and many resemblances to the work of others, our approach is basically not connected to any mainstream line of development. It is a new departure, almost a reconstruction of behavioral theory from scratch. Some of us are convinced that it amounts to a revolution in the life sciences.

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
uppower@bogecnve.bitnet

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Date: Sun, 24 Feb 91 18:10:35 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Second Language

Joel Judd (910222) --

>What most people have "discovered" in the last twenty years or so is
>that to be able to "communicate in" as opposed to just "speak" another
>language one is somehow learning new perceptions of life and the world.

This is a strong hint about how we can study differences between L1 and L2 (I finally get the idea, stupid me, that L1 is the first language and L2 is the second, rather than being two levels of the same language). If I read you right, you're saying that speakers in different languages are attaching words to differently-organized experiences. I've heard this said before (like the French are always saying, that some French words are just too subtle to translate into English, which I never paid much attention to beyond automatically interpreting it as snobbery). Of course now that you put it in this context, it makes sense. It isn't that the WORDS "have" esoteric meanings. It's that perceptions are organized differently.

This, of course, is extremely interesting to a CT theoretician. It's a chance to see whether the definitions of levels I proposed still hold up (i.e., does the speaker still perceive in the same classes?) while also seeing new ways that perceptions can be organized WITHIN the classes. I would be just as interested in finding that a foreign-language speaker has a different hierarchy, of course, but even if that isn't the case, I would be most interested in seeing some of these new examples of perceptions that are derived from different combinations of lower-level perceptions.

This is much more general than a "linguistic" consideration. The perceptions come before the words. Do you think it would be possible to find examples of such perceptions, and show that controlling them requires doing different things with lower-level perceptions than is needed for the nearest English-speaker equivalent?

So how about some examples for us to try taking apart and analyzing in this way? Got any standard ones, or your own list? Maybe you could find some "cognates" that actually have different meanings, and we could go from there.

Rick Marken --

I keep pushing your paper down in the stack. It's a beautiful conception, constructed like a musical composition. Maybe I'll come up with some suggestions, but it's developing so well that I think I'll wait for the next draft. Others will do better with references, as you know.

Best -- Bill Powers
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Mon, 25 Feb 91 08:24:59 -0800
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: Misc Comments

Gary Cziko -- Thanks for posting Bill's summary of CT psychology. How many subscribe to this network now?

Bill Powers and other recipients of my paper: I really would appreciate comments by the end of this week. I'm holding off on the next iteration 'till then. Thanks for the nice comment about the paper Bill. I hope you all feel that way. I just want to see what anyone might have to say about it before I start revising -- ergo, the deadline.

I have not had a chance to do much work lately but I am still interested in designing a system to control a variable defined over time. I have made some stabs at it but have not been able to successfully implement the control system. I have some ideas about how to do this and if I get a chance I'll test them this week and report on my results. Remember, my goal is to show that the kinds of things that seem to demand feedforward (predictive) control can be handled by feedback control of a perceptual signal that represents a time-varying aspect of the environment.

The L1, L2 distinction is starting to get very interesting. Sounds like stuff I used to study years ago when I was in college; about how different languages code different aspects of experience (the Whorf hypothesis). Is this part of what might be going on with L1 vs L2?

Best regards

Rick M.

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Date: Mon, 25 Feb 91 14:03:12 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: <Parser> E: "From:"/"Sender:" field is missing.
From: Undetermined origin c/o Postmaster <POSTMASTER@UIUCVMD.BITNET>

(CZIKO_Gary_A.:_U_Illinois_at_Urbana:_Bitnet:cziko@uiucvmd)g-cziko@uiuc.edu
From: mmt@dretor.dciem.dnd.ca (by way of (Gary A. Cziko) g-cziko@uiuc.edu)
Subject: Control Systems Group

Anyone and Everyone:

Anyone care to respond to M. Martin Taylor (see below)?

We have had a bunch of new subscribers due to a message posted by Cliff Joslyn on CYBSYS-L. Taylor seems to be one of these. We have about 55 subscribers now. A full list can be had by sending the message "review csg-l" without quotes to "listserve@uiucvmd.bitnet" (also without quotes).--Gary
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I note your description, reproduced in the Systems and Cybernetics mailing list. It sounds interesting, and I would appreciate being added to your list as "mmt@ben.dciem.dnd.ca".

You say:

>The basic concept accepted by members of the Control Systems Group is that
>all organized behavior continuously controls the portion of perceptual
>experience which can be influenced by the actions of organisms. This is
not
>an article of faith. It follows from a detailed quantitative analysis of
>behavior, showing that action affects the very perceptions on which action
>is based.

I wonder if you are aware of the book "The Behavioral Basis of Perception" by James G. Taylor (no relation) Yale University Press, 1962. In it, he gives a detailed mathematical and psychological analysis and synthesis of exactly that proposition. In my career as a psychologist, it has been one of three books that have most influenced my thinking (the others being Garner's "Uncertainty and Structure as Psychological Constructs," and Watanabe's "Knowing and Guessing."). Anyone who is interested at all in the cited ?basic concept" should try to get hold of this book.
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Date: Mon, 25 Feb 91 14:40:05 -0600
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: Need examples...

To anyone who can help:

I am having difficulty hooking up the inputs, outputs and reference levels in a hierarchy of control systems. I'd like to specify a set of parameters for the interconnections in a control hierarchy, and then let a genetic algorithm "evolve" optimal configurations. In other words, the genetic

algorithm would be doing the reorganization, the aspect which most interests me as a student of machine learning. However, in trying to find these parameters, I've run into some difficulty understanding exactly how higher level systems control lower level systems. E.g., exactly how are lower level perceptions passed up to higher level inputs? Similarly, how are higher level outputs multiplexed to lower level reference signals? It seems necessary to include some extra processing, such as weighted sums, multiplexing, and other switching functions, rather than just hooking up inputs, outputs and reference signals. But I don't want the network to "degenerate" to a neural network. Neural net design with genetic algorithms is already being studied. I'd like to stress the advantages of a control system architecture over a neural net, but my understanding of this advantage is general and intuitive. I'd like to see a concrete example of a HIERARCHY (not just a single control loop, I've seen lots of those) instantiating some interesting behavior in a concise way. Can anyone help?

Thanks to Gary Cziko who suggested I post this, after providing me some useful suggestions and insights, and that after exposing me to CT and CSGlist in the first place.

-Jeffrey Horn, Graduate Student in AI at University of Illinois
(jeffhorn@uiuc.edu or jhorn@ux1.cso.uiuc.edu)

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Date: Mon, 25 Feb 91 14:44:53 -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: Second Language

Bill (910224) and Rick (910225),

> If
>I read you right, you're saying that speakers in different languages are
>attaching words to differently-organized experiences. I've heard this
>said before (like the French are always saying, that some French words
>are just too subtle to translate into English, which I never paid much
>attention to beyond automatically interpreting it as snobbery). Of course
>now that you put it in this context, it makes sense. It isn't that the
>WORDS "have" esoteric meanings. It's that perceptions are organized
>differently.

I think one of the reasons this idea has just been coming back is that descriptive linguistics held sway for so long, and when you are too concerned with the words, it's easy to lose track of their context. Unfortunately, Rick brought up something which I was going to mention, but then in perhaps Freudian fashion I managed to forget, and that's the Sapir-Whorf hypothesis. In a crude sense this is the linguistic version of the chicken/egg dilemma--does environment fashion language form or does language affect how we experience the environment. Someone more familiar with the hypothesis might take issue with that characterization. I think Gary Cziko has spent more time dealing with particulars of this hypothesis, and arguments for/against it.

Given that CT gives a much more detailed picture of what the "environment" is, and also previous discussions about learning as a process of BVSR (Gary okayed the acronym), I don't think the S-W hypothesis is couched in very good terms. It will be important to recast the experience/language

relationship in CT terms.

>This, of course, is extremely interesting to a CT theoretician. It's a
>chance to see whether the definitions of levels I proposed still hold up
>(i.e., does the speaker still perceive in the same classes?) while also
>seeing new ways that perceptions can be organized WITHIN the classes. I
>would be just as interested in finding that a foreign-language speaker
>has a different hierarchy, of course, but even if that isn't the case, I
>would be most interested in seeing some of these new examples of
>perceptions that are derived from different combinations of lower-level
>perceptions.

Another reason I think the hierarchy will hold up under linguistic considerations is that the levels are described in such a way that the linguistic aspects of any well-known language will fit (without a doubt for the first several levels--those seem constrained by human physiology). But it's that difference in perception that holds the key.

>This is much more general than a "linguistic" consideration. The
>perceptions come before the words.

And then comes language. And then, after a maturation of the hierarchy (or before?), there seems to come times when language is incapable of expressing our perceptions. What is 'loyalty', or 'family', really? Here is where we find poets and artists trying to communicate something, somehow. There is a professor here, almost on the net, whose interests center on culture and anthropology, among other things. I think she'll be able to provide some cross-cultural food for thought. Regarding artists, some work has been done trying to determine how they determine what they want in a work, how they do it, how they know when it is 'done.' The interest in getting someone to articulate the process relates to the idea of "expert systems" and how someone knows how to do something [well]. What researchers find is that the artists can't articulate (or have great difficulty articulating) what it is they are doing, what they 'want' (what they are controlling for). But this is getting far afield! I go to look for some language examples...

Joel Judd

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Date: Mon, 25 Feb 91 13:06:42 -0800
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: Hierarchy connections, Taylor

Jeffrey Horn -- Apparently you want to do what I wanted to do -- reorganize a hierarchy to develop new control systems. It sounds like a great idea. If you want to know how to make the initial structural connections in the hierarchy I suggest my own paper R. Marken (1990) Spreadsheet analysis of a hierarchical control system model of behavior. Behavior Research Methods, Instruments & Computers, 22, 349 - 359. Also see Powers (1979) The nature of robots: Part 3 A closer look at human behavior, Byte, August, 94-116. These references will give you the equations for the basic hierarchical structure of the control model. The neural net reorganization process can then work on the parameters of control (gain and slowing) or the nature of the perceptual functions at each level. Let me know how things progress with your work; it is very interesting to me.

Re: The Taylor book on "Behavioral Basis of Perception". I remember seeing that book when I was in graduate school. I looked it over but don't recall what it said. I do remember that the title made a big impression -- I was a student of perception at the time and the only thing that seemed close to what Taylor might have been talking about was the motor theory of speech perception. I ran into the Taylor book at about the same time I ran into Powers' book. I didn't understand either one at the time but my impression at the time was that Powers' book was a lot more important. But maybe they were saying the same thing. I no longer have easy access to either the Taylor book or the time to read it. Perhaps Martin Taylor could give a brief overview of Taylor's point. Taylor seems to have at least one thing in common with Powers -- he didn't take psychology by storm. I take this as evidence that, like Powers, he was saying something very important. I now understand what Powers was saying -- what was Taylor saying? Thanks

Regards

Rick M.

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Date: Tue, 26 Feb 91 14:44:42 +0100
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Jean-Arcady MEYER <MEYER@FRULM63.BITNET>
Subject: CSG-L

Hello folks!

I have just subscribed to this list few days ago and I have no idea of its history and of the past discussions which it might have generated.

However, I read with great interest the general presentation of the list by Gary Cziko, the introduction to control theory by William Powers, as well as the paper about hierarchical behavior of perception by Richard Marken.

I am an ethologist and a computer scientist involved in the simulation of animal behavior and, together with Stewart Wilson - from The Rowland Institute for Science - I organized last year the conference SAB90 (Simulation of adaptive behavior: from animals to animats). As it is said in the preface of the Proceedings' book - which has just been published by The MIT Press/Bradford Books - "the main objective of this conference was to bring together researchers in ethology, ecology, cybernetics, artificial intelligence, robotics and related fields (Whoops!) so as to further our understanding of the behaviors and underlying mechanisms that allow animals and, potentially, robots to adapt and survive in uncertain environments".

This is just to say how interested I am *a priori* in an explanation of organismic behavior which is said to be "very different from a stimulus-response model, and [ ] is also very different from a cognitive or command-driven model".

I must confess that I haven't read Bill Power's books and that I ordered them in my favorite scientific library this morning. But, reciprocally, I am surprised to haven't seen any mention in (what I know of) the CSG litterature of works which seem highly relevant to perception control by action (Agre, Chapman, Brooks, Whitehead) or to perceptual and behavioral hierarchies (Tinbergen, Simon, Albus, Dawkins, Baerends), these works emanating from ethologists, roboticists or computer scientists. [Incidentally, a technical report reviewing these approaches is available and quoted below].

Naturally, these approaches may be perfectly well known but judged irrelevant by the CSG community. In that case, I would very much like to know the corresponding reasons.

I also fully subscribe to Jeffrey Horn's interests in the use of a genetic algorithm for evolving a control hierarchy. The CSG community seems more interested in preprogrammed behaviors than in those which are learned or evolved. Is that true?

Thank you in advance for any criticism, comment, reply or suggestion.

Jean-Arcady Meyer
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The following technical report is available:

From animals to animats: everything you wanted to know about the simulation of adaptive behavior.

Tech. Rep. BioInfo-90-1

Jean-Arcady Meyer
Agnes Guillot

Abstract: Following a general presentation of the numerous means whereby animats - i.e simulated animals or autonomous robots - are enabled to display adaptive behaviors, various works making use of such means are discussed.
This review is organized into three parts dealing respectively with preprogrammed adaptive behaviors, with learned adaptive behaviors and with the evolution of these behaviors.
A closing section addresses directions in which it would be desirable to see future research oriented, so as to provide something other than proofs of principle or ad hoc solutions to specific problems, however interesting such proofs or solutions may be in their own right.

For a hardcopy of the above paper, please send a request for Tech. Rep. BioInfo-90-1 to: meyer@frulm63.bitnet

Be sure to include your regular address.
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Date: Tue, 26 Feb 91 08:36:23 -0800
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: CSG & Other Behavior Theorists

Jean-Arcady Meyer (910226) says:

>I must confess that I haven't read Bill Power's books and that I ordered them
>in my favorite scientific library this morning. But, reciprocally, I am
>surprised to haven't seen any mention in (what I know of) the CSG litterature
>of works which seem highly relevant to perception control by action (Agre,
>Chapman, Brooks, Whitehead) or to perceptual and behavioral hierarchies
>(Tinbergen, Simon, Albus, Dawkins, Baerends), these works emanating from
>ethologists, roboticists or computer scientists. [Incidentally, a technical
>report reviewing these approaches is available and quoted below].

>Naturally, these approaches may be perfectly well known but judged
>irrelevant by the CSG community. In that case, I would very much like
>to know the corresponding reasons.

I am familiar with the work of Simon and Albus. I read Dawkins' Blind Watchmaker and I know a little about Tinbergen's ethology. I also know a little about Brooks' work. I don't think the work of these folks is irrelevant. They have often made observations that are very interesting and useful. But their theoretical work is quite a distance from a control theory point of view.

I have been working on control theory for over a decade now. I used to think that it was important to show how control theory differs from other approaches to understanding living systems. I now think this is hopeless. I now just present evidence for my point of view and try to ignore the other ones unless they provide alternative WORKING models of the phenomena I study. I have submitted many papers describing experiments showing how a working control model explains behavior that cannot be explained by alternative models. In most cases these papers are rejected by reviewers who simply say that the other model can do what I say it can't; and that the control model cannot do what I have shown that it does do. The reason for this problem is that many of the most popular models are simply verbalisms and descriptive equations -- not working models. So it comes down to me saying "no, it can't" and them saying "yes, it can". Of course, they win because their model is the one that at least half the psychological community belleives. The other half believes another model that is also not control theory. So my policy is to ignore current work unless they 1) claim to demonstrate a phenomenon that control theory cannot explain or 2) they provide a working model of a phenomenon that they say is not a control model.

My policy of ignoring much of the current literature may seem cavalier but I think you will notice that the current literature is also pretty good at ignoring me. I have published quite a few papers that challenge some of the fundemental assumptions made by the "famous" theoreticians that you mention -- and I have never seen anyone try to test my work or challenge my conclusions. That's OK with me but if they want me (and other control

theorists) to refer to deal with them then they will have to with me (us). I have written articles dealing specifically with claims made by other theoreticians-- none have even troubled themselves to answer my work, let alone try the experiments that might show me wrong.

The reason why control theory is ignored is easy to understand -- it isn't at all like the models people are familiar with in psychology or other life sciences. The idea that behavior is the control of perception may be an amuising slogan to these folks but to control theorists it is the basic organizing principle of life. It is nothing like motor programs, chaos, complexity theory, AI, neural nets, fuzzy logic or whatever. Some of these things may have stuff to off control modellers -- but the basic idea of control of perception and all that that implies about how organisms work and how you go about trying to understand how they work -- is what sets control theorists apart. It also, of course, alienates us from much of main stream life science because once you understand how living control systems are organized it means you go about your business in whole new ways -- ways that are basically alien to current practices in many life sciences (psychology in particular). Control theory renders the independent-dependent variable approach to psychological research irrelevant (though it explains why relationships between these variables are found).

I have to get to a meeting. There is much more to say about this. But let me just quickly say that I (we?) have nothing against the theorists you mention. We are always looking for new, useful ideas. But what we usually find is the same old misconceptions dressed in fancy new clothing.

I would like to get a copy of your paper:
Tech. Rep. BioInfo-90-1
Please send it to my my USMail address listed below

Thanks.

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, 26 Feb 91 13:04:55 -0800
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 Theory Approach

Oded Maler (910226) says

>Being a computer scientist , and thus "neutral" with respect to your war with
>psychologists etc., I would like to get the flavour of "your" approach by

>considering the following specific problem:

>Consider an artificial worm, built of segments, each containing a spring and
>some simple sensors (internal tension, external forces, etc.). What would be
>your approach toward the development of a control mechanism that will produce
>crawling in various classes of environments? This is of course an open-ended
>question and I don't expect a "solution", but maybe thru this examples I
>could understnad the unique principles of the approach you advicate.

Great question. I hope I get some help on this from other CSGers but let me try a first stab. First, let me say that I am not at war with psychologists, though sometimes I suppose it seems so. I just think they are wrong and I don't see a compromise position. But I'm happy for them to live, propogate both themselves and their ideas and be happy. No war. It just happens that I'm right and they are wrong; they are free to remain wrong as long as they like.

Now, to the artificial worm. First, I think most CSGers are more interested in understanding the workings of existing systems rather than building artificial ones. Our models are artificial systems, however, to the extent that they produce the same behavior as a living system. So I'll assume that I am building the crawling worm as a model of a real crawling worm.

You pose the problem as though my goal were to produce a worm that produces a particular kind of behavior-- crawling. Assuming that you can give a quantitative description of the behavior you want to see -- ie- segments changing position relative to the floor, perhaps some changing of angles between segments over time, etc) then there are probably many ways to produce mechanisms that will produce these outputs. This is where the control approach differs from conventional approaches right off the bat. To a control theorist, BEHAVIOR is INTENDED PERCEPTION. So the first thing we would want to know is "what perceptions are controlled by the worm when we see the behavior called crawling"? What we are asking is "what variables are being controlled by a crawling worm". The methods for determining controlled variables have been discussed in many places --there are many approaches. But, basically, you start with a guess: "the worm is controlling the amount of heat on its belly" (I don't know much about worms but some experiments when I was young convinced me that worms can sense heat). There are many things that the worm might control. This is one hypothesis. It is tested by applying disturbances (like heating and cooling the surface under the worm) and measure the presumed controlled variable (with a heat sensor on the worm's belly). The variable is controlled if it stays relatively constant despite environmental variations that produce disturbance. You will probably notice that the worm controls the temperature and that one way it does so is by crawling towards a hotter or cooler part of the surface, depending on what is necessary to keep its belly the temperatur it wants it.

The means by which belly temperature is controlled is not an output (or behavior). Crawling itself must be a set of controlled perceptions -- of tension in the muscles (your springs) that create the crunches and relaxations that move the body. This is where Bill Powers could help out; what you need is a person who knows enough about the physics of worm locomotion that they can guess at the variables that must be controlled (PERCEIVED) in order to produce the result you see as "crawling".

Again, let me make clear that the control approach does not try to simulate behavior (as you think of it -- which is from the point of view of the

observer). There are many ways to do that and produce what looks like a fairly convining imitation of what you (the oberver) sees. Control theorists try to imitate behavior FROM THE POINT OF VIEW OF THE BEHAVING SYSTEM; we try to design a system that controls the perceptual variables that the organism controls. This means that a simple sensor will probably not do; what you need is an imitatation system that can perceive (and control) what the actual organsism can perceive and control. Thus, heat sensors, position sensors, etc are not enough. Eventualy you will need sensors for 3D orientation and even more complex variables that are derived from lower level perceptions. Once you design such a system, you will also have solved the problem of behavior in "any environment". In the non-control approach (which simulates the production of output) the system will work only in the environments you have planned for -- and these are usually quite limited. This is because these systems are controlling (producing) particular outputs that will not produce the desired results if things change slightly; for example, a crawling worm that is designed to crawl by producing a particular force in response to a particular sensor input might have a hell of a time when moved from a concrete floor to wet hardwood. An "intelligent" system might be able to solve this problem by recognizing the difference in floors and adjusting the lower level force output "appropriately". But this approach assumes that you can always sense the disturbance; suppose that an invisible, low friction coating is poured on the concrete. This is no problem for a control system, which just controls the perceptual result of the force. Disturbances that cannot be sensed are not rare- indeed, they are the most common (like road friction changes when you drive). A control system doesn't have this problem with this because it is controlling the perceptual results of outputs (which include the effects of disturbance), not the outputs themselves. So the model worm would control, say, the sensed angle between two segments. It would produce the force needed to produce this angle, which will be much greater on concrete than the hardwood.

So, the conventional simulator takes behavior for granted and assumes that what is to be simulated is what he sees. The control theorist sees behavior as the means by which the system keeps perceptual inputs in reference states. The conventional simulator tries to figure out mechanisms that will produce the behavior seen. The control theorist starts by trying to find out what variables are being controlled and then figures out how to connect up outputs to discrepencies between perception and reference so that the perception is controlled.

Hope this helps.

Rick M.

****

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Date: Tue, 26 Feb 91 15:18:00 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: News of meeting; David G. & Gail; Jean-Arcady; Rhythms

CSG MEETING, 1991: Those of you who are members of the Control System Group (CSG) should have received the most recent newsletter, which contained news about the CSG meeting. For those who are not members and who want to receive a copy of the newsletter, or to join CSG, write to:

CSG Newsletter
10209 N. 56th Street
Scottsdale, AZ 85253
USA

Ed. Ford publishes the newsletter and will send a copy to any interested nonmember. To join CSG, include a check for $25 (US) or for $5, if you are a student.

The meeting will be 14-18 August 1991, at Ft. Lewis College, Durango, Colorado. The cost for members is $135 (students, $135) which covers lodging for 4 nights and meals for 4 days. Further, the Director of Conferences and Institutes at the college has arranged for interested members to arrive as early as the weekend before our meeting and use the college dormitory and cafeteria, for quite a modest fee. Details about the meeting and the excellent opportunity for a pre-meeting vacation in scenic southwestern Colorado are in the newsletter.

DAVID GOLDSTEIN: It is good to see you stirring things up on CSG-L! Some of the other clinicians should log on, to help maintain a balance between theory and practice, in our postings.

JEAN-ARCADY MEYER: (910226) Please send me a copy of your Tech Rep. BioInfo-90-1. Rick Marken already said many things I would say in reply to your question about how control theory is related to other behavioral theories. Perhaps the major difference is that control theorists demand that their own model be a working model, a generative model that produces data similar to those produced by organisms that actively control variables in their environments. Apparently, very few behavioral scientists place similar demands on their models. Consequently, "they" do not see the relevance of our reliance on modeling as a method to test our model, or to critique their theories (I hesitate to refer to most behavioral theories as models, because so few of those theories embody a working model.)

Because we (control theorists) encounter frequent rebuffs from advocates of more widely held theories, we sometimes lapse into citing a small core of theorists, rather than the broader range of behavioral theories. That did not come out as I intended. Rather than saying, "Because we encounter frequent rebuffs," I meant, "Because so few advocates of major positions seem interested in what we do, and because much of what they do can be done with words, rather than a functional model... . I look forward to seeing your report.

BILL POWERS, RICK MARKEN, ANY OTHER MODELERS: I will send another post, in a few minutes, on some recent publications that deal with control behavior.

Tom Bourbon <TBourbon@SFAustin.BitNet>

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Date: Tue, 26 Feb 91 21:36:49 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Misc comments

MM Taylor --

Welcome to CSGnet. I hope you will follow through some of the references that our group can provide, so you can make up your own mind about their relevance to other lines of thought. If you need further explanations or clarifications, please ask -- you will probably be buried under helpful comments.

Jeff Horne (910225) --

Rick Marken gave you the same references I would have given.

>I'd like to specify a set of parameters for the interconnections in a
>control hierarchy, and then let a genetic algorithm "evolve" optimal
>configurations. In other words, the genetic algorithm would be doing
>the reorganization, the aspect which most interests me as a student
>of machine learning.

>However, in trying to find these parameters, I've run into some
>difficulty understanding exactly how higher level systems control lower
>level systems. E.g., exactly how are lower level perceptions passed up
>to higher level inputs? Similarly, how are higher level outputs
>multiplexed to lower level reference signals? It seems necessary to
>include some extra processing, such as weighted sums, multiplexing, and
>other switching functions, rather than just hooking up inputs, outputs
>and reference signals.

In the Byte article that Marken mentioned you'll see that I agree with your analysis (except for the multiplexing, which isn't necessary). In that article you will find a general algorithm for constructing hierarchies with a guarantee of negative feedback for every control system. What I do by design, you can probably do with a reorganizing algorithm, if you provide the same basic organization as a starting point. One of the critical considerations in building a self-organizing system, it seems to me, is the decision as to how much pre-organization to allow and what kind of preorganization to use. This is like deciding how much to leave to evolution.

In the kind of hierarchy I visualize, modeled in the Byte article, there are really three interleaved hierarchies working at the same time. A given level-1 control system receives reference signals (both excitatory and inhibitory) from all three level-2 systems at once. The net reference signal is the algebraic sum. Also, the perceptual signal in any one level-1 control system contributes to the perceptual signals in all three level-2 systems. The perceptual signal in each level-2 system is the weighted sum of all three level-1 perceptual signals. The output of each level-2 system's output function contributes to all three level-1

reference signals. The same output signal branches into multiple copies; the only difference at the destinations is whether the copy is connected directly to the lower-order comparator or is connected in the inhibitory sense (multiplied by -1). This expresses the fact that reference signals in the nervous system (at lower levels, anyway) either terminate directly on comparators in the excitatory sense, or go through an interneuron, a Renshaw cell, that converts the excitation to an inhibitory effect. I did not use any weightings on the output side other than 1 and -1.

The result is three systems that control X-force, Y-force, and total force (muscle tone) independently of each other, using three control systems that control the tensions in three arbitrarily-oriented muscles. You can adjust the reference signal for any level-2 system, and it will make its perceptual signal match, regardless of the reference settings for the other two systems (except that there must be more than some minimum muscle tone to prevent muscle tensions from going negative). Each level-2 system senses a weighted sum of the three level-1 perceptual signals, weighted to project them onto an X and a Y axis, or in the third system, weighted equally. This was a force-control system, but it could easily be converted to a position-control system.

Knowing what I know now, I would make all three level-2 output functions into simple time-integrators, and the three level-1 output functions into leaky integrators. That's like saying that components of this type are available for building control systems, per the inherited design. I think you can also take comparators as given components. You can take the weighted summation type of perceptual function as a given, too, with only the weights available to be adjusted by reorganization. That says that you're modeling a particular pair of levels in the nervous system: sensation and configuration, I would guess.

As I see the process of reorganization, it's necessary for the reorganizing system to monitor some variable that depends on the parameters being randomly adjusted. An example would be monitoring the absolute average error signal in a level-2 system, and adjusting the sign of the output connection to each level-1 comparator in turn, changing the sign to see if the error gets larger, and if it does, changing it back. This would let the system accomplish for itself what I do using an algorithm: make all the feedback loops negative.

I don't quite see how to apply reorganization to the level-2 input functions (one level at a time is enough, don't you agree?). The problem is that EVERY set of weights defines SOME controlled variable and gives meaning to the related reference signal. We need some consequence of choosing a particular set of weightings such that when the weightings are "wrong," the variable we use to measure the consequence departs from an optimum value such as zero. Given that variable and a reference level for it, we can convert the error into random small changes in the weights, spacing the changes farther apart if the consequence-variable-error gets smaller. Actually to do that right we would have to change the weights by some small amount delta which can be positive or negative, and then do a random choice of positive and negative. That way if the weightings are improving performance, we postpone the next random change of direction for some number of iterations and let a few more changes of the same sign take place, and if the error gets worse we can cause the next change sooner -- just like E. coli. It seems to me that you'd have to have reorganization going on in all three systems at once in order to converge

on any unique set of weightings, again because EVERY set of weightings has SOME meaning.

So what kind of "consequence" to use? One kind would be arbitrary -- after all, you're in complete control of this organism's environment. You can decide that controlling in the X direction is good for the organism in one way, and controlling in Y is good for it in another way, and simply make the consequence variables depart from zero to the extent that each criterion fails to be met. Or you might find some totally internal criterion such as total absolute error signal in the system that could be minimized by reorganization.

Probably time to pause for you to read the article and come back with objections, questions, etc..

Joel Judd (910225) --

>And then comes language. And then, after a maturation of the hierarchy (or before?), there seems to come times when language is incapable of >expressing our perceptions. What is 'loyalty', or 'family', really? Here is where we find poets and artists trying to communicate something, >somehow.

In my proposed levels, language as symbol-manipulation according to rules occurs at level 9 (programs). This implies that it might be difficult to express perceptions at higher levels (principles and system concepts). What I think we do is to use language at the program level to construct EXAMPLES of higher-level perceptions. That is, the hearer finds a meaning at the program level as a pure description of some program-like perception, or set of perceptions. Then, looking at the evoked meanings from a higher level, the hearer can perceive principles and system concepts. But there really aren't any terms that can directly denote principles and system concepts -- there's always a sort of "you know what I mean" involved. You can use a term like "honesty," but its meaning is pretty vague until you connect it with some specific process: a bank teller counting out money for a customer, for example, dealing empties every fifth time or starting with "2." Yeah, that's dishonest.

What makes a good poet (a subject about which I know NOTHING) may be the ability to use words to lay out in the listener's mind a set of perceptions which, taken together, illustrate some higher-level concept that can't be directly denoted at the level of symbol-manipulation.

>There is a professor here, almost on the net, whose interests center on >culture and anthropology, among other things. I think she'll be able to >provide some cross-cultural food for thought.

I can't wait. A cultural anthopologist would be just the person to finally check out these damned levels of mine against reality and see if they have any meaning in places other than 1138 Whitfield Rd.. There's only one other anthropologist I know of (P. J. Bohannan) who has ever been interested in my brand of control theory, and he isn't active in this field now, being retired.

Rick Marken --

OK, Rick, I promise. Before the end of the week.

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

Date: Tue, 26 Feb 91 21:53:37 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: From MAry: Misc. replies

from Mary Powers

Gary-

To continue with Derek and the tosano - I don't think he finds greenhouse an error. I think kids are perfectly happy to use a word like house generically (any structure you can go in and out of, for instance) and only gradually learn the words for all the different kinds. I've heard kids call all large four-legged animals "horse". When I had last seen him at age one he had one word for everything - bap. That took care of bottles, balls, baths, books, etc. (good thing he lived in a b-world!). As for green, I don't think he is very opinionated about color names. In fact, it's possible that he has some color-blindness. He seems to pick out crayons to use on the basis of saturation - black is best.

It did occur to me that maybe he invented tosano before he caught on to the language game - that everyone is using the same word for the same thing. This reminds me of a New Yorker article by George Steiner, who was raised multi-lingual by being spoken to by each member of his family in a different language. If I remember right, he concluded at one time that everyone in the world spoke a different language and started to make up his own. He ended up, incidentally, with the feeling that no language was really his native tongue, which leads into a very naive question: is L1 one's own language and L2 a new one learned later, or do they mean something else?

David-

Almost everything I'd say about Gail has been said by Bill, except I'd like to connect therapy to some of the parenting issues that came up with Joel - namely the idea of accepting responsibility for one's "bad" thoughts and wishes. If you can't deal with a conflict because it's too threatening to allow yourself to be conscious of one side of it, then one of the primary functions of a therapist is to create a situation that is so absolutely safe that the dangerous thoughts can be allowed to surface. Another is simply being there, listening, when it happens (I don't think control theory has anything to say about why that is necessary - any ideas?). All this sounds very passive, but it isn't - the idea is to BE a therapist, not DO therapy, which makes sense of the idea that all good therapists are pretty much alike, whatever their backgrounds and explicit rationales.

Rick-

I like your paper - it reads very smoothly. I hung up at one place that I thought needed expansion - where you talk about the typist making mistakes when she focuses on what her fingers are doing and then jump to the coach - who would only direct the coachee's attention to low levels if he felt that they needed to be disassembled and reorganized (?) - but you don't say that, and it leaves the impression that the coach wants to mess up the athlete's performance. Or anyway that's how it read to me.

Jean-Arcady Meyer-

You might be interested in the work of the Dutch ethologist F.X. Plooij - various papers and a book - The Behavioral Development of Free-Living Chimpanzee Babies and Infants, Norwood, New Jersey, Ablex, 1984. He uses control theory as a framework to explain his data. He and his wife Hettie are continuing their investigations with human infants along the same lines.

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Date: Wed, 27 Feb 91 00:45:28 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: CSG Meeting: spouses, kids, etc.

To everyone who asked about bringing non-conferencing others to the CSG meeting: Yes, kids are allowed in the dorm at Ft. Lewis College. The rooms are in two-room suites that share a common bath. Each room has two single beds. This is a fairly common arrangement for a dormitory. If one non-conferencing other shares your room, the cost will be about $30 / day, total, for room and board. (I will know the exact amount soon, when the IRS finishes evaluating the tax status of the fees the college charges for conferences.)

The college gave us a special rate for families. A family of up to four may occupy both rooms in the suite for about $25 a day more than was paid by the person attending the conference. Meals for each additional person would be about $15 a day. If you brought three family members with you, the additional cost would be $70 a day. If all four of you stay for the entire meeting, the total cost will be about $435 -- $155 for your registration and an additional $280 for your family.

If you are familiar with costs of food and lodging in the Durango area during the summer tourist season, you will recognize the bargain the college is offering to us. In return, we must honor their request that the family rates be claimed only by families.

In the CSG newsletter, I mentioned that the Office of Conferences and Institutes will organize daytime activities for families and friends. The only charge will be for admissions to museums and other sites.

The college will allow us to arrive as early as the weekend before our meeting and stay for the same low cost. This offer creates the possibility of remarkably inexpensive family vacations in an area rich with historical and recreational opportunities. By the time you receive the formal call for the meeting, probably in early May, I will have the forms for early-arrivers, who will make their own arrangements with the college.

To receive information about the Durango area, call the Durango Chamber of Commerce at 1-800-525-8855. To receive information about the (spectacular) narrow gauge railroad ride, contact:

Durango & Silverton Narrow Gauge Railroad
479 Main Avenue
Durango, Colorado 81301
USA
Phone: 303-247-2733

The railroad fills its reservations quite some time in advance, so contact them as soon as possible, if you are interested.

I am pleased that so many people plan to bring spouses, families or others. When I began negotiating with the college, I did not envision offering a vacation package. This will be different from some of our earlier meetings -- a bunch of rabid theoreticians, cloistered in a former nunnery nestled in the cornfields of Wisconsin!

I will put out the word as soon as the charges for room and board for non-conferencers are firm, but I doubt they will change from what I described here.

I look forward to seeing many of you in August.

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Date: Wed, 27 Feb 91 12:51:55 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: reading lesson for children

Chuck--The reading system is called Ball-Stick-Bird. You can write to: Ball-Stick-Bird Publications, Box 592, StonyBrook, NY 11790
The telephone number is: (516) 331-9164
Good luck!
David G.
internet: goldstein%micvax.dnet@glassboro.edu

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Date: Wed, 27 Feb 91 13:19:25 -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: Language Terminology

To all,

>is L1 one's own language and L2 a new one learned later, or do
>they mean something else?

I apologize for committing the academic error of assuming abbreviated jargon was self-explanatory. When talking about language and language learning, some shorthand which is used is the following:

L1 = the first language; language learned from birth; "native" language
L2 = a second (or 3rd, 4th) language; language learned "sometime" after birth; "non-native" language
PLA = Primary Language Acquisition; the process of learning the L1; major area of interest for psycholinguistics
SLA = Second Language Acquisition; the process of learning another language; name of field of study interested in how people learn more than one language

A point of discussion, both for the L1/L2 distinction and for the seperation of psycholinguistics and SLA, has been at what point after birth does a language become an L2; during what time in life can "PLA" take place and what time can "SLA" take place, and why. In order to distinguish the two much research has concentrated on characterizing the two processes along typical psychological research lines: learning environment, learner characteristics, description of the learner's language, learner motivation, etc.

Joel Judd

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Date: Wed, 27 Feb 91 13:14:14 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: being a therapist

Mary(910223)--
You said: " ...one of the primary functions of a therapist is to create a situation ....safe that the dangerous thoughts can be allowed to surface. Another is simply being there, listening, when it happens (I don't think control theory has anything to say about why that is necessary--any ideas?"

The patient must feel that s/he can trust the therapist. Trust is the necessary condition for establishing a relationship with another human being.

Everytime we meet someone, we will not allow ourself to become close to them until we trust them. If a child trusts to easily, don't parents worry? Too much or too little trust is a problem.

This is starting to sound like a reference condition. The experiences we have in life help us define what the reference condition will be. If we had a lot of abusive experiences, then we will not trust easily. If we grew up with "unconditional love" parents, we may trust too easily.

A therapist who is too directive may unknowingly result in a lot of

error signals in the patient. A therapist who is too nondirective
may also result in error signals in a patient.

Maybe we trust people who do not create error signals in us and
who help us reduce our error signals.

The attitude of the therapist in CT Therapy seems to be: be friendly,
don't be bossy or try to influence/persuade the person very much, be
focused on the person seeking help. The alternative attitude would
be: be neutral or unfriendly, be bossy or try to be influential/
persuasive, be self-focused.

The best attitude of the therapist to take depends on the patient.
What do they want in a therapist? If a patient wants advice and
the therapist refuses to give it, then this would not go over very well
with the patient.

David G.
internet: goldstein%micvax.dnet@glassboro.edu

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Date: Wed, 27 Feb 91 14:00:39 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: <Parser> E: "From:"/"Sender:" field is missing.
From: Undetermined origin c/o Postmaster <POSTMASTER@UIUCVMD.BITNET>

(CZIKO_Gary_A.:_U_Illinois_at_Urbana:_Bitnet:cziko@uiucvmd)g-cziko@uiuc.edu
From: (Gary A. Cziko) g-cziko@uiuc.edu
Subject: Taylor's The Behavioral Basis of Perception

Martin Taylor (910225)

Intrigued by Martin Taylor's mention of the book _The Behavioral Basis of
Perception_ by James G. Taylor (no relation, 1962), I checked out a copy
from my library.

I have only skimmed it, but it seems apparent that it uses the S-R and
S-Organism-R paradigms which control theory rejects. Here are some
extracts:

"We shall assume that each movement [of an infant], at its first
appearance, is an unconditioned response to some pattern of stimulation
that is different for each movement but always includes what Hull called a
drive stimulus." (p. 22)

"It is accepted that any neutral stimulus that regularly precedes the
application of an unconditioned stimulus becomes conditioned to the
resulting response." (p. 32)

In describing the wearing of "distorting spectacles" (the book reports a
number of experiments of this type):

"If, on the other hand, I had attempted to play a game of table tennis, I
would certainly have failed at first, since success depends upon knowing
the position of the ball relative to a frame of reference in the player's
own body, and also the position of the point on the table to which he
proposes to direct the ball. Initially these positions are erroneously

perceived, because the sense-data involve readiness for responses directed to positions other than those actually occupied by the ball and the aimed-at point. However, the game demands that many of the prepared-for responses be actually evoked, and the resulting negative reinforcement will have the effect that the player will immediately begin to modify his behavior, and will thereby acquire sense-data that will reflect the true positions in space." (pp. 342-343).

Martin Taylor and other newcomers to CSG-L may be surprised that terms such as "conditioned responses," "reinforcement," "drive stimulus", and "prepared-for responses" have no place in control theory. Finding out why may not be easy, but this network can help.--Gary

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Date: Wed, 27 Feb 91 13:48:45 -0800
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: Replies to Clark, David & Oded

Thanks to everyone who sent comments on my paper. Please, keep them coming . They are very helpful. I am collecting them in a file and will try to incorporate them as best as I can into the revision. Some of these comments were posted to me and some were to the net; I forget which are which but I do want to respond to a couple of them. I will restate the comments I am responding to in case they were posted only to me.

First, Clark McPhail commented on my description of control as the behavior of perception. Clark says he thinks of control as "producing" perceptions rather than making them behave. What I was thinking of was the behavior of the hierarchical control model (like the spreadsheet model -- I'm sending you the paper Clark). Higher level systems continuously and smoothly change the references for lower level perceptions, causing these perceptions to change continuously and smoothly. The perceptions are behaving in response to changing intentions (references). I think of control as the process of making perceptions match intentions. And when these intentions change to control higher order perceptions, the lower order perceptions change as well; perceptions behave; they move and change and flow in accordance with our purposes. Sometimes we intend to "make" a perception occur; I am not currently seeing a nice juicy steak but I intend to see one in a couple hours. I will produce that perception, I suppose, out of nothing. But usually, I am changing the value of existing perceptions in order to make the values of other existing perceptions be where I want them to be. I think of things like walking, where I continuously change perceptions of forces, limb positions, visual field projections in order to change the current perception of "where I am" into another one. I think you make a good point, though. There are often cases where we are "making" a perception where none existed before; I do this while I type these words-- the letters were not there and now they are. But I see the production of a perception (like a letter configuration) as a behavior

of that perception; when tying a letter, its the behavior of changing
the perception from that of no configuration to a particular configuration.
But I think that much of the behavior of my own intentionally produced
perception has a smooth, flowing quality.

David Goldstein asked what happens to control theory if the evidence for
hierarchical organization doesn't pan out. I think if that is the case we
will have to figure out a better organization than the hierarchical one.
But hierarchy is not essential for control organization.

With respect to my comments yesterday about a control theory approach to
simulating worm behavior, Oded Maler says:

> I have some problems with the
>claim that controlling the perception can guarantee performance in different
>environment. After all you can affect internal perception without
>affecting the externally-observed behavior.

This is an understandable problem because I didn't make part of my point
clearly (come on, other CSGers, help!). First, even though behavior is
the control of perception, you must build the system so that it produces
outputs that influence the perception in all dimensions in which it is
controlled. Thus, if you are building the worm so that one controlled variable
is the sensed angle of a joint then the worm must be able to influence that
angle (one dimension) -- probably by varying the tension on a spring attached
to the joint. Now, regardless what external factors influence the angle
(floor friction, external forces exerted by another spring, etc) the system
can keep the sensed angle at the reference level by adjusting its own
influence on the spring. Of course, there will be limits (thus, the
control system is not guaranteed to work in ANY environment). But you can
state the limits very easily; the limits depend on how much "countering"
force the system can exert. No system can work in any environment. Indeed,
that is probably what evolution is about. Systems come into the world
able to control certain variables within a range of disturbances.
If the disturbances become excessive then some systems will fail
while others (like muscle bound worms perhaps?) will survive.

With respect to your second point: yes, you can affect perception
without affecting behavior; unless you are dealing
with a controlled perception (I presume that what you mean by behavior is
the system outputs that affect the controlled perception). Rather than go
into a long thing on this, just try to imagine changing the percpetion of
temperature in a thermostat (by, say, holding a match near the sensor)
without changing the behavior of the thermostat (the level of output from the
heater and air conditioner).

Best Regards

Rick M.

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Date: Wed, 27 Feb 91 15:47:03 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: Models for movements

FOR ANYONE INTERESTED IN MODELING OF BEHAVIOR:

A couple of recent publications might be of interest to CST modelers. A brief blurb in _Science_, 1991, Vol. 251, 15 Feb. 1991, p. 742, describes a juggling robot at Yale with "a paddle that responds to the ball's motions with the aid of two video cameras." "Unlike industrial machines that don't know when they make a mistake, this one, developed by electrical engineering graduate student Al Rizzi, operates with continuous feedback." Sounds like some other people are on the right track -- or a similar track. Does anyone know Rizzi?

The blurb goes on to mention a "Robo-Pong" competition that was held at MIT on 5 February. All of the robots were built from LEGO kits. This is something we should try. As of now, Keith Deacon has a project, on hold, to build a LEGO unicycle that rights itself and "rides around." That is as close as any of us get to the Pongers, as far as I know.

RICK MARKEN, BILL POWERS: Do you remember the article by M.T. Turvey (Coordination, _American Psychologist, 45_, 938-953, 1990) that I copied and passed around during the CSG meeting at IUP? In that article, Turvey surveyed the history of research and theorizing on "Bernstein's problem," which is the problem of "understanding the control of a complex kinematic system." (p. 938) This is the article in which he describes why he, Kelso, Saltzman, Kay and others decided that it was important to begin using the language of dynamic self-organization -- terms like "dissipative dynamical systems," "attractors" --strange and otherwise, and "the dynamics perspective." With its chronological presentation, that article revealed the history of "reasons" given by reviewers who rejected CST manuscripts over a period of several years.

In the latest issue of _J. of Experimental Psychology: Human Perception and Performance, 17_, February, 1991, is an article by B.A. Kay, E.L. Saltzman and J.A.S. Kelso. Its title is, "Steady-state and perturbed movements: a dynamical analysis." (pages 183-197. I'll send you a copy, Bill.) They studied rhythmic finger movements, using a device in which the person's finger could be "pushed" at various phase angles during the rhythmic wiggling. In all cases, the person was able to resume the rhythm, with phase shifts in some conditions. The interpretation? "The stability of the kinematics in the face of perturbation indicates that an attractor is present." (p. 194) And because people could resume the rhythm no matter how fast or slow the rate of wiggling at the moment of] perturbation, "The attractor strength is also constant across movement frequency ... ." (Read it Rick, if you need more examples of trendiness.)

The underlying model assumes "...two oscillatory components: a central nervous system oscillator driving a peripheral limb segment with its own oscillatory biomechanical dynamics." "... if this model is to be taken seriously, the central oscillator is not

independent of the limb's dynamics." "If there is a central time-
keeper, it is affected by perturbations delivered to the limb being
controlled. In other words, the coupling between the central timer
and the peripheral musculo-skeletal oscillator is fundamentally
bidirectional, not unidirectional. Explicit central pattern
generator models for this activity must, therefore, include
feedback (sic) from the peripheral, controlled, system."
  For Rick and Bill, I could stop and say nothing more.
Refresh my memory: isn't this a little different from some earlier
work from these authors, and Turvey? I knew something interesting
was in store when they declared, in the abstract of the article,
that, "... any central pattern generator responsible for generating
the rhythm must be nontrivially modulated by the limb being controlled."
   What have we here?

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Date: Wed, 27 Feb 91 15:07:11 -0800
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Dynamical Demise

Tom Bourbon (910227)

I love the quotes from the Kelso article, particularly:

 "If there is a central time-
keeper, it is affected by perturbations delivered to the limb being
controlled. In other words, the coupling between the central timer
and the peripheral musculo-skeletal oscillator is fundamentally
bidirectional, not unidirectional. Explicit central pattern
generator models for this activity must, therefore, include
feedback (sic) from the peripheral, controlled, system."

and, of course:
"... any central pattern generator responsible for generating
the rhythm must be nontrivially modulated by the limb being controlled."
   What have we here?

Looks like control to me. It appears that the enemy is retreating.
But I will accept nothing less than complete surrender. Turvey,
Kelso, Saltzman et al must admit that 1) a dynamic attractor is
a reference signal 2) "non-trivial modulation" is a closed loop of
cause and effect and 3) behavior is the control of feedback, not vice
versa. Oh, I also require that they give up their leadership role in
psychology and hand it over to CSG. I also require that they admit
that they committed war crimes when evaluating our manuscripts.
If all these conditions are not met by 12:00 PST tommorrow, I keep publishing
papers on how motor control actually works. I will take no dynamic
attractor prisoners. I will continue my flanking maneuver; the dynamical
systems people are obviously looking for a devastating counter attack
in the Journal of Experimenal Psychology: Human Perception & Performance but
I'm approaching from the rear in the March issue of Psychological

Science. (Who said science isn't fun?)

Thanks again, Tom. I'll try to look at those references soon.

Best Regards

Rick M.

****

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Date: Wed, 27 Feb 91 23:04:11 est
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: Multiplexing
In-Reply-To: UPPOWER@BOGECNVE.BITNET's message of Fri, 15 Feb 91 11:40:09 -0600

Dear Bill,

It's good to finally get through (I've tried to post to Bitnet a couple of times in the last few months--on reorganization and structural evolution--but the mechanics failed me.) I'm sorry my response time is slow -- I have very little time these days (a postdoc, a baby, papers to get out), so I'll try to respond as fast as I can.

Basically, the model you outlined is more or less the basic assumption that most neuroscientists and neural net people are operating with. Let's call this the Standard Model. This assumes that the signal being sent by the neuron is its average firing rate.

An alternative to this is an interspike interval code in which the signal is encoded by some characteristic interval(s) in the spike train. Now this code is still an analogue code--the intervals can vary continuously from the absolute refractory time of the neuron (hundreds of microseconds) to hundreds of milliseconds (or more). Such a signal is generated by elements which fire repetitively at a (relatively) constant interval. (If we look at the auditory nerve array, at each "place" on the frequency axis of the basilar membrane in the cochlea, the auditory nerve innervating that hair cell does fire more when the corresponding frequency is presented, but it also fires at characteristic intervals (1/characteristic freqeuncy), and it looks to me like this is a much better explanation of the psychophisical data that the rate-place hypothesis--the rate-place models have an enormously complex task of reading the entire nedrve array/frequency spectrum and doing pattern recognition on it all--in noise, over 90dB intensity range, recognizing clipped speech, etc.). An interspike interval code can be "read" or "interpretted" by an element with nonlinear properties in the temporal domain. It has been found experimentally by Steve Raymond (and others) that the threshold curve following a spike (N.B. threshold, not voltage) follows a triphasic pattern-- an absolute refractory period, a "superexcitability" period, and a period of relative depression. These three periods seem to be alterable independently over many time scales (msecs to hundreds of msecs and even longer) and are activity dependent. They change with

activity. This sort of element looks like a nonlinear oscillator: it fires preferentially when stimulated at the frequency where the input signal peaks at each superexcitability period. There are cells in the cochlear nucleus that I believe are doing this sort of processing. If this is valid, neurons are more like temporal processors/oscillators than they are like leaky integrators or logic gates. There are many advantages to networks of coupled oscillators (exact timing isn't so important and signals can be encoded in intervals which are orthogonal to each other (we are now in the frequency domain))--a spike train could conceivably carry many different periodicities in the spike intervals and elements (axon branches or postsynaptic neurons) further down which are sensitive to one of the many intervals will preferentially pick up that interspike periodicity. I know this is a really cursory outline, but we can discuss it further.....

I think the problem of the exact nature of the neural code(s) is orthogonal to Control Theory, i.e. Control Theory would be just as valid if neurons were the linear elements of the Standard Model as it would be with neurons as temporal processing elements. The main advantage of these analog temporal codes is their open-endedness and plasticity--another periodicity can always be added by changing the tuning of the excitable membranes (alter the timing of the superexitability phase), and this can be done by changing ion channel densities. They are also nonlocal coding mechanisms--they don't depend on a particular neuron playing a particular function (this is the only way I could imagine the inverted-eyeglasses or seeing-with-the-skin phenomena could even be possible).

Glad I finally got through. I've been enjoying listening to you all for the last few months. My response time constant is alot slower than the average for this community, so we'll see if this control/communications structure can handle processes with different frequencies....

Peter

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All queries, comments, criticisms and suggestions welcomed.	

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Date: Thu, 28 Feb 91 09:27:27 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: THE TEST

The test of the controlled variable runs into problems when applied in the clinical situation. This is what was behind my trying to come up with some new ways of testing for controlled perceptions in a clinical situation. Let me see if I can indicate what some of the problems are.

The therapist guesses at what perception a person is controlling.
The therapist applies what s/he thinks is a disturbance.

The therapist does not observe the person react to the disturbance
in any way.
One interpretation is that the perception is not being controlled.
A second interpretation is that the disturbance did not work.
A third interpretation is that the person has decided not to respond
in order to hide or cover up.
A fourth interpretation is that the person is in conflict and that is why
a response was not observed.
A fifth interpretation is that the person experienced an error signal
but was not aware of it. Perhaps awareness of error signals is required
before we will observe a countermeasure.
A sixth interpretation is that the person doesn't know what to do or
say.

A last comment I would like to make is that psychotherapy is basically
a verbal affair. If Control Theory cannot apply to verbal stuff, for
whatever reasons, then it is not going to be very useful in therapy.
And most psychotherapists will not be very impressed by it.

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Date: Thu, 28 Feb 91 16:29:21 -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: Maybe let's post this bit...

>Hello everybody.

>Joel Judd and I have been having an interesting
>interchange sparked by the L1/L2 clarification he posted. We
>thought you all might like to respond.
>
>The basic issue here is how CT would handle language "policy", or
>the sincere efforts by some to mandate certain
>languages in a national setting. This has happened in many countries,
>and is currently hot in the USA. In some countries (Switzerland,
>Canada) multilingualism does have legislative force.
>
>The two sides to the issue in the USA:
>
>"English-Only" -- English should be legislated as the national
> language of the USA.
>
>"English-Plus"-- U.S. citizens should speak English and any
> one other language [Joel's motivations behind
> being bilingual, and his citation of Edgarton,
> are excellent below.]
>
>I (Fred) have always found the legislative control of language difficult
>to stomach, and as Joel says below, there is good reason against
>it from a CT perspective as well. I don't like it as an applied
>linguist -- it's just too difficult a beast to corral.
>
>But here's what Joel and I have so far. The carats are
>Fred and the replies are Joel.
>
>-Joel Judd and Fred Davidson
>

>P.S. [From Fred] -- this interchange really helped me understand CT
>better due to its contextual relevance to my work.
>-=-
>
>[Fred]
>>I am reminded of what James Fallows, the **Atlantic** columnist
>>and former Pres. Carter speechwriter said at a conference last year.
>>A questioner asked Fallows after his speech: What do you think of
>>the movement to declare English as the official language of the USA?
>>
>>Fallows replied: "I have a hard time getting excited about either
>>side of that argument. Legislating
>>English is a bit like legislating daylight." He explained that
>>in his travels he senses vast desire to learn English the world over.
>>This is a very touchy issue, philosophically. Is English finding
>>its own level of acceptance? Does it have its own impetus?
>>CT would say no? -- isn't that the environmental argument?
>>Or are people **motivated** towards English? What of the counter
>>feeling that FL learning by English speakers is a bloody good
>>idea? (Agree, Joel?) I don't know -- what would CT make of
>>the role of English in the modern world?
>[Joel]
>If there is a usefulness to learning English, especially if learning it
>reduces error in high level controlled variables (education, vocation,
>etc.), then people will want to learn it. The perception of Fallows stems
>from the perception of most(?) of the world that English is good for
>something. WE label this ethnocentric, political, etc. For CT, English is
>simply a behavior which reduces error in a lot of people, and reducing
>error is the name of the game. "Motivation" in CT is simply the drive to
>reduce error. If I am Swedish, and I want to have an international
business
>in computing devices, and I realize that the most successful way to do
this
>is knowing English (or at least enough to do business), then I have an
>error between the controlled variable ('successful inter. business') and
>perceptual variable ('limited national business'). Learning English is a
>behavior I produce to change the perceptual variable and reduce the
>discrepancy between it and my goal state. A traditional psychologist, or a
>Schumann [developer of the "Acculturation Hypothesis in SLA] disciple,
would say I am "instrumentally motivated" to learn
>English.
>
>I agree that learning another language is a good idea, but for other
>reasons I am only beginning to understand. First of all, of course,
>mandating EITHER English Only or bilingual programs is an attempt at
>[educational] control, and CT clearly shows that you cannot control other
>control systems, politically or educationally. I am for learning other
>languages because, in the words of Mills Edgarton, it makes people less
>dangerous. "Knowing" a foreign language (the title of his 1983 paper)
means
>that I understand what a Chinese speaker means when he says something; in
>other words, knowing another language is valuable when we learn something
>about the perceptions speakers of that language have. I see (read:
>perceive) the world another way. How that is possible, even how we meld
>perceptions in our L1, has become more interesting to me because CT offers
>some understanding of perceptions, and language, and how they might be
>related.

>
>Global realities somewhat temper such an idealistic view. As Edgarton
>points out, we rarely do something anymore just for the humanistic value
of
>it. We do it because it will get us a better job, or increase our
prestige,
>or something else. And speakers of different language communities are
>becoming less and less "islands unto themselves". Each generation has more
>and more of a shared world perspective, and therefore shared perceptions
>which are articulated in language. The other side of the coin is the case
>of English, which is being disseminated world-wide. But I would think that
>the perceptions communicated in English by a Pakistani are hardly similar
>to those of a Californian like myself, or an Illinoian. How much speech
>communities will be able to hang onto "their" language against the heathen
>onslaught (eg. Korean, French), and how much the world will turn into a
>"global village", I don't know.
>[Fred]
>>I guess I feel that language should
>>be left alone.
>[Joel]
>Right. It's not languages' fault. Language is simply another behavior
which
>we can perform in the environment to control our perceptions.
>
>
>
>
>
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Date: Thu, 28 Feb 91 21:16:58 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Neural stuff

Peter Cariani (910227) --

>An alternative to this is an interspike interval code in which the
>signal is encoded by some characteristic interval(s) in the spike train.
> Now this code is still an analogue code--the intervals can vary
>continuously from the absolute refractory time of the neuron (hundreds
>of microseconds) to hundreds of milliseconds (or more). Such a signal
>is generated by elements which fire repetitively at a (relatively)
>constant interval.

I don't think it matters much (except computationally) whether you use f
or 1/f as the variable of interest. The real problem here is in the
concept of multiplexing, carrying more than one dimension of signal in a
single axon. Considering the integration of ionic currents in the neuron,
it's hard to see how individual inputs to a neuron could end up as
interleaved independent signals in the output axon.

Also, you still have to worry about DEmultiplexing. If you don't
DEmultiplex, then all you have is a single signal, the average frequency
of which is changing in certain patterns. The patterns have no
significance unless some other system explicitly differentiates between
them by responding to them differently. When you add two constant-

frequency signals together in a subsequent non-electrical neuron, you don't preserve the pulse-to-pulse spacings. That gets smoothed out and becomes just a single frequency. The IMPLICIT pattern in the travelling signal doesn't mean a thing, because there is no physical effect (or not much compared with the main effect) at the destination traceable uniquely to one of the individual inputs.

>If we look at the auditory nerve array, at each
>"place" on the frequency axis of the basilar membrane in the cochlea,
>the auditory nerve innervating that hair cell does fire more when the
>corresponding frequency is presented, but it also fires at
>characteristic intervals (1/characteristic freqeuncy), and
>it looks to me like this is a much better explanation of the
>psychophisical data that the rate-place hypothesis ...

I'm confused: how do the nerves fire "more" when the characteristic frequency is present, but also at a characteristic (constant) interval? In what respect is "more" measured? Pulse amplitude? As far as I know, auditory impulses are still the same old "all-or-none" spikes, and in any one auditory receptor nerve the frequency of firing can be anything between zero and maximum. The firing frequency depends BOTH on the auditory frequency AND the amplitude of the vibrations, ambiguously.

>--the rate-place
>models have an enormously complex task of reading the entire nerve
>array/frequency spectrum and doing pattern recognition on it all.

It's a lot simpler than the problem in the eye, where there is a 2-dimensional array of far more receptors. And in the eye, place information is obviously important, and is carried by place-type representations.

I'll admit, however, that audition is peculiar, particularly harmonic relations. We have to preserve amplitude information because we can perceive and control sound in the dimension of intensity. But we have to preserve rather precise frequency information as well, because we can perceive and control sound in terms of thirds, fourths, fifths, sixths (which are minor thirds), and octaves -- very precise harmonic relations.

One way for this to work would be for the auditory neurons to lock to the vibrations of the basilar membrane. But the mechanical tuning of the spiral is NOT sharp; it is very broad. All I can imagine is that each "place" receptor would be part of a tuned oscillator (with local positive feedback connections) that provides the sharp tuning needed. In that case, however, their frequency couldn't also indicate the varying intensity of a constant-frequency sound at the same time.

I think it's more likely that the entire auditory array is subject to a deconvolution process that imposes the sharp tuning. This is akin to image-sharpening, where the input consists of fussy overlapping regions of intensity and the output is a continuously computed set of signals that represent a hypothetical image made of discrete points (here, a one-dimensional "image"). The spacing of the discrete points would depend on how many "terms" in the "equations" are carried in the computation. With lock-on oscillators there would be no way to distinguish between detuning and a drop in intensity of a constant-frequency sound. With deconvolution occurring at a higher level (sensations), we could have both intensity

signals (directly from the receptors, with very broad tuning) and pitch signals (after the deconvolution). The pitch signals would sample roughly an octave, I suppose, in fifteen steps. In different cultures, it could be that different deconvolutions arise from learning, creating different tone scales. That would fit your ideas about learning, wouldn't it? Then, if the pitches were represented literally by neural signals of the same frequency (over an octave anyway), the next level, configurations, could distinguish invariant configurations of harmonics.

>It has been found experimentally by Steve Raymond (and others) that the
>threshold curve following a spike (N.B. threshold, not voltage) follows
>a triphasic pattern-- an absolute refractory period, a
>"superexcitability" period, and a period of relative depression. These
>three periods seem to be alterable independently over many time scales
>(msecs to hundreds of msecs and even longer) and are activity dependent.

What this means is mostly a matter of interpretation. I would like to see SOMEBODY do a study of input-output relationships in a neurone using frequency as the variable. It can't be that damned hard to do. We ought to be able to find a transfer function that gives a good match to the way the output frequency depends on input frequencies and their derivatives or integrals. Tougher problems are solved in engineering every day. The problem here is that once you get it into your head that the interspike INTERVALS are the interesting variable, you inevitably start giving significance to individual spikes. You start looking at their shapes with a microscope. There are all sorts of reasons to say that individual spikes are of no significance in brain operation. Unfortunately there is a huge investment in the spike-at-a-time approach, going all the way back to the roots of neurology 100 years ago. I have no doubt that these details of the threshold variations exist and are part of the functioning of the neuron. But we won't understand how they explain neural function until we find a good way to characterize what neurons DO. I think the frequency approach is the best bet for doing that. Once we see WHAT computations are being done, we can worry about HOW they are done. We have to exorcize that digital demon.

>I think the problem of the exact nature of the neural code(s) is
>orthogonal to Control Theory, i.e. Control Theory would be just as valid
>if neurons were the linear elements of the Standard Model as it would be
>with neurons as temporal processing elements.

Correct and very well put. But as part of "Elaborated control theory" we have to be concerned with the realism of the model vis-a-vis direct experience. We have to do experiments and build models that can recreate as much of behavior and experience as possible. This means that when we settle on an interpretation of neural phenomena, it must be one that allows the world to appear to us as it does appear: smoothly variable, and decomposable into readily-recognized classes that show hierarchical dependence. If someone has a model of neural functioning that will fit these requirements, I have no reason to reject it -- as you say.

>Glad I finally got through.

So am I. Tell your baby that a very useful word is "bap."
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Thu, 28 Feb 91 21:19:31 -0600

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Subject: Reorganization in motor cortex

Of General Interest to students of self-organizing systems --

Kimberle M. Jacobs and John P. Donoghue, Reshaping the cortical motor map by unmasking latent cortical connections. Science, 22 Feb 91, 944-947.

[the following is the first paragraph of the paper -- not the abstract -- copied with references omitted and explanation added in brackets]

"Although it has been argued that the MI [primary motor cortex] map is basically stable over time in adult animals, the possibility that MI continually reorganizes has been recognized since Sherrington's studies. The details of MI maps vary considerably in size and shape between animals, but the relative location of face, forelimb, and hindlimb areas is one consistent feature. It has been shown that new MI representation patterns emerge after peripheral nerve transections in rats. Within a few hours after the motor nerve innervating the mystacial vibrissa is transected, movements of body parts normally represented in adjacent motor cortical areas can be evoked by stimulation within the former MI vibrissa area. Thus, it appears that a region of motor cortex modifies its output organization so that one set of cortical neurons influences a new set of muscles. The rapid time course of this reorganization suggests that, rather than growing new connections, existing synaptic connections alter their effectiveness." (p. 944-945).

This kind of output reorganization suggests several things to me.

First, reorganization is focused at the site where a control problem has arisen. Note that this happens even though the transaction occurs DOWNSTREAM from the location that is reorganizing. The reorganization clearly is instigated by interruption of the function of a whole closed loop: it must be the failure of control, perhaps evidenced by loss of control of sensory cortex perceptual signals and the consequent increase in an error signal, that elicits reorganization.

Second, reorganization of output appears to involve a search (random, I presume) of adjacent lower-level output sites for a place where output signals can again have effects. The artificial stimulation that evokes movements presumably happens in the same place where natural stimulation would occur -- that is, roughly where signals from higher-level output functions would normally enter the motor cortex (physically from below and laterally; hierarchically from above). The artificial signals could be imitating reference signals or error signals, both of which are associated with the motor cortex (in higher centers, comparison typically takes place in a motor area or a motor nucleus).

When the transection occurs, the higher system's action no longer is able to cause movements and thus create perceptions of movement. The same artificial reference (or error) signal as before now produces output signals that accomplish nothing (especially not reduction of the error). The reorganization described appears to result in a re-routing of the output signal to different lower-order systems. Because of the topographic mapping in the cortex, muscles somewhat related to the one

whose function has been lost come to be activated by signals that now reach places cortically near the formerly effective sites. So the effect of reorganization is as though a search commences for a connection to lower-order systems physically near those that have lost their effectiveness. Muscles near the ones that can no longer be reached might have effects that still contribute, although not as strongly or reliably, to control of the perceptual signals formerly under control. This would restore at least some control and mitigate the condition that is driving reorganization. The "condition," I would guess, is just an abnormally large error signal in the control system.

The authors' experiment is a little more complicated than described here and involves reversible chemical means rather than transection. The aim was to temporarily disrupt inhibitory connections that bridge the boundaries between adjacent cortical areas. It's not clear what the effect of the chemical was other than reducing inhibitory signals. It's possible that the loss of control was caused by interfering with a different part of the control loop (perceptual signals would enter in the inhibitory sense). Or it's possible that the chemical interference imitated one of the mechanisms of reorganization. Perhaps chronic abnormally large error signals have an effect like that of the chemical.

The adjacent area that was put into motion by the signal that formerly operated vibrissae was the forelimb region. It might be that movements of a forelimb would be an alternative way of producing sensations that are normally produced by moving the vibrissa (against the forelimbs as in grooming, or against obstacles). Or perhaps if this type of reorganization occurred in natural surroundings and for a protracted time, it would not be retained because it would not correct the loss of control.

The third main idea suggested by this paper is that reorganization is always at work, implying that the current organization of behavior represents a state that is under continuous control. Because reorganization contains a random component, this continuous control must involve continuous variations that are biased to converge again and again on one state that minimizes the intrinsic errors that drive reorganization. When a drastic physical change occurs, this process immediately begins working (through BVSR), continuing until another organization is found that will serve the same purpose.

This study didn't directly attack the problem as a control theorist would see it, but the results give us a little more to chew on.

The picture I get is that of a higher-level control system presented with some array of lower-level comparators where it could send its output signals. During reorganization, it pokes around until it finds a place where injecting a reference signal will result in (a) a change in the perception it is trying to control, and (b) a change of the right sign. This makes "the reorganizing system" seem like an aspect of the forming control system itself, rather than some system operating from off to one side somewhere. At least it must be a DISTRIBUTED system capable of having its connection-altering effects anywhere in the hierarchy. It's pretty clear that reorganization takes place right where the problem exists, that it responds to sudden problems right away and with great specificity, and that it responds appropriately even to problems that originate at a remote site in the control loop.

It also seems clear that the motor functions in the brain are spatially distributed in a way that promotes the effectiveness of reorganization of this sort. Or maybe that's just a natural outcome of the process itself.

Sudden thought. Could the glial cells have something to do with reorganization? They outnumber ordinary neurons 10 to 1 (if I remember right) and nobody knows what they are for.

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