CSG_9106

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Date: Sat, 1 Jun 91 17:39:15 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Intents and Accidents

[From Rick Marken]

I'm posting from home again which means I can't use my editor and I might be interrupted. But I just thought I would try to give a quick followup on the "accidentally produced" results as a possible aid to learning topic. Joel Judd (910529) said that he would call the profanity that is written as a result of compensating for 2 dimensional disturbances to cursor position an accidental result-- and I heartily agree. It actually would be a great demo to set up because writing a word (not necessarily a profanity) as a result of controlling a cursor look so much like a behavior someone is "doing". It is even better if you can get people to do things that they could not do "intentionally" -- like making a perfect drawing of some popular cartoon character. This would be a powerful way to demonstrate the fact that what people do is not necessarily done intentionally. This is easy to understand when the unintentional behavior is something that is easy to do intentionally -- like knock over a glass. But have someone draw the Sistine Chapel ceiling by accident (and a person could be made to do this in response to disturbances of another variable) and you would definitely create some excitement. Of course, as Bill mentioned, this might also be a great way to teach people certain skill (like drawing). I'm pretty bad at drawing; maybe I could learn it using this technique.

I agree that it would be hard to find verbal examples of this phenomenon. The idea would be to have someone say something complex by using their voices to resist disturbances to another variable. I guess something like this is done when a typist types a manuscript that he/she does not understand. The typist is controlling a relation ship between letters typed and letters on the page. But the result is a paper expressing a higher order set of programs and principles that the typist could not produce intentionally. Actually, my experince is that typists do seem to learn something about the higher level variables being expressed by the typing -- and become better secretaries because they do learn something about how to intentionally express the programs, principles and system concepts that are communicated in the papers.

Bye for now

Rick M
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Date: Sun, 2 Jun 91 01:42:57 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

From: Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject: heeding the functional level; artificial cerebellum

[From Joe Lubin (910601)]

Rick Marken (910531)

> > We can not access our retinal, or
> >cochlear representations, only abstractions of these.
>
> I don't understand this. We simply experience. The physiological
> evidence and models suggest that all neural representations (from
> the retina, up) are abstractions of what our current physical
> models tell us is reality.

By this I meant that we only experience representations at levels higher than these sensory transduction levels. Our conscious mind -- instantiated for the sake of argument as an attentional mechanism -- cannot operate on these primary levels. They are involved in automatic computation; they are afferents to the levels than we can consciously manipulate and consciously access. (There are no efferents from the brain into the retina in primates. There are significant efferents to the cochlea, so this is a poorer speculative choice for the above argument. Essentially the retina is on its own and no other brain area can manipulate its processing.)

> So, while I agree that neural findings constrain the details of
> the model somewhat, I do not believe that the model should be based
> on those findings. Indeed, those findings, being experiences
> themselves and contingent on the accuracy of current models of
> physical reality (which are tentative -- remember, we used to think
> Newton was right) are themselves data to be handled by the control
> model itself!

You sound almost defensive. There is no reason to be. The reason I read CSGnet, and the reason I have chimed in, is that I feel that PCT is indeed, as some of the literature claims, a most powerful framework for understanding behavior as well as a potential revolution for the behavioral sciences. I do my work by accumulating data and searching for frameworks -- either implicit in the data or explicitly rendered using potentially apparently dissimilar formalisms: I guess this makes me a theorist. PCT is not one of those frameworks that I will be able to reject, for two reasons. It feels right at what I would call a systems level (and what you would call "functional model"). It also provides a tremendous amount of manouvering room as PCT workers begin to flesh out the connections between the higher level functional constructs of PCT and the computational substrates of neuroscience.

> If neural evidence requires changes in the model that make it a
> poorer model of control, then there would have to be some serious
> re-looking at the neural evidence, the phenomenon of control and

> the model itself.

This is the dielectic of theory and experiment; it is essential that constraining and explorative information run in both directions. I tend to view neural data in terms of higher-level formalisms. This is the way I organize my understandings of a very complex field.

> Your knowledge of neural processing can certainly "keep us honest"
> by limiting, to some extent, our speculations about how the
> functional model should work.

There is probably not much I can discount in anybody's modeling work (as long as the constructs/computations employed are remotely reasonable) while remaining on firm neurological ground. The variety of computations that might be done in the brain for all we know is large. My function, if I am to have one, would be to propose likely computational constructs in order to ground the systems level descriptions. Its all got to be built from neural stuff -- boxes and arrows are only so convincing.

> This last comment reminds me of a little neuro-paradox ...
>
> ... I would think that the researcher would trust his EXPERIENCE
> of the letter X and start reconsidering the neural evidence. Neural
> grounding could produce absurd results.

Is the imagination perhaps more dangerous (deceiving, labile, unconstrained, "runaway") than data?

Bill Powers (910531a)

> Years ago I worked out an algorithm that derives graphical
> transfer functions (non-analytic) from real-time tracking data.

> The "artificial cerebellum" keeps computing a transfer function
> and correcting it ....

Can you be more specific about what you mean by transfer function -- perhaps with a detailed example. Neural net modelers employ the term to stand for the function which takes input to a neuron and transforms it into output. This loosely corresponds to the input-current/output-voltage characteristic of real neurons.

> I can show you this demo at the meeting in August, and also
> explain the algorithm and how it translates into neural circuitry.

Have you written this up? If so, send it. Be warned that I know little about motor systems. I am on more solid ground when dealing with sensory systems (particularly vision), and cognition (whatever that is).

Bill Powers (910531b)

> Rick is saying that experience is the final arbiter when it comes
> to brain models -- not just experience of images in a microscope or
> traces on an oscilloscope, but experience of the world and one's
> inner self. This is why functional models will ALWAYS be important.

I may not sound like it, but I do rely strongly on my intuition, which I think is similar to saying "experience is the final arbiter."

> But if the functional model tests out against experience with
> sufficient accuracy, it can provide a way for neural
> researchers to conceive of the system they are trying to explore.

I am a big fan of multiple levels of analysis and understanding. Phenomena must be conceived of at an appropriate level to facilitate understanding. I will never be able to understand what causes me to control my perceptions as I am engaged in a game of squash by looking solely at the molecular level (or try a quantum mechanical description). Your control theory is in place at its appropriate level -- and there is undoubtedly much work to do both at that level and in relating that level to lower levels. (And higher levels -- might there be any? Without even knowing the levels I would bet that there are.) Fortunately, it appears unnecessary to consider all levels in the construction of viable models due to a modularity employed in the construction of the biological systems being modeled. Blah, blah blah; this is all obvious but I guess I have to make it clear that its obvious to me also.

> I would go even farther, and say that you always find what you
> are looking for in neural research -- it's too easy to create
> put-up experiments that are designed to favor a preselected
> interpretation.

And also the data are rarely very constraining on what computations are possible and impossible. Its like trying to write an article and finding out you ONLY have the characters on your keyboard to work with.

SUMMARY: Yes, I agree. That's why I'm here.

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Date: Sun, 2 Jun 91 13:31:35 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: reply to anxiety

From Ed Ford June 2, 1991

Gary - You said "even if the original source of conflict can be found and eliminated, it is possible ..that the person can still remain anxious about being anxious..." And finally, you suggested that possibly "some type of desensitization would be needed here."

When I work with people as a counselor or therapist, I perceive them as "living control systems" trying to restore and then maintain harmony within their systems. I see my job as trying to help them to restore this harmony by teaching them, in the most efficient way possible, how to accomplish this task. I don't see my job as "finding and eliminating" or "trying to figure out what to do to them" but rather helping them to reflect on how they have created their internal world (only they can really perceive their own created world) and, secondly, teaching them how to restore harmony and the sense of satisfaction that comes from having achieved this goal. For example, when I work in juvenile residential treatment centers, I'm working with children who don't have a belief in their system's ability to deal effectively with their conflicts. In short, their conflicts aren't really their problems, their conflicts are merely symptoms of their problem. The real problem these juveniles have is that they lack a belief in themselves, their living control systems if you will, to restore harmony to their lives. Not only is their belief in themselves lacking, but also they lack the skill to use their systems effectively. The difference between those juveniles and you is that you have a strong belief in yourself along with the skill for dealing with your conflicts as a well-functioning living control system should have. They don't have this confidence. You are both designed the same, but their confidence in their ability to deal efficiently with their system is what is lacking. Your comment of a person thinking "what would happen if I freaked out here" is just evidence of this fundamental problem. When someone else resolves their problems, their life may temporarily get better. They may feel better, they may act better, but the next serious conflict comes along and they continue to get into trouble. Why? The fundamental problem still exists. They lack self-confidence. They don't believe they have solved their conflict. They believe someone else has. When someone else solves our problems, WE BUILD NO CONFIDENCE WITHIN OURSELVES. WHAT WE DO BUILD IS A DEPENDENCE ON OTHERS. Juveniles have got to develop within themselves the belief THEY can resolve their conflicts. Parents who always rush to protect their children from having to deal with problems are really protecting their children from developing this badly needed self-confidence. If I solve my child's problems, am I really helping my child to grow? So it is not the elimination of the

conflicts, but rather TEACHING OTHERS THE PROCESS BY WHICH THEY CAN DEVELOP A BELIEF IN THEMSELVES THAT THEY CAN SUCCEED BY RESOLVING CONFLICTS THROUGH THEIR OWN EFFORT. Juveniles have no confidence in themselves and lack the badly needed skills and you have both. That is the main problem in the field of juvenile corrections. They lecture kids, they punish kids, they diagnose kids, they staff kids, and they put kids in various programs to try to get them to behave and motivated. What gets anyone going is first the juvenile has to perceive that someone believes they can make it, and then, as they begin to achieve their own chosen goals through their own creative thinking, they begin to build self-confidence. I struggled for years to try and understand control theory. I made it first through Bill Powers belief in me along with Tom Bourbon and others. Bill once told me that I could make it but that I would have to struggle on my own, that it was a matter of self discovery. He could explain the ideas and encourage, but he couldn't do it for me. And therein lies the heart of helping people to resolve their conflicts and build self confidence. Someone has to believe they can make it, but in addition, it is through a person's own effort, not another's, at succeeding at their own chosen goals (not chosen by the treatment center) that builds the belief in self through that self discovery process. As the cloud of my confusion concerning control theory began to lift, I not only understood control theory, but, more importantly, I really began to understand the value of knowing how a living control system works can do to people trying to reorganize their lives. That is why I teach it wherever I go. Just the limited understanding along with using that understanding to reduce current conflicts helps them to realize their lives are in their own hands, they have control over re-establishing and then maintaining harmony within their own system. They no longer blame their parents, their past, their feelings, or anything else outside of themselves. They deal only with themselves. Heck, that's what makes this stuff so darned exciting. Just think of the potential for restoring this human race to sanity, along with some help from Someone above (sorry, just had to put that in).

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Date: Sun, 2 Jun 91 17:06:40 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: neural nets, misc

[From Bill Powers (910601.0800)]

Joe Lubin (950131) --

>> The physical arrangements in the brain are not represented, as far
>> as I know or can imagine, as neural signals. Therefore they are not
>> themselves represented in experience.

>Yes, but by being fundamental to the computing substrate, they

>constrain experience.

We've established a middle ground of agreement that's enough to assure that we're not headed in opposite directions. Hierarchical control theory and neuroscience at least as you approach it can clearly live together. Now the question is: can they learn from each other and become a Grand Unified Theory, or will they split over the same issues that have traditionally divided psychologists and biologists?

Before we go any farther I have to take some steps to restore my intellectual honesty in this discussion. As people on this Net will attest, I have repeatedly warned that the "levels" in my model, an extension of the levels that R. K. Clark and I started with over 30 years ago, are based more on private observation than scientific experiment, and should not be memorized. But in the heat of practical discussions, I often talk as if experiments that in principle can test for the existence of these (or other) definitions have already been done; in fact very few of them have been done. All theorists do this -- refer to results they think they know how to obtain as if they have already been obtained. That's part of telling the tale. But one has to pause now and then to set the record straight.

I will defend the idea that these level-labels individually refer to types of perceptions that human beings experience and control, because I know of one human being who experiences and controls them, and I assume I am just an example of the human race in general. But verbal descriptions are always fuzzy, and furthermore there is a total lack of proof that these categories of perception are arranged hierarchically, or always have the same hierarchical arrangement, are operative in every context, or are even at different levels. It is far easier to take two specific perceptions and demonstrate a hierarchical control dependence between them than it is to find general levels in which this dependence is always seen. I am damned sure that in order to drive into town, I have to keep the car on the road, but that in order to keep the car on the road I do not have to drive into town (puns aside -- the repair shop is in town). But I am not sure that in order to control a transition, it is necessary to change a configuration -- in general. I know that in order to create the impression of an object moving toward oneself, it is necessary (and in some cases sufficient) to make the apparent diameter of the object change, but that to make the apparent diameter change it is not, in general, necessary or sufficient to create the impression of an object moving in depth. Think of blowing up a balloon.

That works in the specific case of object sizes and distances. One is tempted to generalize. But you say that

>Visual motion and spatial information can be extracted computationally
>without employing explicit configuration perception constructs (shape
>representations).

Yes, I agree that they CAN. I have even attempted to include some such backup position by saying that a given level of perception is drawn from signals of the next lower order of perception, OR signals below that level (signals that skip levels on the way up). Your statement, however, is a little ambiguous because the question is, ARE they so derived by the

human brain? My position is that object-motion perception requires that there be something moving against a background, implying prior discrimination of the object from the background. Nothing can move in a uniform visual field: a white cat creeping across a snowfield.

But clearly there are edge-detectors in the retina, and when edges move, there are motion-signals developed. Furthermore, in physics we learn that position (a spatial-pattern variable) results from integrating velocity (a motion variable). In the self-stabilizing arm model I will show you in August, the hierarchical relationship between velocity control and position control puts velocity at a lower level than position, for exactly this physical reason. So you can see that I say one thing in the abstract hierarchical model, and another in practical models that work. I am clearly confused.

The truth is that I believe BOTH views: that velocity comes before position in physics, and that velocity-perception requires hierarchically-prior perception of position.

Doesn't the same problem exist neurologically? There certainly are motion-sensitive processes very low in the visual system -- but there are also motion-sensitive processes at much higher levels, aren't there? What this suggests to me is that I have left a hole in the hierarchy: that there is a very low-level kind of change-detection, which conforms to the physical relationship between velocity and position, and also a higher-level kind of change-detection, which is the one I have represented as the transition level. It may be that the lower-level one is not accessible to consciousness, which is one way of accounting for the fact that I missed it because my method depended on consciously noticing experiences and identifying them as perceptions. Of course another way of accounting for the omission is that I am still taking the lower level of change-perception for granted as an aspect of the external world, which was true of all the other levels until I realized that they were perceptions. So far, however, I haven't noticed that.

There are more possibilities. In the level I term the "configuration" level, ALL aspects of static space are included: shape, size, separation, and position for the visual modality, for example. This again may prove to be a collapse of several levels into one, or an inclusion of levels that are really much higher.

These problems (and many more like them that will show up as we get further into the details) constitute a big mess with a slight suggestion of superimposed order. Neurological investigations are going to help a great deal. At the same time, however, neural researchers are not going to recognize the significance of afferent signals that they have not identified themselves as aspects of human perception. Where control theory comes into the picture is through providing an experimental method that can identify at least those signals that are in fact under behavioral control.

Our biggest problem in bridging the gap between the neural level of investigation and the behavior/experiential level is finding the correct level of description of functions of the brain that corresponds to the sorts of functions we can observe by experimenting with whole organisms.

When we find that level, it will be clear that any more detailed description belongs at the component level -- that is, at the level where we find out HOW the more global functions are implemented.

If we set the bridging level of description at too high a degree of abstraction, we lose the ability to span the gap. If we set it too low, we are overwhelmed with details we can make no sense of. At the right level, and I am convinced that such a right level exists, we can find functions that have clear significance in behavioral/experiential models, while at the same time clearly deriving from mechanisms of more detailed kinds. I think that control theory models are expressed at pretty close to the right level, even though we're still fishing around for function definitions that will fit the requirements.

In the midst of establishing our agreements, I hope that on both sides we will also take pains to identify ambiguities, contradictions, and holes. I believe that research is really driven by questions, and that the questions represent areas where we are confused and contradict ourselves, or areas where we are working from assumptions that are leading us astray. I think we have to keep our priorities straight, as Ed Ford would say. What we are all after is something pretty grand and inspiring (at least in my view): A unified conception of human systems and other organisms that is consistent all the way from the molecular level to the level of whole-organism behavior -- and that is consistent with all our other models of the world, including physics and chemistry and the world of direct experience. We can't be JUST neurologists or JUST psychologists or JUST sociologists or JUST physicists. We can't pick one level of observation and make that the center of the universe, if any larger picture is to emerge.

I think that the Grand Unified Theory is attainable. It will never be the Right theory, of course -- but at least it can be a theory that is broad enough to include all of human experience without simply being a huge empty generalization. I hope we're together in holding this as our aim.

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Date: Sun, 2 Jun 91 20:29:35 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: neural stuff: transfer functions: perception

[From Bill Powers]

Joe Lubin (again)(910602) --

>... we only experience representations at levels higher than these
>sensory transduction levels.

I'm not sure this is true. Signals coming directly out of sensory transducers correspond to stimulus intensity (steady-state). It is possible to separate the intensity of a sensory experience from the kind, so that sound and light can be compared (by higher-level systems) with respect to intensity. Intensity can be experienced in ANY sensory modality. This makes me think that primary sensory signals are available to awareness without passing through higher-level interpretations.

(Lubin to Marken --)

>You sound almost defensive. There is no reason to be.)

You haven't tried to submit articles on control theory to a mainstream publication in psychology.

I'm puzzled by the following:

>(There are no efferents from the brain into the retina in primates.
>There are significant efferents to the cochlea, so this is a poorer
>speculative choice for the above argument. Essentially the retina is on
>its own and no other brain area can manipulate its processing.)

I have thought of perception strictly as an afferent process. Likewise, I've thought of awareness strictly as a receiver. Of course higher levels can influence perceptual processes (although not in any published version of my model), but even if they don't, the incoming signals are still there. What do efferents have to do with the experiencing of perceptions?

Also, in my old Ranson and Clark, it says that 25% of the fibers in the human optic nerve carry signals outward. Is this wrong now?

>My function, if I am to have one, would be to propose likely
>computational constructs in order to ground the systems level
>descriptions. Its all got to be built from neural stuff -- boxes and
>arrows are only so convincing.

How about building the boxes from neural stuff? Then we'll all be happy.

>Is the imagination perhaps more dangerous (deceiving, labile,
>unconstrained, "runaway") than data?

No, imagination is a phenomenon that has to be explained by any valid neural model. It IS data. Rick's "paradox" can easily be transformed: any neural theory that says I can't imagine an "X" is wrong.

(Lubin to Powers --)

>Neural net modelers employ the term [transfer function] to stand for the
>function which takes input to a neuron and transforms it into output.
>This loosely corresponds to the input-current/output-voltage
>characteristic of real neurons.

I employ it similarly, but with reference to a function that requires a number of neurons to implement. In the model to which I refer, the input to the function is the error signal of a control system; the output becomes either a motor signal entering a muscle, producing muscle force and limb angular acceleration, or a signal entering a lower system as a reference signal (there are two levels in this model).

I'm curious -- why don't neural net modelers extend the concept of a transfer function so it spans all the way from input frequencies to output frequencies?

>Have you written this up?

No, I haven't. I will prepare a written description to bring along to the meeting.

We need a way to transmit diagrams on this network. Are there any programs anyone knows about that will work on ALL PC-type or workstation-type terminals? I know that transmitting 8-bit codes would allow us to exchange programs and graphics files, but also that some facilities don't seem to handle 8-bit-coded data. How about some suggestions from those with more network wisdom?

>I may not sound like it, but I do rely strongly on my intuition, which I >think is similar to saying "experience is the final arbiter."

It wasn't that kind of experience I meant, although I don't disgree with the use of intuition. I was referring to something more analogous to the experimental methods of science (and this is what Rick was talking about, too). Theory must eventually make a prediction, and the prediction must be cast in terms of something that a human being can experience. If the theory is about perception, it must lead to prediction of an experienced world that is just like the world we do in fact experience (which includes imagination). If the theory says we can't experience something that we actually do experience, then the theory is wrong, no matter how convincing it may be in other regards. It's in this sense that I mean that experience is the final arbiter. In deference to the season, we might also say it is the final exam that any theory has to pass. Internal consistency isn't enough.

>And also the data are rarely very constraining on what computations are >possible and impossible. Its like trying to write an article and >finding out you ONLY have the characters on your keyboard to work with.

But data never exist except in the context of a theory. I referred to S-R theory. The data show that incoming stimuli are connected to higher relay centers, which pass on the impulses to still other centers, until the impulses finally reach muscles, which contract as a result. Given the premise that you are trying to trace out S-R connections, it is possible to verify, by examination of neural circuits, that the required connections do in fact exist. That's data. Of course there are a lot of other connections, too, but because they don't fit the assumed model, they are dismissed as "collaterals" or simply ignored. The data, TOGETHER WITH THE THEORETICAL CONTEXT IN WHICH THE DATA ARE GIVEN MEANING, very much constrain the kinds of computations that will be considered as possible explanations of neural function -- and determine which equally valid or even more valid interpretations will not even be considered.

Another example is the digital interpretation of neural processes. McCullouch and Pitts assumed not only that single impulses are significant, but that the nervous system is clocked, so that coincidences could be considered in logical terms. The data, interpreted that way, would never lead to the concept that the very same neurons might be analog computers in which concidence of impulses is not required.

Welcome to the CSG, Joe. If we had any requirements for membership, you would have met them by now. Probably the main requirement is being able to hold your own end up in an argument, as you'll see in August.

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Date: Mon, 3 Jun 91 11:17:38 MEZ
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Peter Parzer <A5363GAD@AWIUNI11.BITNET>
Subject: Re: neural stuff: transfer functions: perception
In-Reply-To: Message of Sun,
2 Jun 91 20:29:35 -0600 from <powersd@TRAMP.COLORADO.EDU>

On Sun, 2 Jun 91 20:29:35 -0600 POWERS DENISON C said:
>
>We need a way to transmit diagrams on this network. Are there any
>programs anyone knows about that will work on ALL PC-type or workstation-
>type terminals? I know that transmitting 8-bit codes would allow us to
>exchange programs and graphics files, but also that some facilities don't
>seem to handle 8-bit-coded data. How about some suggestions from those
>with more network wisdom?
>
There is a public domain utility to transform 8-bit files to 7-bit files and back. The programs a called UUDECODE and UUENCODE. Another way is to use programs where you can define the plot in a simple ascii-character file. Two such public domain programs that run on almost all machines are Tex (LaTex, PicTex) and GNUplot.

Peter Parzer a5363gad@awiuni11.bitnet

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Date: Mon, 3 Jun 91 12:52:14 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: How do we know what to do?

Ed said (910602),

>They don't believe they have solved their conflict. They believe
>someone else has. When someone else solves our problems, WE BUILD
>NO CONFIDENCE WITHIN OURSELVES. WHAT WE DO BUILD IS A DEPENDENCE
>ON OTHERS. Juveniles have got to develop within themselves the
>belief THEY can resolve their conflicts. Parents who always rush
>to protect their children from having to deal with problems are
>really protecting their children from developing this badly needed
>self-confidence. If I solve my child's problems, am I really
>helping my child to grow? So it is not the elimination of the
>conflicts, but rather TEACHING OTHERS THE PROCESS BY WHICH THEY CAN
>DEVELOP A BELIEF IN THEMSELVES THAT THEY CAN SUCCEED BY RESOLVING
>CONFLICTS THROUGH THEIR OWN EFFORT.

Again I'm tempted to claim this as responsibility of teaching in general ie. the public schools included. There have been those who SAY that teaching others is teaching them HOW TO LEARN, where that is conceived to be some skills or whatever, apart from facts and figures. Ed concludes:

>They no longer blame their parents, their
>past, their feelings, or anything else outside of themselves. They
>deal only with themselves. Heck, that's what makes this stuff so
>darned exciting. Just think of the potential for restoring this
>human race to sanity, along with some help from Someone above
>(sorry, just had to put that in).

This raises two related questions for me, which might be begged in the above, I don't know. First, the process Ed and others go through when working with people assumes that each individual is worthwhile, important; what each has to say and contribute valid. However, do you acccept EVERY outcome of making them aware of their abilities to change and cope? In _Freedom from Stress_ for example, everything works out in the end (failure stories are probably not best sellers) but because the people involved met a couple of conditions (eg. 'want to change'). What about those who don't get to the point where they develop confidence? What conditions have you come up with through your experience that are necessary for the change you look for?

The second part of this how are changes evaluated? This seems to be one of the social-psychological difficulties to be dealt with. What if, as Gary asked a long time ago, some juvenile gets self-confidence and awareness, stops blaming others, gets in touch with himself, and then joins a drug gang? In other words, can you say someone like that has "successfully responded to treatment" in your terms? If not, then we return to the problem of someone or someones determining acceptable and not-acceptable outcomes/behaviors. Laissez faire (spelling Gary?) doesn't work well socially. It's the old problem of what does "freedom" mean. Do we let society bear the brunt of the responsibility for forming acceptibility? Is it mainly a family problem? Then what is a "family"? Does what's acceptible change with time (ie. morality)? Do we set up guidelines that stay rigid or change when convenient? Are these enough questions?

Joel Judd

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Date: Mon, 3 Jun 91 14:26:25 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Converted from PROFS to RFC822 format by PUMP V2.2X
From: "Bill CUNNINGHAM - ATCD-GI (804" <CUNNINGB%MON1@LEAV-EMH.ARMY.MIL>
Subject: HCT and collective vs individual perception

Bill Powers asks whether I am trying to apply HCT to an organizational process where each individual is operating on his/her own hierarchy.

The answer is yes, but.... The individual's hierarchy with respect to specific automated decision aids was where I was headed when I realized that HCT provided a very good model for how the information reaches the individual, and what the individual (more or less consciously) tries to do with the information upon receipt. So now I have several problems instead of one clean one. I, too, am seeking a good framework.

The individual(s) here receives input about the environment via their

sensory organs, but that input is a representation of what some other sensory organ (man or machine) has sensed and reported. The recipient's lower
levels have to efficiently process the information to a level where the conscious mind can consider the content and its meaning with respect to other inputs and render perceptions about the environment, albeit indirectly. We would like that process (I'll call it spinup) to be as efficient and as error free as possible, serving as sort of an impedance match. The individual can control his/her lower levels to improve receipt (say by squinting), but that in not what the individual would really like to do--control the source of the incoming information. This is done, by the way; but the feedback is frequently very slow with respect to whatever is being remotely observed--leading to sampling well below the Nyquist rate.

The organizatinal goal is to insure accurate and useful high level perception (certainly 8th or 9th level) in the mind of a single individual who can't be everywhere at once. So we create an organizational structure to abstract the information and to serve as an impedance match. That individual has learned the organizational foibles and insists on direct observation/interpretation when possible, and certainly where critical. The individual can reject the corporate perception in favor of a more direct one. HOWEVER, that same individual still can't be everywhere at once and some regions are inaccessible. So now, the individual (read topdog) must resort to some scheme that bypasses the "staff" and provides the remote sensory reports directly. Both the need to do this and the ability to do this depend significantly on how well automated decision aids work.

The HCT model really does fit the organizational behavior quite well in terms of feed-forward of perceptions. It fits well up to the point where a remote sensing is reported, and it fits well at the higher levels after the remote sensing reports have been received. I suspect this is true because humans have
unconsciously constructed a process to mirror their own perception hierarchy. After all, the organizational purpose is to generate a perception of an environment inaccessible to the topdog. The feedback part of the HCT model seems to fit the lower level and upper level segments, taken separately--although I suspect there is less conscious design at play. The breakdown occurs at the bridge between event reporting and relationship development, in that the human outperforms the organization by a wide margin. My instinct is try to fix the organizational/machine behavior so that it better fits the HCT model for individuals. That includes both feed-forward, feedback and the "spinup" process mentioned earlier.

So, does this apply to the CSG net? I think so. Despite the higher levels of perception carried out by the participants, we all get our sensory input from the computer screen or printout in front of us. Every one of us is operating in our own hierarchy, but we all are working from written reports (significantly, the SAME reports) and we all have to go through the same "spinup" process before we enter upper level perception. The question on providing graphics on the net is directly related to the the "spinup" problem. I don't understand HCT well enough to criticize in depth, but the shortfall I see is that the individual's perception of his/her environment is

based the individual's immediate (and personal) sensory input and references built up from previous perception. But the previous perception would include influence from external input (i.e. written reports from external sensors, etc.) So, to completely describe human perception, HCT should include an explanation of how an individual "spins up" from an external report to abstract reasoning that includes personal sensory input.

With respect to perception of motion, the idea of motion detection at two levels seems to have real merit. One of the (machine sensors) I have to deal with is doppler radar, which is fundementally sensitive to motion and is used to discriminate from those things not moving at the same rate. That's a machine--what about animals? Does anybody know whether a dolphin's sonar works as a doppler detector. A bat's? What happens as we vary the frequency of a strobe light on a fan? We can negate or reverse our perception of motion. Is this a matter of edge detection on the retina or a higher level comparison to a long stored reference? Camouflage is known to work on several principles, and defeat of edge detection is one of them. The first goal is to avoid notice (with respect to background). The second goal is to disguise what is noticed. That sounds like working against two levels of perception, one closely associated with sensor physics and one associated with higher level processing.

Bill Cunningham

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Date: Mon, 3 Jun 91 18:04:03 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject: levels of motion detection; consciousness; adaptive resonance

[From Joe Lubin (910603.1630)]

Bill Powers (910601.0800)

> >Visual motion and spatial information can be extracted computationally
> >without employing explicit configuration perception constructs (shape
> >representations).
>
> Yes, I agree that they CAN. I have even attempted to include some such
> backup position by saying that a given level of perception is drawn from
> signals of the next lower order of perception, OR signals below that
> level (signals that skip levels on the way up). Your statement, however,
> is a little ambiguous because the question is, ARE they so derived by the
> human brain? My position is that object-motion perception requires that
> there be something moving against a background, implying prior
> discrimination of the object from the background. Nothing can move in a
> uniform visual field: a white cat creeping across a snowfield.

Yes, I shoud have emphasized "CAN."

> Doesn't the same problem exist neurologically? There certainly are
> motion-sensitive processes very low in the visual system -- but there are
> also motion-sensitive processes at much higher levels, aren't there?

This is essentially correct. I take you as postulating two genres of motion detection: (i) motion derived from pure low-level motion detection cells (amacrines, retinal ganglion cells), and (ii) motion derived from higher level object descriptions.

After my assumption you made it explicit:
> Doesn't the same problem exist neurologically? There certainly are
> motion-sensitive processes very low in the visual system -- but there are
> also motion-sensitive processes at much higher levels, aren't there? What

> In the midst of establishing our agreements, I hope that on both sides we
> will also take pains to identify ambiguities, contradictions, and holes.
> I believe that research is really driven by questions, and that the
> questions represent areas where we are confused and contradict ourselves,
> or areas where we are working from assumptions that are leading us
> astray.

I have no qualms about dragging or being dragged in the muck.

> What we are all after is something pretty grand and inspiring (at
> least in my view): A unified conception of human systems and other
> organisms that is consistent all the way from the molecular level to the
> level of whole-organism behavior -- and that is consistent with all our
> other models of the world, including physics and chemistry and the world
> of direct experience.
> ...
> I hope we're together in holding this as our aim.

I wouldn't be so bold as to say that I'm trying to flesh out a GUT of this sort, but this does describe my orientation.

Bill Powers (910602)

> >... we only experience representations at levels higher than these
> >sensory transduction levels.
>
> I'm not sure this is true. Signals coming directly out of sensory
> transducers correspond to stimulus intensity (steady-state). It is
> possible to separate the intensity of a sensory experience from the kind,
> so that sound and light can be compared (by higher-level systems) with
> respect to intensity. Intensity can be experienced in ANY sensory
> modality. This makes me think that primary sensory signals are available
> to awareness without passing through higher-level interpretations.

There are pure intensity cells in the retina. Some of these project to the suprachismatic nucleus for regulation of diurnal

cycles.

In your last post (910601.0800) you said

> Doesn't the same problem exist neurologically? There certainly are
> motion-sensitive processes very low in the visual system -- but there are
> also motion-sensitive processes at much higher levels, aren't there? What
> this suggests to me is that I have left a hole in the hierarchy: that
> there is a very low-level kind of change-detection, which conforms to the
> physical relationship between velocity and position, and also a higher-
> level kind of change-detection, which is the one I have represented as
> the transition level. It may be that the lower-level one is not
>   -------------------------
> accessible to consciousness, which is one way of accounting for the fact
>   ---------------------------
> that I missed it because my method depended on consciously noticing
> experiences and identifying them as perceptions. Of course another way of

Are you trying to argue both sides?

What I said was that consciousness, as instantiated as an attentional process, can not directly access retinal representations because these are in the retina and there are no neural projections to the retina (in primates, at least). At the lateral geniculate nucleus (the next stop along the main visual processing line) the story is different. Here many different brain regions have direct access to the LGN signals both because they receive signals from it and because they can send signals to it (gain control would be a major part of such an attentional mechanism). The question of "Where is the seat of consciousness?" notwithstanding, it seems highly likely that attentional gain control as mediated by some neural substrate (like the reticular activation system) is at least partly responsible for what can and cannot enter consciousness.

Consciousness appears to have an element of seriality to it. One may access highly-parallel representations (imagery) but one has trouble accessing more than one type of representation concurrently. To me this implies a type of serial search (in your model, and in the models that I work with) or one-at-a-time access. Thus we may start conceiving of consciousness as a gain-controlled search process.

In contrast our retina performs an automatic computation which is independent (directly -- not counting hormones, temperature, Vitamin A, etc.) of any neural signals in the brain.

> I have thought of perception strictly as an afferent process.

I think of awareness, or at least sensory processing as a bottom-up and top-down "resonance" (in terms of Stephen Grossberg's Adaptive Resonance Theory).

> Likewise,

> I've thought of awareness strictly as a receiver. Of course higher levels
> can influence perceptual processes (although not in any published version
> of my model), but even if they don't, the incoming signals are still
> there. What do efferents have to do with the experiencing of perceptions?

Not efferents in the motor sense, but top-down processing (of
which there appears to be as much in cortex as bottom-up)
constrains what we can experience by employin top-down
representations to serve as expectancies (What I would term a
cognitive reference signal). Mismatches of bottom-up sensory
data with top-down expectations produce -- you guessed it --
error signals which can drive a serial search process to
reorganize the network. Such mismatches, as you know, can occur
at a variety of levels, in which case nonspecific reset signals
are sent where appropriate. This model is instantiated in a
neural network formalism which employs system of nonlinear
differential equations to define the neurons and their
connectivity. I (and a student) have taken this as far as
employing a model of neurotransmitter release and postsynaptic
uptake to drive the search process. More in Durango.

> Also, in my old Ranson and Clark, it says that 25% of the fibers in the
> human optic nerve carry signals outward. Is this wrong now?

I know at least some fish have projections into the retina, but
primates don't.

> How about building the boxes from neural stuff? Then we'll all be happy.

In my last post I was going to say something like this. Ideally
it would be nice if what I (we) arrived at could be drawn on a
viewgraph and laid under one of your old viewgraphs in perfect
correspondence (with perhaps a little prior fixing of dimensions).

> >Is the imagination perhaps more dangerous (deceiving, labile,
> >unconstrained, "runaway") than data?
>
> No, imagination is a phenomenon that has to be explained by any valid
> neural model. It IS data. Rick's "paradox" can easily be transformed: any
> neural theory that says I can't imagine an "X" is wrong.

What I was trying to get accross in my percerves manner was that
until such a paradox appears why bother worrying about it. It
is precisely this sort of "ooh, wow" philosophy that strikes me
as unconstructive, and perhaps destructive in the sense that if
psychologists and philosophers are going to address such issues,
it makes it difficult to strike a rapport with a neuroscientist.
In my view, good brain/mind philosophy explores, and is motivated
by neural data -- there is a tremendous amount of maneuvering room
even there.

> I'm curious -- why don't neural net modelers extend the concept of a
> transfer function so it spans all the way from input frequencies to
> output frequencies?

If I understand you, they do. The activity of a model neuron is often taken as the mean spike rate ranging from quiescent (zero) to rapid firing (positive maximum). Thus the transfer function relates some conglomeration of the input frequencies (eg. summation of the impinging signals) to the output frequency (a number in the range [0., MAX].

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Date: Mon, 3 Jun 91 17:31:36 cdt
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "McClelland,Kent" <MCCLEL@GRIN1.BITNET>
Subject: Hierarchical levels, intentions, and loop gain

[From Kent McClelland]

Recently I've been trying to think about how hierarchical control systems work, and to understand the notion of "loop gain."

In a hierarchical control system, not all of the systems at any one moment have the same level of activity. Somewhere in his voluminous exchange with Joe Lubin, Bill Powers talks about the necessity for modeling the way that systems not currently controlling perceptions get switched off. Even for systems that are active, activity at one level must often imply a certain passivity at lower levels (and perhaps higher levels). For example, in order to maintain relative constancy in perceptions at one hierarchical level, say the perception of going from point A to point B, the hierarchical system must allow reference values for lower level perceptions (say, variations the "feel" of the path or other disturbances encountered) to fluctuate freely in order to find a perceptual match for whatever experiences might appear along the way. In his article on "Degrees of Freedom in Social Interactions," Bill makes a similar point: "A successful control system adjusts its actions to oppose external circumstances that would tend to disturb the controlled quantity--which means that it must allow external circumstances, to a great degree, to control its actions, its selection of lower-level goals" (p. 228 in his 1989 collection, Living Control Systems).

Does it make any sense to make a distinction between reference values that are relatively fixed (so that all disturbances are actively opposed) and others that are relatively fluid or changeable (in a passively take-whatever-comes along sense)? Could this distinction have something to do with the difference between intentional or willed actions and actions which merely happen without conscious intention? In other words, does intention equate to fixity of the reference value (at the highest hierarchical level defining the

action)? As my use of the word 'conscious' suggests, I'm also wondering if not only INtention but ATtention may be involved. Could the spotlight of attention be normally focused on the hierarchical level where reference values are most fixed at the moment? (Or perhaps the level right above this one?)

While I'm speculating, let me ask a few questions about "loop gain." My understanding of this concept is rudimentary to say the least, but I have the impression that this somehow relates to a system's sensitivity in opposing disturbances, or in other words, closeness of control. (Is that right?) One question, then, is how do control systems at different levels of a hierarchy differ in loop gain? I know that lower-level systems act more rapidly, because the higher-level systems depend on a summation of lower-level signals. But do the lower-level systems also have greater loop gain (closer control)? Or is loop gain a variable property of the system, changing perhaps when the system switches from active mode with relatively fixed reference levels to passive mode with relatively free-floating reference levels as suggested above? And how, if at all, does loop gain change as a result of reorganization?

My suspicion as a sociologist is that a good many of the high-level system concepts we have for social entities such as social groups or organizations or society are pretty loosely controlled. Our mental models of these things (even "scientific" models presented in the sociological literature) are just too crude to match very closely with the complexity of the environmental phenomena to be perceived (whatever they might be). And the notion that an individual's actions can do anything much to counteract the overwhelming disturbances these "entities" might present us seems a bit ludicrous, at least for entities like society. Hence my guess that gain goes down as hierarchical level goes up.

Can someone help me sort this out?

Kent

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Date: Mon, 3 Jun 91 17:01:29 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Artificial

[From Bill Powers, 910603]

For Joe Lubin and other modelers on the net: preliminary version of a paper on the transfer-function method of simulating control systems.

An "Artificial Cerebellum" as an adaptive transfer function

DRAFT

This somewhat pretentious title is partly justified by the generally understood function of the cerebellum as a stabilizer,

as well as coordinator, of motor behavior, and also by some resemblances of architecture in the cerebellar cortex to a block diagram of the algorithm to be described here.

The algorithm is a method for dynamically constructing a transfer function to be used as the output part of a control system. The transfer function converts an error signal into an output of the control system; this output might directly operate an actuator, or it might serve as a lower-order reference signal in a hierarchical control model. In the present example, both possibilities are included.

In general, a control system using a simple proportional sensor and comparator must have an output function of the proper dynamical design in order to stabilize control of an external load. When the load characteristics involve accelerations and velocities, and are nonlinear, optimizing the system by normal means can be extremely complex, and requires solving the system equations by numerical approximation.

The method to be described here is derived from the well-known superposition theorem of control-system engineering, and involves both a convolution (during application of the transfer function) and a deconvolution (for correction of the transfer function). There are several novel features in the implementation of this general procedure. First, the computation method is realizable as real-time analog processing in a nervous system, and second, only information in the error signal is used for correction of the transfer function. Also, the transfer function that is derived is non-analytical -- that is, its form does not have to match any known analytical form. This system can therefore adapt itself to a variety of loads without either a teacher or any direct knowledge of the nature of the load. It learns by practice.

The generality of this method is not known. It has been tested, and is presented, as a method for stabilizing a simulated two-joint robot arm working in the vertical plane only, actuated by torques at the two joints.

Convolution

A device receiving a continuous input waveform g(t) and emitting a continuous output waveform h(t) can be represented as a transfer function f(tau), where f(tau) is the impulse response of the system.

The impulse response is the response of the device to a brief input impulse having zero duration, infinite amplitude, and an area proportional to the value of the real input at a given instant. In most physical systems with a natural steady-state condition, the response to an impulse will be a rise of the output to some maximum followed by a slower decay back to zero, with or without oscillations about zero. An example would be the response of a damped pendulum to a tap with a hammer. The shape of this rise and fall curve is the impulse response curve of the

system. It may or may not be analytically representable. This analysis requires the assumption that the device is linear.

If the varying input waveform is divided into segments of very short but not zero duration, the value of the input impulse is closely approximated by the average value of the waveform during each segment. Any one of these segments can be considered an input impulse of a given magnitude (and sign). Following such an inpulse, the output of the system would follow a time-course that is the response for an input impulse of that magnitude and sign.

The varying input waveform can be treated as a sequence of impulses that vary in magnitude and sign. At any given moment, the value of the output of the device consists of the sum of all responses to impulses that have occurred from the present time back to the earliest previous time at which an input impulse would have left any lingering traces at the output. This is why the term "superposition" is used. The present value of output is considered to be the sum of many superimposed impulse responses that began at increasingly earlier times.

If the shape of the impulse-response is given as f(tau), the output h(t) is found from the input g(t) by

h(t) = integral[g(t - tau)f(tau)] from tau = 0 to infinity. This calculation is called "convolution."

Given knowledge of the function f(tau) for a physical device, the output of the device as a function of time can be calculated from the input by using the above equation. In practice the integration is done by summation over arrays of numbers.

Precursor applications

Prior to the use of this principle in constructing an adaptive control system model, it was used as a way of finding transfer functions for a human being in a tracking task. A control-system model was set up using the convolution calculation in its output function. The model was subjected to the same conditions as the human being stabilizing a cursor against random disturbances by using a joystick. The output of the model, the stimulated handle position, was compared against the output of the person, the actual handle position. The error of prediction was then used to correct the transfer function in the model.

The correction method was extremely simple. First, the expected handle position at time t was generated by convolution of the model's error signal with f(tau), where f(tau) was a table that was initially set to all zeros. The expected position was subtracted from the actual handle position at that time to generate the prediction error. For each value of tau, the prediction error was multiplied by the model's error signal tau units of time in the past, the result (divided by a scaling factor) being added to the tau-th entry in the transfer function table. For each value of tau, the correction to f(tau) was

computed over all data points from an experimental run.

Repeated iterations of this prediction-correction cycle gradually altered the shape of the f(tau) table. The result was that the model's behavior converged on the actual behavior while the f(tau) curve approached a final form more and more slowly. This form was taken to be the impulse response of the human being, or a linear approximation to it. Correlations between the model's handle movements and the real ones were consistently above 0.99 at the completion of the process.

More or less accidentally, I realized that the same principle could be used in a self-contained adaptive system, where the "error" being corrected by adjusting the transfer function's shape was also the error signal in a control system.

The complete algorithm

1. Computing the convolution.

Let e(t) be the error signal in a control system, o(t) be the output signal, and f(tau) be the transfer function generating o(t) out of e(t). Thus, in discrete form,

$$o(t) = \text{sum over tau } [e(t - \text{tau})f(\text{tau})].$$

The basic task is to find f(tau) such that the behavior of the simulated control system, with an external physical load, becomes stable and the error signal is minimized.

In a model of human arm movement control, the span of tau needs to be only a short time. With a time resolution of 0.01 seconds, 90 entries in the f(tau) table will suffice.

The function e(t - tau) is created in a shift register. Ninety past values of the error signal are saved. On each iteration, the table entries are shifted by one, vacating the first entry where the current value of error signal is entered. The last entry is discarded. The transfer function f(tau) is stored in a parallel table, also 90 units long. The value of the output is computed by summing the products of each entry in the error-signal table and the corresponding entry in the f(tau) table.

Suppose that the error signal in the physical system were carried by a neural pathway traversing a number of synapses in passing. The speed of conduction along this pathway determines the time delay represented by the distance between successive synapses. Further, suppose that in the dendrites receiving signals at each synaptic junction, there were a way of adjusting the weighting of the transmitted signal. The weightings would correspond to f(tau). Finally, if the signals entering all the synapses were summed (or if outputs from corresponding neurons were summed) to produce a frequency-modulated output, we would have an approximation to the convolution operation. Now the transfer function represented by this entire neural assemblage would be

adjustable through altering the weightings of each input through the dendrites.

An arrangement very similar to this is found in the cerebellar cortex: the error signal would correspond to signals in the parallel fibers, which synapse in passing with dendrites of a lineal array of Purkinje cells. The outputs of a row of Purkinje cells converge to nuclei below the cerebellar cortex, where summation could take place. The outputs from the nuclei would be the output signals of the control systems.

2. The method of correction

During adaptation, the control system must either be given a varying reference signal or be subject to a varying external disturbance, or both. In the simulations, a smoothed random variation in reference signal is used.

For each entry in the f(tau) table, on each iteration of the algorithm, the value of f(tau) is changed by an amount proportional to e(t)*e(t - tau). On any one iteration, the amount of change must be very small, so that f(tau) is allowed to change only very slowly. This correction method suffices to alter f(tau) in the way required to minimize the error signal e. Because the error signal is affected by external feedback from the output of the system (this being a closed-loop control system), each change in f(tau) alters the behavior of the error signal; the behavior of the error signal, in turn affects not only the output of the system but the form of f(tau).

I believe it can be shown rigorously that if the alterations of f(tau) are sufficiently slow, this process will converge to a steady-state condition in which f(tau) approaches a final form and the error signal fluctuations are minimized. In practice this is what happens. The whole system does not achieve perfect stability, but neither does the human system being modeled. It remains to be seen whether this model reaches a state with imperfections that are the same as the imperfections in similar real behavior. The ultimate test will require a model of human arm behavior in which muscle nonlinearities, stretch and tendon feedback, and joint-angle effects are taken into account, a project which is approaching completion.

To implement the correction method neurally, it is necessary for a copy of the same error signal that passes along the parallel fibers to reach all the Purkinje cells at the same time, undelayed. These copies of the error signal should alter the Purkinje cells' dendritic sensitivity to stimulation by the parallel fibers. The climbing fibers or the mossy fibers, which reach all Purkinje cells in parallel, may be the appropriate candidates for the required undelayed error signals -- if it can be shown that the signals they carry originate from the same source that feeds the parallel fibers, and that these are in fact error signals in a neuromuscular control system.

The alteration in sensitivity to stimulation must go approximately as the product of e(t)*e(t - tau). In other words, the sensitivity must be altered according both to the amount and sign of error signal directly reaching the dendrite, and the amount and sign reaching the same place through a delayed path. This kind of signal interaction is known to occur; it has been proposed as the neurochemical basis for classical conditioning.

If the corrections in the model are applied in a completely cumulative manner, the system as a whole at first becomes more and more stable, but with passage of sufficient time it develops oscillations. To prevent these oscillations, each value of f(tau) is subjected to a slow exponential decay toward zero over time. It is not presently known whether the eventual appearance of oscillations is a purely computational problem (integer arithmetic is used in the computer simulations) or whether it is inherent in the correction method. In either case, introducing a gradual decay in the f(tau) values cures this defect. By adjusting the amount of correction allowed on each iteration and the rate of decay of f(tau), it is possible to make the process of adaptation go quickly or slowly. The system reaches a higher degree of stability when the decay is slow. The exact effect of this tradeoff will not be known until the reason for the instabilities that eventually occur without any decay of f(tau) is understood.

Behavior of the simulated system

The simulation in question is a model of an arm operating in the vertical plane. At the elbow and shoulder joints, torques are applied. These torques are the outputs of the first level of control systems for each joint. The torques produce angular accelerations of the arm masses about their joints, the equations of motion being the ones normally used in the analysis of robot arm movements. The feedback signal is derived from the angular velocity of the joint (physiologically it could arise from rate-sensitive stretch receptors in the muscles). At the next level of control for each joint, the feedback signal corresponds to joint angle (for which receptors exist). The output signal from these level-2 position control systems sets the reference signal for the velocity-control systems of level 1. Random variations in the reference signals given to the level-2 systems provide the required variations in error signals at both levels. The f(tau) tables in all four control systems are set initially to zero. Each control system employs an independent version of the algorithm in its output function.

At first, the arm dangles in the direction of gravity. As time passes, the arm begins to move slowly, then more and more rapidly. It is initially very unstable; it oscillates in several modes, undershooting and overshooting the continuously-changing target positions. Eventually both joint angles follow the varying positions specified by the varying reference signals with a lag of perhaps 0.1 second, which is actually somewhat faster than a real human arm can respond. The dimensions, masses, and moments

of inertia in the model approximate those in a real arm, so the performance of this model is reasonably close to reality. Switching gravity on and off shows that the system is quite stable with respect to external disturbances.

Comments and conclusions

There are stabilizing mechanisms in the human arm control systems that are built into the spinal reflexes, and do not require cerebellar intervention. However, these mechanisms adapt only very slowly if at all, and they do not appear capable of achieving high degrees of stability under conditions of varying external loads -- especially loads involving masses and springs.

The present model essentially ignores these built-in stabilizing mechanisms and throws the entire burden of achieving stability onto the "artificial cerebellum" model. If the model were revised to include the existing stabilization factors, it would work even better, because the "cerebellar" mechanism would be required to inject much smaller corrections. One can visualize a "mainstream" hierarchy of control in the central nervous system that, save for the problem of stability, can achieve neuromotor control out of its own structure. The cerebellum, if it actually incorporates the adaptive mechanism envisioned here, would simply monitor error signals in the main hierarchy and construct transfer functions that would add signals to the outputs of the central control systems, continually trimming the dynamic responses of these systems so as to maintain dynamic stability.

In closing, I should remark that the success of this method probably depends heavily on the fact that we are dealing with a closed-loop system. If the loop were not closed, so the output of the transfer function did not affect the input to that function through an external loop, there could be no criterion for "success" except whatever an external teacher chose to impose. In the present case, the only criterion is the natural one for a control system: minimum error signal inside the control system itself. The stability of behavior, as seen by an external observer, is just a side-effect of achieving this goal. From the point of view of the system itself, the objective is to keep the error signal as small as possible -- not to behave in any particular way.

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Date: Tue, 4 Jun 91 10:08:08 SST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <ISSCCC@NUSVM.BITNET>
Subject: Re: Artificial
In-Reply-To: Message of Mon,
3 Jun 91 17:01:29 -0600 from <powersd@TRAMP.COLORADO.EDU>

(from Chung-Chih Chen)

Bill Powers:
I read your draft about "artificial cerebellum".

I think your algorithm is an LMS (least-mean-square) learning.
LMS algorithm is quaranteed to converge to the global minimum as long
as the learning rate is small enough. It is an one-layer neural network
widely used since 1960. The weight decay is also used in neural networks.
The basic equation to change the weight using LMS is:

w(t+1)-w(t)=l*e(t)*i(t)

where w(t) is the weight (your f(tau)); l is the learning rate;
e(t) is the error of the actual output and the desired output
(the same as your e(t)); i(t) is the input (your e(t-tau)).

Regards,

Chung-Chih Chen

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Date: Tue, 4 Jun 91 08:29:17 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Converted from PROFS to RFC822 format by PUMP V2.2X
From: "Bill CUNNINGHAM - ATCD-GI (804" <CUNNINGB%MON1@LEAV-EMH.ARMY.MIL>
Subject: mailing graphs

*** Forwarding note from EGC --CMSNAMES 06/03/91 18:37 ***
Received: from s850.mwc.edu by LEAV-EMH.ARMY.MIL (IBM VM SMTP R1.2.1) with TCP;
Received: by s850.mwc.edu
(15.11/25) id AA20068; Mon, 3 Jun 91 17:39:58 edt
From: Eric Cunningham <ecunning@s850.mwc.edu>
Subject: mailing graphs
To: cunningb%mon1@leav-emh.army.mil (W.B. Cunningham)
Date: Mon, 3 Jun 91 17:39:55 EDT

Mailer: Elm [revision: 64.9]

You can mail graphs through profs or any ascii
mail system. There is only two catches.
1)
You must have access to a drawing program that
can print to PostScript printers (and thus
the PostScipt to a file).
2)
The receiver needs to have a PostScript Laser
printer (the laser is not needed but that is
really the only printer that has a PostScript
translator.

Explaination:

PostScript is a language for laser printers, just like
FORTRAN is a langauage for computers. If a printer has
a translator (thus it is called a Postscript printer) it
will read the ascii code and translate it on the fly to
form a drawing, graph, or whatever. There are tons of

PostScript files out on Internet that can by printed
just by sending them to the printer. In DOS the command
would be copy filename prn. It is that simple, plus the
file is straight ascii until translated. There are now
programs out there that will translate PostScript code
AND PRINT ON NON POSTSCRIPT PRINTERS.

Eric Cunningham ecunning@s850.mwc.edu

Home: (703)-372-3722
435 Greenbrier Court Work: (703)-898-7555
Fredericksburg
VA, 22401

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Date: Tue, 4 Jun 91 07:07:22 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: LMS algorithm

[From Bill Powers]

Chung-Chi Chen (910603) --

Thanks for the comment -- I don't mind re-inventing the wheel, as long as it turns out to be round and not square. But I'm going to need a little help in understanding your comment.

>The basic equation to change the weight using LMS is:

>w(t+1)-w(t)=l*e(t)*i(t)

>where w(t) is the weight (your f(tau)); l is the learning rate;
>e(t) is the error of the actual output and the desired output
>(the same as your e(t)); i(t) is the input (your e(t-tau)).

My problem is in seeing how t-tau is equivalent to t. In my algorithm, the correction of f(tau) for a given tau involves the product of the PRESENT error (at time t) and the input at a PAST time (t - tau). The LMS algorithm you wrote seems to involve the product of e at the present time t and i at the SAME present time t.

My "proof" of the convergence involved expanding the method into a series of equations of the form

$$y(t) - y^*(t) = f(0)*e(t - 0) + f(1)*e(t - 1) + ... f(T)*e(t - T)$$

where T is the maximum value of tau in f(tau),
y(t) is the predicted output signal,
y*(t) is the actual output signal, and.
e(t) is the input signal common to the model and the reality.

There is one such equation for each data point beyond the T-th point.

What we get is a highly overdetermined set of equations, as long as the number of data points is greater than the maximum lag, T. My method is then a method of steep descent which is guaranteed to minimize the sum of (y - y*)^2.

I first used this general method in the 1960s as a way of sharpening optical images, when I was pretending to be an astronomical engineer.

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Date: Tue, 4 Jun 91 10:42:48 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: levels of motion detection; consciousness; adaptive resonance

[From Rick Marken]

Joe Lubin (910603) says:

> >Is the imagination perhaps more dangerous (deceiving, labile,
> >unconstrained, "runaway") than data?

Bill Powers replies:

> No, imagination is a phenomenon that has to be explained by any valid
> neural model. It IS data. Rick's "paradox" can easily be transformed: any
> neural theory that says I can't imagine an "X" is wrong.

And Joe responds:

>What I was trying to get accross in my percerves manner was that
>until such a paradox appears why bother worrying about it. It
>is precisely this sort of "ooh, wow" philosophy that strikes me
>as unconstructive, and perhaps destructive in the sense that if
>psychologists and philosophers are going to address such issues,
>it makes it difficult to strike a rapport with a neuroscientist.
>In my view, good brain/mind philosophy explores, and is motivated
>by neural data -- there is a tremendous amount of maneuvering room
>even there.

The paradox was just for fun. It sounds like you took it as a somewhat disparaging remark about neuroscientists. It was certainly not meant to be; some of my best friends are neuroscentists. The point of the paradox was simply to say that neural grounding is not nessarily the best strategy for understanding the phenomenon of control. Indeed, my impression is that these "groundings" are often motivated by preconceptions about the nature of the phenomena that are to be explained. Much of the work that I have seen in neural modeling, neural networks, etc is based on the idea that the phenomenon to be explained is how outputs are caused, planned, or end up occuring as a result of inputs from the environment or higher levels of the nervous system. As Bill noted in his earlier post, neuroscientists tend to find what they are looking for. You don't need "ooh, wow" philosophy (like my paradox) to see that neuroscientists have already claimed to have discovered "facts" about the behavior of the nervous system which would obviate the possibility of purposful behavior (control) as the phenomenon is understood by control theorists.

I think that the neuroscience findings are extremly important and interesting. But the neuroscience should serve the behavioral modelling -- by making it feasible (neurologically) and, possibly, by suggesting mechanisms.
But a discovery in neuroscience would never (even if it implied it) lead me to reject the existence of a behavioral phenomenon, such as control, until I had done some serious tests of the behavioral implications of the findings. That's all that the paradox meant to say. I think you might disagree with this.
If so, that's ok (I'm not defensive, really -- maybe a tad offensive). But I would be curious to know if there is an example of a neuroscience finding that has led to the reconsideration of the existence of a behavioral (or experiential) phenomenon. Did a neuro finding ever suggest the existence of a behavioral or experiential phenomenon that was never suspected before the neuro findings but then shown to exist? I can think of examples where things worked the other way -- from experience/perception to neurology. A great example is Mach bands -- the existence of this phenomenon, I believe, led directly to the kind of research that produced the discovery of lateral inhibition in the retina.

For all I know, there might be examples of neuro findings leading to behavioral/experiential findings. I just can't think of any myself -- maybe it's my bias.

Kent McClelland (910603) says:

>Recently I've been trying to think about how hierarchical control systems
>work, and to understand the notion of "loop gain."

I think that my "spreadsheet hierarchy" model might help you understand how a hierarchical control system works (it will certainly show THAT is works). I can send you a copy if you are interested (I have versions for the PC in Lotus and for the Mac in Excel). If you want a copy, send me a self addressed disk mailer specifying what you'd like, and the type of disk. Or you can get a copy from me if you come to the CSG meeting.

Best regards

****

Richard S. Marken USMail: 10459 Holman Ave
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Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)

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Date: Tue, 4 Jun 91 15:53:26 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject: consciousness and time constants; psych/neuro interface

[From Joe Lubin (910604.1500)]

Kent McClelland (910603)

> In a hierarchical control system, not all of the systems at any one moment
> have the same level of activity. Somewhere in his voluminous exchange with
> Joe Lubin, Bill Powers talks about the necessity for modeling the way that
> systems not currently controlling perceptions get switched off.

I think he was refering to systems whose control is no longer
effective or useful: these must be taken offline somehow (either
passively or actively).

> Does it make any sense to make a distinction between reference values
> that are relatively fixed (so that all disturbances are actively opposed)
> and others that are relatively fluid or changeable (in a passively
> take-whatever-comes along sense)? Could this distinction have
> something to do with the difference between intentional or willed
> actions and actions which merely happen without conscious intention?
> In other words, does intention equate to fixity of the reference value
> (at the highest hierarchical level defining the action)? As my use of
> the word 'conscious' suggests, I'm also wondering if not only INtention
> but ATtention may be involved. Could the spotlight of attention be
> normally focused on the hierarchical level where reference values are
> most fixed at the moment? (Or perhaps the level right above this one?)

Relative fixity or fluidity would be controlled by time constants
in the equations describing the systems. At the higher levels
the systems would be as "fluid" as the lower-level systems, but
the time scale on which they express their fluidity is
necessarily larger. There is no inherent discrete
difference between relatively fixed levels and relatively fluid
levels, to my mind, that might be useful in constructing a
mechanism for consciousness or attention, but your intuition may
be right when you consider that in order for an attentional
system to operate it must have a finite window within which to
sample activity and make determinations on what representations
to promote. In this sense, your distinction would be valuable in
determining what levels can be affected by such a mechanism and
what levels move too fast.

Rick Marken (910604)

> But a discovery in neuroscience would never (even if it implied it)
> lead me to reject the existence of a behavioral phenomenon, such as
> control, until I had done some serious tests of the behavioral
> implications of the findings. That's all that the paradox meant to
> say. I think you might disagree with this.

A neuroscientific discovery should motivate reflection on

relevant behavioral phenomena (and vice versa). I agree with the
spirit of the paradox if not taken to extremes.

> Did a neuro finding ever suggest the existence of a behavioral or
> experiential phenomenon that was never suspected before the neuro
> findings but then shown to exist? I can think of

I can't think of one, but (defensively) you have to realize that
the field is relatively young and its relatively difficult to do
a neuro experiment than it is to do a psychological one. And
also, and you and Bill have pointed out, neuroscientists are
often looking for something. And these somethings are usually
constrained by psychological or behavioral observations.

There are many examples that neuroscience has elucidated that
could not be grasped by psychology alone:
To my knowledge, nobody had any clue that the visual system might
be divided into a where (dorsal) system and a what (ventral)
system. Psychologically, I can't imagine an experiment prcise
enough to elucidate this fact.
And who would have expected that the left side of the brain
controls the right side of the body?
Or that severing the corpus callosum could render a single person
into something approximating two people in the same body and
head. This latter case involved doing the neuroscience first and
then coming to conclusions based on some ingenious psychological
testing (Sperry, Gazzaniga). This is a case where the
neuroscientific level was not the appropriate level on which to
construct explanations.

Joseph Lubin   jmlubin@phoenix.princeton.edu
Civil Eng. Dept.   609-799-0670
Princeton University   609-258-4598
Princeton NJ 08544

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Date: Wed, 5 Jun 91 09:46:00 SST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <ISSCCC@NUSVM.BITNET>
Subject: Re: LMS algorithm
In-Reply-To: Message of Tue,
              4 Jun 91 07:07:22 -0600 from <powersd@TRAMP.COLORADO.EDU>

Bill Powers:

I think the equation should be:

y(t)=f(0)*e(t)+f(1)*e(t-1)+...
(not y(t)-y*(t)=...)

The novelty in your algorithm is that the inputs are shifting each time.

And the inputs are exactly the past errors (e(t), e(t-1), ...).
Intuitively since the inputs shift only one each time, the inputs at t
and the inputs at t+1 are quite similar. From the viewpoint of neural networks,
similar inputs produce similar outputs. I think this is why your algorithm
can converge: Although the inputs are not the same each time, they don't differ
very much.

Chung-Chih CHen

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Date: Tue, 4 Jun 91 20:12:44 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Graphics; Behavioral ID of perceptions

[From Bill Powers (910604.1900)]

Bill Cunningham (910604) --

Postscript is a good solution for those with a laser printer and an appropriate drawing program, but all that's beyond my system. I've tried uuencode, and it expands a file size about 50 percent. If I ZIP the (executable) file first, then uuencode it, it comes out about 5/6 the size of the original .exe file. (uuencode and then ZIP comes out a little larger). But we're still talking files of 65K to 100K (if I send demo programs). For diagrams PIC files (PC Paintbrush) will be much smaller, especially if we use the minimum size needed for a simple diagram. Of course that leaves the problem of screen graphics compatibility.

Another solution is a shareware drawing program written by Pat Williams, Greg William's wife (Greg is the CSG publisher). It's called "PictureThis." It's a poor man's Postscript drawing program, very powerful. It will output postscript files, but will also save screens in its own format so any other PictureThis program can read them. It needs a CGA screen or a Hercules screen (although I believe the next release will have EGA and VGA capabilities). This is, of course, strictly PC world stuff. However, even underprivileged people who work on workstations or mainframes can probably get access to a PC these days.

Greg's address is Rt. 1, Box 302, Gravel Switch, KY 40328.

Joe Lubin (910604) --

In your remarks to Rick Marken you say

>To my knowledge, nobody had any clue that the visual system might be
>divided into a where (dorsal) system and a what (ventral) system.
>Psychologically, I can't imagine an experiment prcise enough to
>elucidate this fact.

Which fact? -- the fact that people perceive (and control) visual variables in the "where" and the "what" categories, or the fact that these perceptions are located in the dorsal and ventral parts of the

visual system? If the former, we have demonstrated (starting 35 years ago) perception and control of a much larger assortment of visual variables. In all these cases the means of control was a control handle or a mouse.

Intensity: How Much. Task: maintain constant brightness of a patch on the computer screen with disturbances tending to alter the brightness.

Sensation:
 What Quality. Task: maintain a patch on the screen at "orange" while disturbances tend to alter the color. (in the auditory modality, maintain a constant pitch while disturbances tend to change it).

Configuration:
 What Shape. Task: maintain an arbitrary figure composed of linked straight lines in a particular shape.
 What Size (linear). Task: maintain a circle at constant diameter while disturbances tend to make it shrink or grow.
 What Size (area). Task: maintain a constant apparent size while disturbances alter the shape and linear size of a rectangle.
 What Orientation: Task: keep a triangle's apex pointing upward.
(Auditory mode)
 What interval: Task: maintain a major third interval by varying one tone while the other one changes pitch (like singing in harmony).

Transition:
 What Rate of Spin. Task: maintain a triangle spinning at a constant rate around its center.
 What Rate of Shrinking or Expansion. Task: Keep a circle contracting or expanding at a constant speed (the circle snaps to the opposite extreme when it reaches a limit).
 What rate of flow. Task: keep a "waterfall" of random dots falling down the screen at a constant velocity. (This control task has also been used to measure the motion aftereffect quantitatively).

Event: No good experiments, although I believe Rick is working on some.

Relationship:
 Where in relationship: Task: keep movable cursor on, above, below, left of, right of, target. In two dimensions, keep movable dot inside circle, outside circle, on circle, centered in circle.

 Spatiotemporal relationships. Task: Keep dot moving toward target. Keep dot that moves around periphery of circle on circle. Keep one shape matching another (changing) shape (i.e., figure changes from tall diamond through square to wide diamond). Keep one line moving antisymmetrically to another line.

 Kinesthetic-visual relationships. Task: maintain tracking while sign of handle effect on cursor periodically reverses.

Category:
 What class. Task: Answer question by making disturbed cursor pick answer out of a list of numbers or phrases (disturbance requires different handle positions to repeat same answer; same handle position can be

required to point to different answers).

What numerical class. Task: keep a displayed number at "50" while disturbances alter it.

What verbal class. Task: Keep the displayed name of a U.S. President at "Chester B. Arthur" while a disturbance alters which president is displayed from a chronological list (A killer! But it can be done).

Programs, Principles, System Concepts.
No good experiments yet, but again I believe Rick is working on it.

In all of the above, where I got lazy and didn't mention the disturbance, there was always a continuously-varying disturbance tending to alter the state of the controlled variable. In all these cases we have observed people (and been the people) controlling variables of the respective types, and either measured the parameters of control or set up the experiment so they could have been measured if we had wanted to. Rick Marken and Tom Bourbon may think of some examples I've left out.

These control system experiments demonstrate, if you accept the basic premises of the model, that perceptions of all these kinds exist in the brain as neural signals. We can (or in some cases, could) tell you the parameters of the control systems that control perceptions of each kind. We don't know where in the brain these signals are to be found, or in what form, but we can tell you that they must be there.

This is how "psychological" experiments can reveal details of perceptual processes even though we don't know how they are computed, or where in the brain these computations take place. We are also, of course, building up information about hierarchical relationships, but we haven't done this systematically enough to draw any conclusions. In a piecemeal fashion, at any rate, we are gradually accumulating a list of perceptions that have been proven controllable. This is also proof that signals representing them must exist in the brain.

I hope that this kind of information, at the block-diagram level, can be of use to neuroscientists as they explore the brain trying to understand what the signals they encounter at the component level mean. I venture to guess that not many neuroscientists have used this method to test their definitions of correlates of neural signals.

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Date: Wed, 5 Jun 91 07:50:32 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: More on f(tau) algorithm

[From Bill Powers (910605.0730)]

Chung-Chih Chen (910604) --

Yes, you're right. I was writing something from memory instead of working it out again.

Here's how I set up the method to make a model's behavior converge toward real data from a tracking experiment:

 (Assume compensatory tracking, desired cursor position = 0)

Let c   = model's cursor position
    h   = model's handle position
    h'   = observed handle position
    d   = disturbance applied during real run and model run
    z   = delay time (I can't transmit "tau")
    f(z) = the transfer function.
    e   = prediction error

Set f(z) to all zeros.

For all values of t from t+z to tmax do:
{
 Compute model handle position from control-system model:
   c(t) = h(t) + d(t)
   h(t) = SUM[c(t-z)*f(z)] over all z
 Compute prediction error e:
   e(t) = h'(t) - h(t)
 For all z, make correction:
   f(z) = f(z) + e(t)*c(t-z)*k, where k is a small fraction.
}

The entire procedure above can be repeated again and again until f(z) attains a final form or blows up. Putting a very slow decay into f(z) will keep it from blowing up. I use integer arithmetic: maybe with real numbers, k can be made small enough so the decay isn't needed.

I'll leave it to you to prove that this process converges!

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Date: Wed, 5 Jun 91 10:55:41 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject: what/where vision; artificial cerebellum

[From Joe Lubin (910605.1100)]

Bill Powers -- Neural Net Modeler (910604.1900)

> >To my knowledge, nobody had any clue that the visual system might be
> >divided into a where (dorsal) system and a what (ventral) system.
> >Psychologically, I can't imagine an experiment prcise enough to
> >elucidate this fact.
>
> Which fact? -- the fact that people perceive (and control) visual
> variables in the "where" and the "what" categories, or the fact that
> these perceptions are located in the dorsal and ventral parts of the
> visual system? If the former, we have demonstrated (starting 35 years
> ago) perception and control of a much larger assortment of visual
> variables. In all these cases the means of control was a control handle

> or a mouse.

The latter fact. This can be thought to be a carry-over from neuroscience into psychological thinking. It is clear that variables in these two classes can be controlled individually, but I am not sure there have been psychological experiments that have explicitly demonstrated the separation of the two systems per se (eg. as some sort of disassociation phenonmenon.) There is some neuropsychological evidence. I once read about a women with damage to the dorsal system (MT maybe?) who could not perceive motion. She would perceive a series of snapshots, sort of. I remember something about her having difficulty crossing the street. This kind of selective impairment argues for a functional separation (to a degree) of the two systems, in a way that selective control does not.

Thank you for the catalog. It is and will be very useful for "grounding" me in PCT.

I read quickly through your Artificial Cerebellum paper. It strikes me as very similar to Albus' CMAC from what I remember of it, as well as Widrow and Hoff's LMS. The CMAC, I believe, employed similar error signals to make weight adjustments. I will, over the next few days, review the CMAC, the PAC (Powers' Artificial Cerebellum) and David Marr's model, and let you know what I come up with.

Joseph Lubin
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Date: Wed, 5 Jun 91 17:22:31 +0200
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Oded Maler <Oded.Maler@IRISA.FR>
Subject: Re: what/where vision; artificial cerebellum

I am not as fast reader as others, and I had no time to read the PAC paper, but incidently I have on my shelf the following 500 page book:

R.J. Baron, The Cereberal Computer, An Introduction to the Computational Structure of the Human Brain, L. Erlabaum, Hillsdale NJ, 1987, ISBN-0-89859-824-9

I have not read it, of course, but it contains at least an introduction to the dirty biological details for non-biologists, as well as a lot of useful references. From browsing I find its chapters on motor control much better than
on higher-level cognitive functions (as is the case usually).

--Oded

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Date: Wed, 5 Jun 91 12:15:52 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: what/where vision

[From Rick Marken (910605.1200)]

Bill Powers -- (910604.1900)

Great list of controlled variables. A start at the catalogue you had suggested some time ago. It does motivate me to find a good event control demo.
I will add one: you didn't mention any examples of controlled sequences.

Sequence. Task. Move a cursor to a sequence of locations to spell out a work (your name). The cursor was disturbed as usual. The alphabet was typed across the top of the screen. The subject pointed to each letter of their name, in sequence. They could do this over and over again although the sequence of mouse movements used to produce the letter sequence was always different (due to the disturbance) -- do we know that the resulting sequence of cursor pointing positions is a controlled variable.

I also did this using what you would call letter class control. The subject would spell out their name by keeping a displayed letter at the appropriate value while a disturbance affected which letter was being displayed (as in your numerical class control task: it is much harder to control a sequence (like a name) it this way.

Hasta Luego

****

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, 5 Jun 91 13:26:01 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Trendy Science -- Redux

[From Rick Marken (910605.1330)]

Just a quick observation before placing my nose back to the ol' grindstone. This was motivated by some of the comments about Bill's transfer function derivation algorithm (the artificial cerebellum). The comments seemed to suggest that it wasn't anything new -- which it may not be. I think it is good to try to see if a "new" idea is really like some other "old" or currently existing idea. It helps avoid "reinventing the wheel" and it points

to helpful work on the problem that might have already been done. But it can also be a reflection of, what I believe, is an all too common assumption in science and engineering today -- namely, if it's not new, it's not important. This is a version of what I discussed as "trendy science" some months ago. I believe that there is a tendency to think of ideas as scientifically important if they make use of or are based on the development of what amount to new tools. Perceptual control theory has suffered significantly from this attitude. As those watching the discussions on CSGNet might have noticed, we rarely use tools much more complicated than simple algebra (a pretty old tool). Control theory itself is pretty old and we rarely use the newer, snazier tools of control theory (like Laplace transforms, Bode plots, etc) which are themselves rather old. Unlike dynamical systems theory, neural network theory and other hot topics in living systems theory, we tend to use simple tools that are part of most high school curricula.

If you are looking for the hot new mathematical madel, the snazziest statistical theorm, the most state of the art computational algorithm, you have probably noticed that you will find it on CSGNet only by chance. We use simple tools to communicate a very deep fact -- behavior is the control of perception. Everything we say -- about coordination, perceptual hierarchies, consciousness, religious values, cerebellar stabilization, neural wiring, etc -- is based on that extraordinary model. Bill Powers was the first person to realize that that is what is occuring when systems (living and non-living) behave purposefully. In my humble opinion, that realization creates a scientific revolution as big as any that has ever occurred. But it can be stated with simple algebra:

p = r
o = -d/k

Tools are great and fancy tools may be necessary to help us understand and test detailed implications of a model. But the fanciness of the tools should not be the basis for judging the value of an idea. Not that this is what anyone commenting on Bill "cerebellum" algorithm was doing -- again, I think it is very helpful to see if something is similar or identical to prior work. But the most important tool of perceptual control theory is definitely new -- behaviors are controlled PERCEPTUAL variables. Looking at behavior from this perspective is an extremely powerful tool in itself.

Hasta Luego

****

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213 336-6214 (day)
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Date: Wed, 5 Jun 91 12:06:29 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: ed ford <ATEDF@ASUACAD.BITNET>
Subject: stuff on counseling

From Ed Ford June 5, 1991

Joel, to answer your last question first, there were more than enough questions.

Regarding your comment "assumes that each individual is worthwhile". You bet your sweet life they are. To me, each person with whom I work has value. If I don't believe in the person with whom I am working, how do you expect them to begin to believe in themselves. And if they don't eventually believe in themselves, they aren't going to make it. The more a living control system believes in itself, the more easily it will deal with its internal conflicts. That's the way it was designed to operate.

"what each has to say as valid". Because a person has value doesn't argue for the theraputic importance of everything they say. It is the job of the therapist to ask the kind of questions that will help direct the client's thinking. This is done by asking clients to explore their world of goals and perceptions with as much efficiency as possible; to get them to make comparisons within this world, namely to compare what they want with how they presently perceive things to be; to set a new reference condition or commitment if they believe that what they were doing wasn't getting them what they wanted; and then to teach them (if they haven't the skill) how to make a plan to improve their life without hindering another person's right to do the same.

"Do you accept EVERY outcome of making (I prefer helping them become aware through asking questions) them aware of their abilities to change and cope?" The meaning here is sort of hazy to me. The outcome is whether through making and then effectively implementing a plan their lives begin to improve. If and when their life begins to improve, they'll perceive that improvement. This recognition of achievement and increased harmony within their system helps develop a belief in self, and results in a growing belief in ones abilities.

"everything works out in the end because the people involved met a couple of conditions (eg.'want to change')" In my experience, that "want to change" is critical with everyone with whom I work. AA people will tell you that unless an alcoholic says "I need help", nothing is going to help him change. Ultimately, with every human being, unless they want help or they want to work at their life, nothing is going to happen. Control theory perceives us as internally driven, and if we don't set a reference level for change or imporvement, we aren't going to change. That's what makes this theory so great, all you do is follow the yellow brick road, or, in our case, the loop, or whatever.

"What about those who don't get to the point where they develop confidence?" I learned something from a distinguished professor of criminology, Alex Bassin, which was, never give up. Which means, they might quit working with you, but you don't give up on them. If you do, who do they have and where do they go? That gets back to your first

question about each individual having value. In this business, if you can't stand working with tough cases or obnoxious people (and many are), get out of the business. Or be prepared to deal with a greatly reduced case load.

With regard to your second part, namely "how are changes evaluated". Every culture or system in which we live, whether our family, school, city, state, church, lodge, club, or whatever (even the CSG), establishes standards which reflect that culture. These standards or rules are established as the basis for people being able to live in harmony within that community or group. Also, the establishment of the standards acts as the basis from which people can make choices as to whether they want to stay in that community so that that entity can function according to the way it was designed. If a child of mine wants to live in my home with someone of the opposite sex, my wife and I have set the standard that they be married. To drive on a highway, you have to live by the rules of the road or your license is taken. If you want to live in a community and enjoy the subsequent privileges, then you have to live by their rules or suffer the consequences. I have attempted to work with numerous juveniles in major lockup facilities. Some see their life improving by getting away from drugs, returning to and getting along with their families, and getting an education. You can work with those juveniles because they have begun to perceive a way of life better that offers more internal harmony than what they have been living. Other juveniles look at me and laugh. "I make more in a month on the streets than you do in a year, and I don't pay taxes," they'll say. I ask if it's worth taking a chance on getting killed or being locked up in state prison, and they reply "if I get killed or sent to prison, that's the breaks". In short, they are willing to establish values, set goals, and make choices that are contrary to the community standards and they're willing to live with the consequences (at least for a time).

The someone determining the outcome for each of us is us. If we find happiness that satisfies our internal goals by violating the standards of the community in which we live, then we will have to live with the consequences of our decisions. Those in power, occasionally the majority (in our country Congress is supposed to represent the majority, but most often seems to represent themselves), set the standards, be it a family (in my case the minority, since we had eight children) or a community. Or, again in my case, my standard was guided by the golden rule. He who has the gold, makes the rule.

You ask "what does freedom mean?" As a living control system, it means being able to establish my own values and priorities, set my own standards, and make my own decisions (here's the catch) while respecting another's right to do the same. Certainly CT allows for a far more logical basis for the concept of freedom than cognitive or behavioral theories. Finally, your interest and questions could easily form the basis for a lively discussion in Durango.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, Arizona 85253
==========
Date: Wed, 5 Jun 91 10:42:50 MST

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: questions on counseling

From Ed Ford June 5, 1991

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Regarding your comment "assumes that each individual is worthwhile". You bet your sweet life they are. To me, each person with whom I work has value. If I don't believe in the person with whom I am working, how do you expect them to begin to believe in themselves. And if they don't eventually believe in themselves, they aren't going to make it. The more a living control system believes in itself, the more easily it will deal with its internal conflicts. That's the way it was designed to operate.

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Date: Thu, 6 Jun 91 09:56:01 SST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <ISSCCC@NUSVM.BITNET>

Rich Marken:

I admire and respect Bill Powers more than other people. I read his papers and book, and I know he is one of the greatest psychologists. Especially he is self-made, it's not easy. My comments on Bill's draft didn't mean it's nothing new. It just meant to point out the relationships between his algorithm and others. If we can discover something new from the old stuff, or make new applications, it's also a great scientific contribution. I think Bill's idea has made a new scientific contribution undoubtedly.

Chung-Chih Chen

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Date: Thu, 6 Jun 91 07:48:26 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Tom Sawyer; Hebb's Rule

[From Bill Powers]

To Whom it May Concern:
I feel like Tom Sawyer (character in classical US story, a boy who allowed his town to think he was dead so he could come to the funeral and hear what they said about him). Don't forget that the victim of all this praise is listening. It's nice to hear, but difficult to deal with. Please don't put me on a throne. Remember that you're going to have to live with these ideas a lot longer than you're going to have to live with me. So be skeptical. Bill Powers can be stupid and say some pretty dumb things, too. It's up to you to sort out the good stuff from the bad stuff, and not just write a blanket receipt for anything that comes out. If you could only see how messy it is in here!

Here's an idea to sort out.

I keep running across references to "Hebb's Rule" of learning, which seems to mean that if there is activity on both sides of a synaptic junction, the strength of the synaptic connection should be increased. The motivation appears to be to account for classical conditioning. I would like to suggest a different principle -- not necessarily as an alternative, but as a principle that would seem necessary if a negative feedback control system is to be learned.

In order to get activity on both sides of a synaptic junction, we have to think of three neural signals: two in and one out. The second input is necessary to get activity on the other side of an inactive synapse, before any connection at all can be established. So we can think of primary and secondary inputs.

The primary input is the one that is going to be established as a result of injecting secondary inputs (the secondary inputs, in this case, correspond to the UCS). Let's pick up the action when the primary input is having some effect on the output.

If this link is to be part of a control system, it must become part of a negative feedback loop that extends through the organism from input to output and from the output of the organism back to its input via a different path. If any signal or physical variable in this negative feedback loop is disturbed from outside the loop (by a physical disturbance or by an extraneous neural signal), the effect of the disturbance will be counteracted by an equal and opposite change in the signal arriving at the same point from a prior part of the loop. For example, if the reference signal is increased, the error signal will increase, but this change will be counteracted by feedback effects on the perceptual signal; the perceptual signal will rise, inhibiting the comparator, and reduce the error signal again.

This says that during formation of a control system, the rule must be that activity on the downstream side of a synapse caused by injection of a secondary input must change the primary input so as to result in an equal and opposite effect downstream. The critical thing is that the effect of the secondary input downstream must result in the strength of the synapse changing in the way that gives the primary input the OPPOSITE effect downstream, so that the disturbance is resisted.

If the rest of the closed loop is set up so the primary signal decreases (because of negative feedback effects) when the downstream activity increases, then Hebb's Rule would be appropriate: the primary signal should be given more effect on the downstream signal, increasing the loop gain and reducing the effect of the disturbance from the secondary signal. But (and here is where Hebb's rule, I believe, must be modified) ...

If an increase in downstream activity results in an increase in the primary signal (positive feedback), the strength of the synapse should be DECREASED. Furthermore, if the positive feedback persists, it should be decreased to ZERO, and some other synaptic connection carrying the same primary-signal information should begin to form an INHIBITORY connection.

Notice that this rule would take care of ANY cause of positive feedback, including a transport lag or phase lag occurring in the rest of the closed loop. If the feedback ever becomes positive, the loop gain should be decreased. This takes care of the tradeoff between loop gain and phase or transport lag. If decreasing the loop gain somewhat does not remove the positive feedback, it must be decreased more. If we get all the way to zero before the positive feedback is removed, then the problem was not phase relationships but an incorrect sign of connection somewhere in the loop. We must establish the opposite kind of connection: inhibitory if it was excitatory, excitatory if it was inhibitory.

It is also interesting that this modification of Hebb's Rule (as stated here) applies only at the point where an extraneous disturbing signal is injected. The test for the sign of feedback requires that some

independent source cause a change in the level of activity on the downstream side of the synapse, and that this change be compared with the resulting change on the upstream side. This can have meaning only in a closed-loop model: in an S-R model, effects always run from upstream to downstream and there is no general expectation that a downstream change will be reflected as an upstream change.

Changes in activity in a synapse where the upstream signal changes BEFORE the downstream signal should result in NO change in synaptic strength or sign. The changes should occur only when the downstream activity changes first (because of an extraneous disturbance). If there is NO external feedback loop, apparent positive feedback will occur as often as apparent negetive feedback (due to extraneous fluctuations) and there will be no systematic effect on the kind or strength of synaptic connection.

I think we're talking about an aspect of the "reorganization system." If the rule for changing synaptic strength worked as proposed here, there would be an EXTREMELY strong tendency for the nervous system to become organized into negative feedback control systems.

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Date: Thu, 6 Jun 91 12:47:04 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: stuff on counseling

[from Joel Judd]

Ed (910606),

>"Do you accept EVERY outcome of making (I prefer helping them become
>aware through asking questions) them aware of their abilities to change
>and cope?" The meaning here is sort of hazy to me. The outcome is
>whether through making and then effectively implementing a plan their
>lives begin to improve. If and when their life begins to improve,
>they'll perceive that improvement. This recognition of achievement and
>increased harmony within their system helps develop a belief in self,
>and results in a growing belief in ones abilities.

I think my meaning here centers around "improvement." That's an individual perception influenced by one's environment. One may perceive his life improving through joining a gang. This gets into if someone is making a judgment concerning acceptable improvement. That's why I used a more neutral term like "change" to ask the question. Regarding the judgment aspects of this:

>With regard to your second part, namely "how are changes evaluated".
>Every culture or system in which we live, whether our family, school,
>city, state, church, lodge, club, or whatever (even the CSG),
>establishes standards which reflect that culture. These standards or
>rules are established as the basis for people being able to live in
>harmony within that community or group.

So as long as people live together harmoniously the standards are OK? Aren't there standards that simply don't work, haven't they been

demonstrated before historically, and isn't there some basis for talking about "right" "effective" or "universal" values? Why do succeeding generations always seem to feel somehow exempt from those values which have caused problems in the past?

>You can work with those juveniles because they have begun
>to perceive a way of life better that offers more internal harmony than
>what they have been living. Other juveniles look at me and laugh. In short, they are
>willing to establish values, set goals, and make choices that are
>contrary to the community standards and they're willing to live with
>the consequences (at least for a time).

This seems to imply that there are 'ways of being' that are intrinsically harmonious, and ways that aren't. Why might this be the case? Why would this be inherent in the design of something like a CSH?

>You ask "what does freedom mean?" As a living control system, it means
>being able to establish my own values and priorities, set my own
>standards, and make my own decisions (here's the catch) while
>respecting another's right to do the same. Certainly CT allows for a
>far more logical basis for the concept of freedom than cognitive or
>behavioral theories. Finally, your interest and questions could easily
>form the basis for a lively discussion in Durango.

Yes, I would like to pursue this.

Joel Judd

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Date: Thu, 6 Jun 91 18:23:52 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Morals vs. System concepts

[From Bill Powers (910606.1800)]

Joel Judd, Ed Ford (910605+) --

Poeple often use terms like "live in harmony" and "learn to cooperate," which sound like unbiased and fair prescriptions for social life. But they seldom mean it that way -- just think of a teacher who puts on a report card "needs to learn cooperation with others." What it means is that Little Johnny had better get in line or there will be trouble for Little Johnny. Cooperation definitely doesn't mean that the social group is going to cooperate with the individual; it's a one-way street the other way.

As Ed said, "I've got the gold, so I set the rules." This means that the person with power (personal, financial, or political) uses it to assure that those who want the gold (food, shelter, health) behave as that person wants, whether or not it conflicts with what they want. This method creates a certain limited range of harmony within a group of limited size, but at the expense of harmony within all but one of the individuals: outward harmony, inward conflict. It does not work across

groups.

The opposite is no cure: inward harmony, outward conflict. Think of Donald Trump enjoying his triumph over that little businessman on whom he stomped. He bragged about it in a book! No inner conflict there. But who would want to live in a world of Donald Trumps? How long would such a world last?

True harmony means inward and outward harmony. It means that in finding ways to avoid conflict with others, individually or as a society, the individual is also able to avoid inner conflict. Our world is not set up this way at present. It's set up on the basis of controlling others and winning conflicts. It's set up so that most people must be losers, because winning is organized like a pyramid. Kids are taught that in this country ANYBODY can grow up to be President. Somehow, all these millions of kids fail to be advised that in their lifetimes there will only be perhaps ten Presidents. Some opportunity: there's a better chance of getting into the NBA. The ladder of success is not designed to let everyone climb it without knocking someone else off.

Joel, you say:

>Aren't there standards that simply don't work, haven't they been
>demonstrated before historically, and isn't there some basis for talking
>about "right" "effective" or "universal" values? Why do succeeding
>generations always seem to feel somehow exempt from those values which
>have caused problems in the past?

I think one problem is that there are no "succeeding generations." Life doesn't consist of cohorts separated by a whole generation. People are born every minute. A child grows up in a world composed of people of all ages, and there's never a moment when one "generation" pauses, sums up its knowledge, and passes it on. Whatever understanding of human problems there is is presented simultaneously to one-year-olds and to ninety-year-olds, and it's presented by, say, 12-year-olds through eighty-year-olds (want to leave myself a safe margin there).

Human understanding slowly changes shape and is shared in varying degrees by a wide assortment of people of many ages. Human societies, therefore -- since they are the product of all the people in them -- also slowly change shape. When exceptionally workable ideas come along, the changes are rapid. When the ideas become muddled, the changes can slow or reverse or take off in unpromising directions. But I think that the changes that can be made by fundamentally good ideas remain -- regression is uphill. The natural direction is toward the greatest return for the least onerous effort, and that is the direction of eliminating both internal and external conflict.

I don't think that the world is going to be either saved or destroyed by any particular set of proposals as to how we should run our affairs. Specific proposals are at too low a level. So are specific principles -- moral standards, economic principles. The ideas that stick around and have a long-term (if slow) effect are the system concepts (or whatever that level of conceptualization is). The question is always "what kind of world do I want to make and live in?" That question is even more

important than "What kind of person do I want to be?" Living in a world of limited resources with other people who are just as autonomous as you are is a difficult problem, an extremely complex problem. We will arrive at successive approximations to solutions by trying different solutions and seeing how they work. Gurus and saviours come and go; they leave their traces, and we choose which traces to retain. Blind variation, but selective retention.

Joel, you can argue that there is something that tells us what to retain and what to reject. I won't object, although I reserve the right to my own thoughts about what that something might be. My point is that when we think at the system concept level, we are far more likely to be helping to provide a choice of viable futures than when we simply propose clever sets of principles and rules that look as it they might achieve some immediate semblance of order -- even a New World Order.

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Date: Thu, 6 Jun 91 21:53:30 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Stumped Mice

[from Gary Cziko]

Ever since discovering perceptual control theory (PCT) about a year and a half ago, I naturally see the behavior of the people and animals around me in a quite different light. While I find the perspective provided by PCT to be usually quite illuminating, I occasionally run into things (or remember things) that seem at least in some ways inconsistent with PCT. Here is one.

I recently ran across a reference to a study by Fentress (1973) who amputated the forelimbs or mice who nonetheless went through the motions of preening the head with the nonexistant forelimbs as evidenced by the motions of the stumps and coordinated movements of the head and eyes. I have not yet looked at the study, but would be interested in anyone who has or anyone who could throw some light on this from a PCT perspective. I would have normally thought that the mice was controlling some tactile perception involving the paws and head since how would the genome know how long the mouse's limbs and claws would be, how large the head, etc. But this is obviously not the case for the mice-amputees. Any thoughts?--Gary

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Date: Thu, 6 Jun 91 22:49:48 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Stumped mice & model-based control

[From Bill Powers (910606.2030)]

Gary Cziko (910606) --

Concerning the "stumped mice" grooming themselves with missing limbs.

Do you remember late last Fall (I think) when I went on about a possible revision of the basic model? The idea was that instead of perceptual signals passing from one level to the next, the error signal from the lower level system went into the perceptual input of the higher system. Along with the error signal there was a modeled perceptual signal, generated inside the higher level system. The net result -- model plus lower-level error -- yields the same effect at the higher level, provided that the model is an accurate representation of the average response of the lower-level world to reference signals. The higher system sends its output both into the model and into the lower system as a reference signal as usual. If the reference signal is manipulated so as to make the model behave correctly, it will also make the lower systems behave correctly. Wayne Hershberger refers to this as "re-afference" although it is a bit more complicated than that.

The error signal carries information telling the higher system the amount by which and the direction in which the model fails to behave as it should. It can therefore serve as the basis for corrections of the model. I suppose that this will eventually be related to calculations like those in the "artificial cerebellum" algorithm.

I haven't tried to model this arrangement. Someone should. It is needed -- perhaps -- to explain your mice, and to explain how it is that we can manuever through a room for a while when the lights go out. There are many cases in which feedback information is interrupted, yet we seem able to continue, at least for a short time, as if we were still controlling the same perception. This could be explained in part by saying that we switch to controlling in some equivalent modality -- kinesthetic or auditory instead of visual, for example. But I think the model-based control idea is neater; it works automatically as it should without any sudden switching to a different means.

In simpler organisms there may be simpler explanations. The mice, for example, may be controlling not tactile perceptions alone, but kinesthetic perceptions which remain at least partly the same.

My proposal above is just a complicated way to say "imagination," but maybe it will prove to be correct in some instances. As far as the basic premise of PCT is concerned, we would still be controlling perceptions, and the perceptions would still be based on experience with the outside world. All the "model-based" control method does is to make the main part of perception into a model that reflects the average state of the world instead of its momentary state. With the lower-level error signal added to this average model's behavior, the net result is still a real-time current picture at the higher level. But loss of lower-level information does not cause immediate collapse of the control system.

Robert Albus, I believe, has proposed a model-based control system, although his categories of control seem dictated more by industrial project management principles than by observing what organisms in general do.

I still don't know whether to propose this revision seriously or just to let the original model stand for the time being. I don't think it will make much difference in our present state of progress. Perhaps we should stick with the simpler model until we run out of material to use it on. On the other hand, we may find model-based control a better and more intuitive explanation of enough kinds of behavior that we will more or less be forced to make the change. What do you think?

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Date: Fri, 7 Jun 91 13:29:44 +0200
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Oded Maler <Oded.Maler@IRISA.FR>
Subject: A Bug or a Feature? [Re: Trendy Science -- Redux]

[From Oded Maler]

(Rick Marken says:)

>Unlike dynamical systems theory, neural network theory and other hot topics
>in living systems theory, we tend to use simple tools that are part of
>most high school curricula.

etc.

The attitude people adopt toward things they don't (or can't) comprehend is usually one of the two extremes. They either blindly admire other people who
do understand such things, or dismiss the whole subject as irrelevant, unimportant or silly and even make an ideology out of their ignorance.

Having myself being completely ignorant in the technicalities of mathematical control theory and continuous dynamical systems (I have a theoretical CS background, which means that I proved some theorems in my thesis), I face similar problems when I try to implement my worm simulator. Aware to my limited mental resources I try to distinguish between the mathematical and conceptual essence of such theories, and what are just technical gadgets which are irrelevant to what I try to solve.

It might be true that 90 percent of mathematics is irrelevant to what you are interested in, and that most of mathematician are just high-quality abstract tecnicians who never think about the real question of life (I'm not sure it's different with most other disciplines), but it might be the case that the remaining 10 percent are exactly what is needed to make some flaky intuitions into a sound theory. I also see point in criticising mainstream "trendy" science, but you should always remember that the alternative might look to others just as bunch of loonies repating some self-evident mantras such as: "behavior is THE.."

I hope I haven't deviated from the polite and friendly conversational standards
of this group, and I would like to say also that I prefer simple solutions based on simple tools (even your high-school algebra is sometimes hard for me to follow..), I think I would be very proud of myself if I re-invent the wheel, and you can be sure that in a debate with some mathematicians scientists or engineers coming from established disciplines I would always represent the opposite case of the outsiders.

--Oded

p.s.

A constructive question: I might have missed something but I'm not sure I understood the intention behind Bill's recent list of various control task experiments. Are the experiments associated with response time in order to place each skill in its proper place in the hierarchy? Are they intended to test other hypotheses concerning the underlying control mechanism?

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Date: Fri, 7 Jun 91 09:54:13 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Trendy Science Redux, Hierarchical Control Paper

[From Rick Marken (910607)]

Chung-Chih Chen (910606) says:

>My comments on Bill's draft didn't mean it's nothing
>new. It just meant to point out the relationships between his algorithm and
>others. If we can discover something new from the old stuff, or make new
>applications, it's also a great scientific contribution.

Of course, you are right. I'm afarid my response to your helpful comments about the algorithm seemed more negative than they were intended to be. My comments to you (Chen) and Joe Lubin about "trendy science" were strongly colored by my previous experience with colleagues and reviewers who seems to be more interested in whether an idea is new and clever than in whether it is right.

The fact that the intent of my comments was not clear is reflected in comments by Oded Maler (910607) who says (to me):

>It might be true that 90 percent of mathematics is irrelevant to what you are
>interested in, and that most of mathematician are just high-quality abstract
>tecnicians who never think about the real question of life (I'm not sure
>it's different with most other disciplines), but it might be the case that the
>remaining 10 percent are exactly what is needed to make some flaky intuitions
>into a sound theory.

I agree. I did not mean to suggest that new ideas are wrong because they are new, or popular or complex. Complex mathematical tools can be extremely useful; in fact, they can be essential for the theory. Calculus was a very

new mathematics and it was essential to Newton's physical theory. The same, I believe, is true of the mathematics of Minkowski (Non- euclidean, multi-dimensional spaces) and Einstein's relativity (I think). There is no question in my mind that new and complex tools will be needed to support perceptual control theory. My problem is with those who get excited about the tools and then look for applications -- tools first, phenomenon second. That's what I think is the problem -- as well as a somewhat superficial approach to a problem that makes the use of the tool seem appropriate. This, I believe, is what is going on with "dissipative systems" approaches to understanding coordination and control (an area of "trendy science" with which I have some painful familiarity). The simplest dissipative system is like a pendulum; it returns to the same state after a disturbance. This looks like control but it is not -- a continuous disturbance is not resisted; the pendulum stays displaced. Thus, although dissipative systems do not account for some of the basic facts of control, an enormous amount of work has been done with more and more complex dissipative system models that are well beyond my mathematical skill. People keep this up, I think, because it is trendy-- it has to do with stuff like chaos and self-regulation. I have a problem with it, not because it is complex, difficult or popular (the dissipative system models may be great models of actual dissipative systems) but because I still want to know how the model deals with a very simple phenomenon before I go into the more complex stuff. I think Einstein looked to the Minkowski mathematics only AFTER he knew what he wanted to explain -- and he couldn't find the available tools in the obvious places. To me, trendy science fits Maslow's description of the limitations in problem solving -- "when all you've got is a hammer, every problem looks like a nail" (or something like that).

> I also see point in criticising mainstream "trendy"
>science, but you should always remember that the alternative might look
>to others just as bunch of loonies repating some self-evident mantras such
>as: "behavior is THE.."

I am well aware of this. I was just reminded of it yesterday afternoon when I received the rejection, from Psychological Review, of my paper "Hierarchical control of behavior". One reviewer (there were two -- neither thought much of it) said that "there is an air of superficiality to the piece, exemplified by catch phrases such as "Behavior is perception in action"". I never heard that criticism before but I can understand it. It must be annoying for people to hear me keep repeating my "self-evident" little mantra "behavior is the control of perception" over and over again. I have spent over 10 years trying to show, through computer models, math, experiments and demonstrations, what that phrase means. I keep repeating it because it is so hard to repeat the hundreds of pages and hours of computer demos that back it up. I repeat it in the hopes that people will be bugged enough by the peculiar phase to look more carefully at what it means. I repeat it because I know from experience that the meaning I get from it is not the meaning many other people get from it -- the reviewers of my paper, for instance. I can't count the number of times people have said "sure, behavior is the control of perception, no problem" and then give me examples of the obviousness of this phrase by explaining how our behaviors are "based on" what we perceive. It eventually becomes crystal clear (to me) that the listener has managed to translate my little self evident mantra into their own little self evident mantra "behavior is controlled by perception".

We communicate our ideas through words. I tried, in my "Hierarchical control

of perception" paper to communicate a model; a perspective; an imginative vision of what undelies an expereince -- the experience of behavior. I tried to explain what this experience is like from the point of view of the model AS A BEHAVING SYSTEM and AS THE OBSERVER OF BEHAVING SYSTEMS. To some
extent, communicating this vision is like using language as poetry. So I prefer to think of a phrase like "Behavior is perception in action" as a poetic statement -- rather than a mantra, a "catch phrase", a slogan or a scientific explanation.

Bill Powers (9106??)

I liked the Hebb post. It will take some time for my little brain to understand it and formulate an intelligent reply (if I can). But I shall try. I also loved your beautiful reply to Joel and Ed. I'd join your religion any day. Actually, I guess I did, long ago.

Also, sorry about the embarassing praise. I knew you would be embarassed. You can avoid it, of course. Just stop having those incredible ideas. There, got another one in.

I will post more details about the rejection of my paper (which, in my humble opinion, was mind bogglingly good) when I get a chance. Now, back to the ol' drawing board.

Best regards

****

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Date: Fri, 7 Jun 91 23:08:51 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Model Revision

[from Gary Cziko 910607]

Bill Powers 910606:

I must admit that I didn't quite understand the purpose of the revision you proposed last October. But your reply to my question about the stumped mice makes it much clearer now. It looks like a clever move, maybe too clever--sort of like having your feedback and eating it too.

I am taking the liberty of reposting below your original mention of this on 901011 for those who have joined the network since then.

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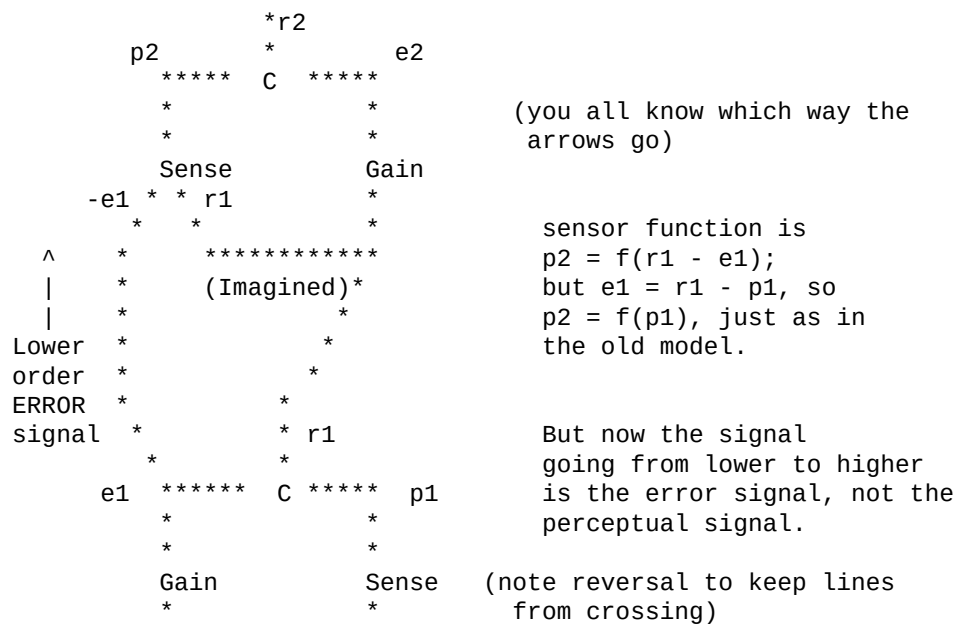
[from Bill Powers; originally posted 901011]

Out-of-the-blue department. Hershberger's recent comment, plus past suggestions by many others (resisted by me), plus some unknown extraterrestrial force, has created a REVISION OF THE MODEL (maybe, if you think it checks out). The basic problem is that we seem to know what we are doing before we do it. The "imagination connection" partly takes care of this apparent perception of reference signals (i.e., apparently perceiving an output signal), but requires a clumsy and mysterious switch to bring the outgoing signal into the incoming channels where I still think perception takes place. And you can't have imagination and real perception going on at the same time without some really ad-hoc design features that would probably turn out to be bugs. Scott Jordan and Wayne found out that subjects' brains compute the position of the light as if the eye were already in its intended position (but before eye movement to that position starts). Here, I think, is the model that takes care of that and a lot of other problems:



This does a number of nice things. If some other system completely inhibits the lower-order comparator (which turns off the lower-order system, because you can't have negative frequencies in neural signals), the higher system is automatically in the imagination mode. The subject perceives the intended result, not the actual result. The higher system experiences NO ERROR. When you disinhibit the lower comparator(s), there should be a momentary error in the higher system until the lower one succeeds in making its error signal zero again. The result is exactly the same as in the former model, but the process of getting there is different, and the experience is different.

Comments?

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Date: Fri, 7 Jun 91 17:22:47 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Mantras ETc.

[From Bill Powers (910607.0730)]

Oded Maler (910607) --

Not many delicate psyches on this Net, Oded. If we ever start sounding like loonies chanting mantras, we, of course, will hope you get over your auditory delusion, but feel free to speak up, we'll try to be kind.

Good mathematics (to comment on the other topic) helps us to understand the implications of our own assumptions and to work out complexities that are too much for human intuition. Bad mathematics (for the purposes of modeling at least) is a substitute for understanding. Some people will believe anything if they can find a theorem to prove it.

The catalog of controllable variables is, in part, a device to help people overcome the impression that "Behavior is the control of perception" is just a mantra. The basic test for control is to see whether a disturbance of some variable is systematically resisted by an active system. If you have access to my '73 book, check out the Coin Game to see that this Test is not necessarily simplistic. Controllable variables do not have to be physical aspects of the environment, although many of them are. A "physical aspect of the environment" is a perception that is part of a particularly well-constructed model of how the world of direct experience works.

By setting up a situation in which the Test is formally applied, we can establish the reality of control with respect to any definable variable. The list of tasks accompany the catalogue shows how anyone can set up an experiment that demonstrates each variable to be controllable. We start with simple and self-evident perceptual variables in the attempt to accumulate a solid base of facts about control behavior, at the level of Physics 101. Nobody doubts that pulleys work, but students of physics have to show formally not only that they work but how they work. When they've done enough of that they can go on to the next stage of complexity. We're trying to build an understanding of control processes that is as reliable as theories about pulleys.

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Date: Thu, 6 Jun 91 15:36:50 BST
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: Re: Artificial

[from Marcos Rodrigues]

Bill -- (060491)

As far as I remember of Albus' CMAC, your "Artificial Cerebellum" is based on the same principles. Again, I see the same problem I've found in CMAC: there is no guidance on how a just learned

trajectory (through adjustment of system sensitivity and
f(tau) table) can be used for a new, different arm trajectory and for
different loads. Your decay function predicts that the table
will (eventually) converge to zero, so there will be occasionally
(often, always?) a fresh start.
When you write:
>   known analytical form. This system can therefore adapt itself to
>   a variety of loads without either a teacher or any direct
>   knowledge of the nature of the load. It learns by practice.
the reader (I, for once) gets the impression that a few trials
with different trajectories and loads will increase the system
performance. Am I the only one?
So, it would be interesting to know how such stabilising procedure could
be used, for instance, to improve "shooting a free throw" discussed here in
the net last December (I think).

Best regards,

Marcos.

Marcos Aurelio Rodrigues
Univ. College of Wales, Dept CompSci, Aberystwyth, UK, mar@uk.ac.aber.cs
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Date: Sat, 8 Jun 91 02:39:14 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject: Hebb's Rule

Subject: Hebb's Rule

[From Joe Lubin (910606.1600)]

Bill Powers (910606)

First let me preface my response by saying that I am having trouble
mapping a small circuit (a synapse) for classical conditioning into
the control loop as you describe it. I'll probably need some help
in trying to flesh this out. As a result of my confusion, the
following may ramble a bit.

As a preliminary consideration, we need to address Hebb's rule.

Physically, Hebb's Rule is not entirly plausible because in an actual
setting resources (membrane proteins, mRNA, protein kinase C, Ca++,
etc.) are not unlimited. Hence, the continued pairing of
two signals cannot raise the association strength beyond some
asymptote. What's required is some sort of synaptic strength
conservation. Many modelers have used such a thing, some
elegantly (employing lateral inhibition), some not so (ad hoc

normalization). A simple Hebbian equation for the modification of synaptic efficacy would look like:

```
  dz/dt = F1[ activ1 ] * F2[ activ2 ]

  where
    z is the synaptic efficacy,
    F1 and F2 are nondecreasing, nonnegative transfer functions,
    activ1 is the activity of the presynaptic neuron, and
    activ2 is the activity of the postsynaptic neuron.
```

Under normal assumptions for physical neurons, this rule does not permit any decay of the synaptic trace: z grows monotonically. An equation typical of Grossberg's approach [Grossberg S, Levine DS 1987] would look like this:

```
  dz/dt = (-A * z  + F1[ activ1 ]) * F2[ activ2 ]

  where
    z is the synaptic efficacy,
    A is a slow decay constant,
    F1 and  F2 are nondecreasing, nonnegative transfer functions,
    activ1 is the activity of the presynaptic neuron, and
    activ2 is the activity of the postsynaptic neuron.
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This equation contains a Hebbian term (F1[activ1] * F2[activ2]) and a decay term ((-A * z) * F2[activ2]). The decay term is active whenever the postsynaptic membrane is active. Grossberg has called this an associative decay rule elsewhere [Carpenter GA, Grossberg S 1987]. In order for the synapse to increase in efficacy, both pre- and postsynaptic elements need to be on. If only the presynaptic element is on there is no change. If the presynaptic element is on and there is a small postsynaptic activity, then the efficacy begins to grow. Here is where your secondary input comes in. The secondary input (or UCS) would create postsynaptic activity (increasing the activ2 signal in the postsynaptic neuron) so that learning at the primary input's synapse could begin. Notice also that this equation allows for decay at the primary synapse if the postsynaptic activity is high and there is no presynaptic activity. This is a mechanism for forgetting. One of its justifications is that there must be limited resources available for constructing and maintaining the memory substrate (whether it be enhanced transmitter production, enhanced transmitter receptivity, growth in synaptic membrane, etc.). This equation implements a competition for memory stuff. The multiplicative "turning on" of growth or decay by the postsynaptic activity is termed "gating."

In a different vein, Dan Alkon [Alkon 1989] postulates a "local-interaction model" to replace Hebb's model of learning-induced changes. In Alkon's model, a process carrying a UCS synapses on a dendritic spine very near a second spine on which synapses a process carrying a CS. These two signals interact in "Hebbian fashion" to increase the synaptic efficacy but without requiring the firing of the neuron. Competition is still in effect because the synaptic modifications require raw materials from the

cell's nucleus.

> In order to get activity on both sides of a synaptic junction,
> we have to think of three neural signals: two in and one out. The
> second input is necessary to get activity on the other side of an
> inactive synapse, before any connection at all can be established.
> So we can think of primary and secondary inputs.

This is a fundamental postulate in many models.

> This says that during formation of a control system, the rule must
> be that activity on the downstream side of a synapse caused by
> injection of a secondary input must change the primary input so as
> to result in an equal and opposite effect downstream. The critical
> thing is that the effect of the secondary input downstream must
> result in the strength of the synapse changing in the way that
> gives the primary input the OPPOSITE effect downstream, so that
> the disturbance is resisted.

This paragraph was very difficult for me. Let's see if I sorted it out. After reading it about 50 times I just can't believe it is true. For a purely linear model of a control system this would be true. For a purely linear model of a conventional synapse this would be false. I think however I can carve out a middle ground. Linear modeling in either the field of computational neuroscience or artificial neural networks has been largely abandoned except in a small number of relatively simple situations. I take both the control system model and the synapse model that you are describing to be either linear or nearly so. In my opinion, simple learning, as you've described it with primary and secondary actors on a postsynaptic site, must act largely by strengthening associations. If I understand you, this poses a problem for control theory since if the secondary signal grows it should set up a chain of events which lead to an offsetting injection by the primary signal, which requires the primary synapse to change with a sign that is opposite to the initial disturbance.

I think there may be a flaw in your reasoning. First of all, is there any strong reason to believe that the control system is a closed loop which must feed back onto that particular synapse? I can imagine that synapse as one of a number of alternatives in a closed loop control system. As a result of the secondary signal injection, the "activity path" through the network might avoid that synapse the next time around. You say,

> The critical thing is that the effect of the secondary input downstream must result in the strength of the synapse changing in the way that gives the primary input the OPPOSITE effect downstream, so that the disturbance is resisted.

Is it possible that the disturbance is resisted by rerouting?

You are assuming that, in a closed loop, the primary signal and the secondary signal are both going to be active and are going to sum in a nearly linear way. The primary signal is being trained as something

of a replacement for the secondary signal. It is to be a predictor designed to obviate the need for the secondary signal in certain situations. Even if the two signals are active simultaneously and sum linearly, this does not mean that there will be a linear summation effect on the signal traversing the loop. Two signals impinging on a neuron may not be enough to cause it to fire. In this sense the computation is very nonlinear. It doesn't matter too much to a neuron if two, seven, or fifty signals impinge on it. The only thing that matters is how close these signals take it to threshold. As I see it, the primary synapse, and its support, is added to the control loop as a consequence of a temporal pairing with the secondary event. The secondary synapse is already part of that loop. The addition of the primary synapse adds lateral complexity to the loop (in the sense that a new pathway becomes feasible). It does not add a quantity of signal to the activity already in the loop. If an auditory cue becomes a predictor of meat powder for a dog, it means that, within some feeding control loop, an auditory associative assembly has an effect on a salivation assembly. Once this auditory assembly affects the salivation assembly there is not much for the olfactory (UCS) or visual assembly to do in this loop. These latter assemblies will just be pissing in a rainstorm.

> If an increase in downstream activity results in an increase in
> the primary signal (positive feedback), the strength of the synapse
> should be DECREASED. Furthermore, if the positive feedback persists,
> it should be decreased to ZERO, and some other synaptic connection
> carrying the same primary-signal information should begin to form
> an INHIBITORY connection.

In this positive feedback situation either the behavior associated with the synapse should decrease in magnitude, should be turned off, or should be turned off and replaced by another. This much is agreed. I am leery of instantiating control loops at the level of synapses. Actually, more accurately, I am leery of instantiating control loops at the level of synapses using linear models for synapses and neurons. I have a strong feeling that control loops will be found from the atomic levels to the social levels, but I must balk when I see a mapping of a linear control loop onto what I assume is a nonlinear construct (the neural circuitry). I am very excited, however, in embracing control theory in my modeling, but I still feel that the control theory model will need to be instantiated in many diverse substrates. Essentially, it won't look the same in all cases.

> Changes in activity in a synapse where the upstream signal changes
> BEFORE the downstream signal should result in NO change in
> synaptic strength or sign. The changes should occur only when
> the downstream activity changes first (because of an extraneous
> disturbance).

Grossberg's shunting equation above works this way.

It is entirely possible that I have totally misunderstood you. My lack of detailed knowledge of CSG-type control systems may be leading me into misinterpretations.

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Date: Sat, 8 Jun 91 09:48:22 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Models; Neural Nets

[From Bill Powers (910608.0800)]

Gary Cziko (910607) --

Thanks for reposting that diagram. Unless someone else beats me to it I will try to work this up as a real model of tracking behavior to see if

it has an advantages at all. It won't be hard to beat me to it.

Marcos Rodrigues (910607) --

I guess I'm going to have to read up on CMAC. I recall thinking that Albus had some very good features in his model, including model-based control, but I'll have to study up on it some more. If you have (or somebody closer has) a writeup on this model, I'd appreciate getting a Xerox. I've just begin looking into Ft. Lewis College's library, but it's probably going to be pretty limited in this area. Getting references is going to be slow.

This transfer-function thing is difficult to communicate so that the right "feel" of it gets across. The decaying function doesn't imply that behavior decays to zero. Think of this algorithm as a way of designing a box (a self-adapting box) that goes into the control-system diagram where the normal output-function box would go. Through furious internal activity, this box simulates a dynamic response function; it can act like various combinations of proportional, integral, and derivative response to the error signal, but it isn't limited to forms that can be handled analytically.

The input to this box is a continuously-varying error signal, and the output from it is a continuously-varying output signal. You don't see the shape of the transfer-function curve in the output unless the error signal consists of a very brief impulse. Normally, the output is a continuous stream of overlapping impulse-responses of this sort, with only the envelope of the overlapping responses visible in the output signal.

There's no problem with "trajectories," because they are taken care of by the usual variations in reference signal. This algorithm is strictly a way of making the DYNAMIC characteristics of the system match the requirements of the external feedback path, so that the whole loop doesn't oscillate, overshoot, undershoot, and so on. The time-scale of the transfer function curve, for the arm-control model, needs to be only a few tenths of a second long, a second at the most.

You can see why I haven't put this method forward before. It's just very difficult to understand unless you've had some hands-on experience with this kind of analysis. Keep trying -- look up "convolution" in a text on servo design.

Joe Lubin (910608) --

>First let me preface my response by saying that I am having trouble
>mapping a small circuit (a synapse) for classical conditioning into the
>control loop as you describe it. I'll probably need some help in trying
>to flesh this out.

It's a mutual problem. We can work on it from both ends.

I assume you're familiar with my "standard" block diagram of a control system: input function, comparator function, output function, and a

feedback path through the environment where disturbances also can be injected.

I'm imagining that we're talking about just one of these boxes. The one that naturally fits the closest is the comparator, where we have a perceptual signal entering one input, a reference signal entering the other, and an error signal exiting. As always, it's implicit in the diagram that this box is really a set of boxes operating in parallel, redundantly, so we're talking about composite signals and sets of neurons. But it's easier to take a single two-in, one-out circuit as representative.

Suppose we start with perceptual signals existing but not connected to comparators. Assume that there are reference signals entering the comparators in the excitatory sense, producing error signals. The error signals are "fanned out" to lower-level systems to serve as reference signals, or else to go muscles, depending on the level of system we are talking about. The lower-level systems do as the reference signal recommends, have effects on the external part of the loop including the environment, and end up altering the perceptual signals.

To complete the control systems, we need the perceptual signals to synapse with the comparator-neurons in the correct sense (for each perceptual signal involved in this system). Which sense is correct depends on the external loop -- whether there is a net sign inversion between the error signal and a given perceptual signal. If there is no sign inversion, the perceptual signal must synapse through a Renshaw cell -- or if possible form a direct inhibitory synapse at the comparator. This is where my version of Hebb's Rule comes into play.

The modified rule says that if there is postsynaptic activity prior to presynaptic activity, a positive synaptic connection must weaken or an inhibitory one must strengthen. The postsynaptic activity comes from the reference signal. The presynaptic activity is in the perceptual signals that are being affected through the long feedback loop through lower-level systems and the environment (it's long compared with neural-system distance scales). If that long loop does not invert the sign of the effect, then the perceptual signal must synapse negatively, so as to have the opposite effect from the reference signal.

Actually, reference signals from higher systems can enter ANY box in the control system: input function, comparator, or output function. In the brainstem, comparators and output functions appear to be combined in the motor nuclei. So this application of the modified Hebb Rule could be thought of as taking place anywhere in the neural part of the control loop. With suitable transformations these various ways of injecting reference signals are equivalent to the standard representation that I use, with the reference signal entering the comparator only.

>A simple Hebbian equation for the modification of synaptic efficacy
>would look like:

> dz/dt = F1[activ1] * F2[activ2]

> where

>   z is the synaptic efficacy,
>   F1 and F2 are nondecreasing, nonnegative transfer functions,
>   activ1 is the activity of the presynaptic neuron, and
>   activ2 is the activity of the postsynaptic neuron.

My modification is simple: it says

  dz/dt = -F1[activ1] * F2[activ2]
           Where [activ2] leads [active1] in phase.

Note the negative sign.

The reason that synaptic-strength conservation or decay is needed in the standard approach is the same one that Harry Klopf (Wright-Patterson) ran into. One of his graduate students, trying to make the model work for classical conditioning of a "selfish neuron," concluded that the bare rule itself led immediately to all synapses going to maximum strength (no matter what variation he tried). As you suggest, maximum strength is set only by phsiological limitations on resources. The student concluded that this is a pretty useless model as it stands.

To make the model work, it's necessary to postulate some mechanism for keeping all the synapse-strengths from saturating. One mechanism that works is, as you mention, "normalization." This method very subtly introduces some magic: when one synaptic strength increases, all the others must decrease by some amount. So a mechanism for DECREASING synaptic strengths by just the right amount is introduced (the decay method does the same thing with less magic).

But this doesn't cure the basic problem, which is that learning can't involve just INCREASES in "efficiacy." The very word "efficacy" implies that more weight is better; the most efficacious synapse is the one that conducts the most. If you use any sort of weighted-sum model, this is of course nonsense. In general you need both positive and negative coefficients, and to achieve any particular form of a function the model must make the weights converge on SPECIFIC VALUES, not maxima.

By introducing decay you can achieve non-saturated weights through a balance between opposing effects; one effect increases strengths while decay decreases it in proportion to the remaining strength (the second equation you show). But this is an ad-hoc solution. It would be much nicer if the method caused convergence toward the right weightings either from above or from below -- which suggests that some control process would provide a better model. In my artificial cerebellum algorithm, this is exactly what happens: the algorithm can be drawn in block-diagram form as a control system. I'm sure that won't come as a great surprise.

There's another subtlety here. In your description of Grossberg's model, you say

>In order for the synapse to increase in efficacy, both pre- and
>postsynaptic elements need to be on. If only the presynaptic element is
>on there is no change. If the presynaptic element is on and there is a
>small postsynaptic activity, then the efficacy begins to grow.

I'm referring to the word "on." The implication is that "activity" consists only in the neuron being in an "on" or an "off" state -- the old digital model again. I'm conceiving of the rule as involving continuous variables -- in other words, frequencies, concentrations, and variable potentials. This actually fits the above equations better, unless the "*" means "and." I hope we can get entirely away from considering "firing" as a behavior of a neuron unless we mean "rate of firing."

Now another problem:

Me:
> This says that during formation of a control system, the rule must
> be that activity on the downstream side of a synapse caused by
> injection of a secondary input must change the primary input so as
> to result in an equal and opposite effect downstream. The critical
> thing is that the effect of the secondary input downstream must
> result in the strength of the synapse changing in the way that
> gives the primary input the OPPOSITE effect downstream, so that
> the disturbance is resisted.

You:
>This paragraph was very difficult for me. Let's see if I sorted it
>out. After reading it about 50 times I just can't believe it is true.

I suspect the problem is in "activity on the downstream side of a synapse caused by injection of a secondary input must change the primary input...". This sounds as though I am saying that the synapse has to work backward. I just said it poorly. The downstream activity changes the primary input not backward through the synapse but by following effects further downstream, into output systems, through the environment, back inside through sensors, and finally back to the same control system's input signals (or wherever you are examining a neurone in the system). Have I identified the difficulty?

I'm not sure, however, that we have done away with this problem:

>Linear modeling in either the field of computational neuroscience or
>artificial neural networks has been largely abandoned except in a small
>number of relatively simple situations. I take both the control system
>model and the synapse model that you are describing to be either linear
>or nearly so.

Linearity is only a convenience, not a necessity, in the control-system model. The arm model with dynamics, that you will see in August if you haven't seen it already, is full of nonlinearities. In the newest version that Greg Williams and I have tried, the muscle length-force relationship (taken from nature) is described by a sixth-power curve! The model works better with this nonlinearity than without it. No special modifications of the model are needed to "deal with" this nonlinearity.

The rumor that control theory can only deal with linear systems has, I think, two origins. The first is that mainstream engineering control theory approaches this subject using analytical mathematics, which is extremely limited as a way of analyzing nonlinear systems. When the main method is simulation (as in the CSG work), this is no longer a problem.

if analog computing hadn't, for all practical purposes, turned into a lost art, nobody would be worrying about nonlinearities in control systems. Control systems eat up nonlinearities. In fact they linearize them.

The second reason is that in fields peripheral to engineering control theory, some very wrong concepts of how control works have arisen. One common misconception is that a control system has to compute output -- that is, take in information about error, calculate the physical outputs that will effect the environment in the way required to correct the error, and then execute the computed output. This is just an adaptation of S-R theory so that it seems to apply to a closed-loop system. With this conception of control, there are enormous problems when the system itself or its environment are markedly nonlinear -- you have problems with double-valued functions, for example. And the computations needed to compensate for disturbances and nonlinearites get so complex that the whole model ceases to be understandable or analyzable.

>I think there may be a flaw in your reasoning. First of all, is
>there any strong reason to believe that the control system is a
>closed loop which must feed back onto that particular synapse?

That depends. If you think of control loops as consisting of multiple redundant signals working in parallel, with each function-box representing an aggregate of function-boxes performing similar computations, then what you suggest is not only possible but probable. It doesn't even matter if a few of the loops are incomplete or even have the wrong sign of feedback. I brought this up in my '73 book, and I believe it was even mentioned in the '60 article with Clark and MacFarland. The nervous system doesn't have to be perfect to implement the kinds of systems I envision.

On the other hand, if we consider the aggregate loop as a single loop, yes: the loop must be closed if there is to be control. The net effect must be negative feedback.

>Is it possible that the disturbance is resisted by rerouting?

Show me how that would work and I'll believe it.

>You are assuming that, in a closed loop, the primary signal and the
>secondary signal are both going to be active and are going to sum in a
>nearly linear way. The primary signal is being trained as something of
>a replacement for the secondary signal. It is to be a predictor
>designed to obviate the need for the secondary signal in certain
>situations.

The first sentence, yes, except for the linearity aspect. Linearity is not required. The second part, no. I'm not trying to model Pavlovian conditioning. I'm trying to model control. Control does not entail prediction, in general (although at higher levels, it is possible to control the state of a signal that is itself a predicted state of lower-level signals). Think of my "comparator" example above. The perceptual signal is not a "replacement" of the reference signal. The system makes the perceptual signal match the reference signal (give or take scaling

factors).

All the interpretations you are suggesting are based on a completely different conception of what behavior is and how it works. Remember that in my model I need to REVERSE the Hebb Rule to make the feedback come out negative. You really have to choose between behavior as response and behavior as control: there's no compromise.

>I am leery of instantiating control loops at the level of synapses.

Perhaps by this point in this post you understand that I am not trying to do that. I am seeing synapses as components in a much larger control loop.

There are, however, some very interesting possibilities for interpreting processes WITHIN small neural nets as control processes. Even just negative feedback from an axon to the dendrites of the same neuron can be interpreted in some possibly productive ways. If we use frequency as the variable, this sort of feedback can actually govern the overall input-output transfer function of the neuron, completely hiding the "forward" characteristics. See any treatise on operational amplifiers and their use in analog computing for further elucidation. The nonlinearities in the forward transfer function can be cancelled by similar nonlinearities in the negative feedback path. Furthermore, if the negative feedback path has dynamic characteristics, they can appear in the overall input-output function as the INVERSE of those characteristics. If the feedback signal's frequency is roughly the time integral of the axon output frequency, the axon output will be roughly the first derivative of the (net) input signal to the dendrites. And so on. but you have to think analog to see how this stuff works: you don't get it from studying what creates a single firing of a neuron.

>It is entirely possible that I have totally misunderstood you. My lack
>of detailed knowledge of CSG-type control systems may be leading me into
>misinterpretations.

Problems in communication have to be solved at both ends. I'm willing as long as you are. It isn't necessarily a trivial problem even to discuss where to meet for lunch (and actually meet at the same place). What we're trying to do is quite a bit harder than that.

I'm having fun. I hope you are, too.

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Date: Sat, 8 Jun 91 10:01:17 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: For fun

[From Bill Powers (910608.1000)] --

Found this on an old disk -- just for fun.

HIAWATHA DESIGNS AN EXPERIMENT
by

Maurice G. Kendall

Hiawatha, mighty hunter
He could shoot ten arrows upwards
Shoot them with such strength and swiftness
That the last had left the bowstring
Ere the first to earth descended
This was commonly regarded
As a feat of skill and cunning.

One or two sarcastic spirits
Pointed out to him, however
That it might be much more useful
If he sometimes hit the target
Why not shoot a little straighter
And employ a smaller sample.

Hiawatha, who at college
Majored in applied statistics
Consequently felt entitled
To instruct his fellow man on
Any subject whatsoever
Waxed exceedingly indignant
Talked about the law of error
Talked about truncated normals
Talked of loss of information
Talked about his lack of bias
Pointed out that in the long run
Independent observations
Even though they missed the target
Had an average point of impact
Very near the point he aimed at
(With the possible exception
Of the set of measure zero).

This, they said, was rather doubtful
Anyway, it didn't matter
What resulted in the long run
Either he must hit the target
Much more often than at present
Or himself would have to pay for
All the arrows that he wasted.

Hiawatha, in a temper
Quoted parts of R. A. Fisher
Quoted Yates and quoted Finney
Quoted yards of Oscar Kempthorne
Quoted reams of Cox and Cochran
Trying to impress upon them
That what actually mattered
Was to estimate the error

One or two of them admitted
Such a thing might have its uses
Still, they said, he might do better

If he shot a little straighter.

Hiawatha, to convince them
Organized a shooting contest
Laid out in the proper manner
Of designs experimental
Recommended in the textbooks
(Mainly used for tasting tea, but
Sometimes used in other cases).
Randomized his shooting order
In factorial arrangements
Used in the theory of Galois
Field of ideal polynomials
Got a nicely balanced layout
And successfully confounded
Second order interactions.

All the other tribal marksmen
Ignorant benighted creatures
Of experimental setups
Spent their time of preparation
Putting in a lot of practice
Merely shooting at a target.

Thus it happened in the contest
That their scores were most impressive
With one solitary exception
This, I hate to have to say it
Was the score of Hiawatha
Who, as usual, shot his arrows
Shot them with great strength and swiftness
Managing to be unbiased
Not, however, with his salvo
Managing to hit the target.

There, they said to Hiawatha
That is what we all expected.

Hiawatha, nothing daunted
Called for pen and called for paper
Did analyses of variance
Finally produced the figures
Showing beyond peradventure
Everybody else was biased
And the variance components
Did not differ from each other
Or from Hiawatha's.
(This last point, one should acknowledge
Might have been much more convincing
If he hadn't been compelled to
Estimate his own component
From experimental plots in
Which the values all were missing
Still, they couldn't understand it
So they couldn't raise objection

This is what so often happens
With analyses of variance).

All the same, his fellow tribesmen
Ignorant benighted heathens
Took away his bow and arrows
Said that though my Hiawatha
Was a brilliant statistician
He was useless as a bowman
Several of the more outspoken
Made primeval observations
Hurtful to the finer feelings
Even of a statistician.

In a corner of the forest
Dwells alone my Hiawatha
Permanently cogitating
On the normal law of error
Wondering in idle moments
Whether an increased precision
Might perhaps be rather better
Even at the risk of bias
If thereby one, now and then, could
Register upon the target.

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Date: Sat, 8 Jun 91 13:50:06 +0300
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: Hiawatha

Bill on Hiawatha (910608),

Pretty amusing. People often begin a thesis or dissertation with a quote. I wonder what people would think of an entire poem?! (Gary; Fred...?)

>I think one problem is that there are no "succeeding generations." Whatever understanding of >human problems there is is presented simultaneously to one-year-olds and to ninety-year-
>olds, and it's presented by, say, 12-year-olds through eighty-year-olds >(want to leave myself a safe margin there).

I don't think I understand why you say knowledge is presented "simultaneously." There is unitary-ness to one's environment, but our (higher level) perceptions develop over time and change slowly, don't they? Schooling, as one arena for the presentation of "human problems," certainly isn't simultaneous. What do you do with sentiments such as "history repeating itself?" I'm trying to find out how we might direct one to "learn from others' mistakes" at least as far as possible regarding deadly and damaginb one. We do throw out principles to live by but with the hope that one then develops certain systems concepts. "Anything goes" doesn't work well in a society. Anyway I'm finding it more and more difficult to conduct this discussion over e-mail. Apart from the problems in dealing with higher level perceptions, the delays in the conversation affect my train of thought (such as it is). So I hope again there can be a few moments of face

to face(s) discussion of this--perhaps including some demonstration of the "game" you described a while back regarding moving up levels in a conversation.

P.S. I keep thinking that it would be an interesting project for someone to take a Lakoffian (George of _Women, fire, and dangerous things_ fame) view of many of the phrases we use from a CT viewpoint (e.g. "learn from our mistakes") It would make a fascinating psycholinguistic study.

Joel Judd

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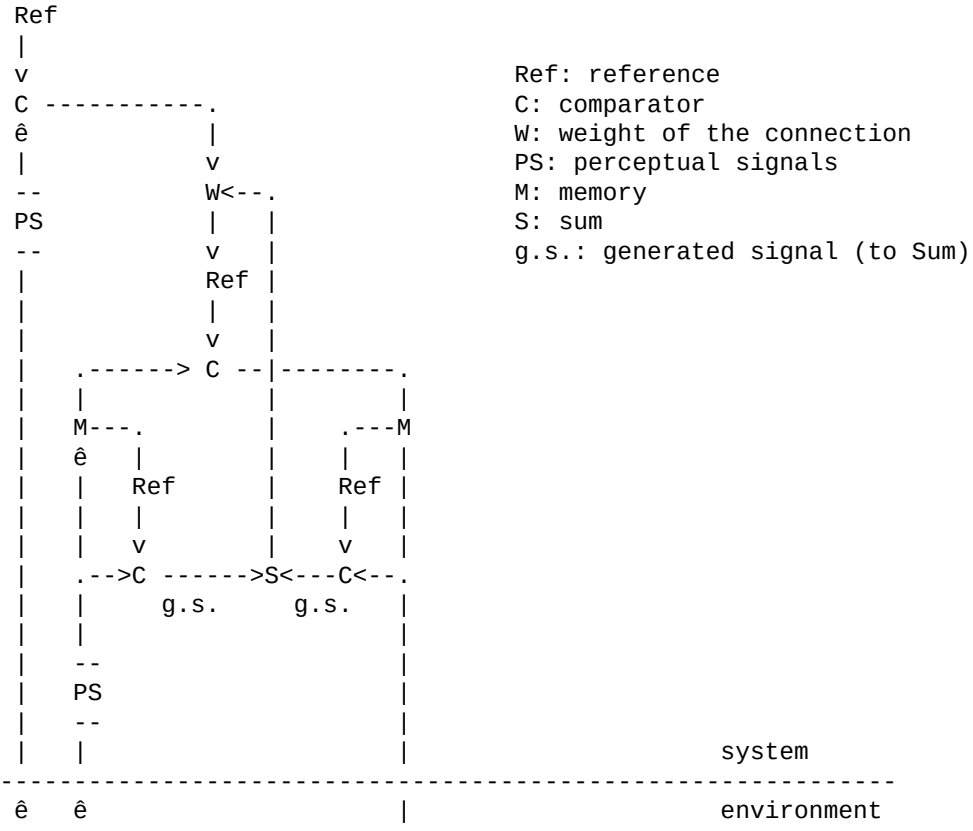
Date: Fri, 7 Jun 91 12:44:49 BST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mar@CS.ABER.AC.UK
Subject: Re: Tom Sawyer; Hebb's Rule

[from Marcos Rodrigues]

Bill -- (910607)

I'm trying to understand your modification of Hebb's rule. I have built a model of it based on the anticipatory phenomena model I've posted some time ago (re. classical conditioning).
Is it something like this you are talking about?



```
      |   |                    v
      |   primary input        output
      |
      secondary input
```

A small table can be built as well:

```
-------------------------------------------------
primary input ->  generated signal  <- output
--------------.---------.-----------.------------
increasing         -    |     -       increasing
decreasing         +    |     +       decreasing
decreasing         +    |     -       increasing
increasing         -    |     +      riba del decreasing
-------------------------------------------------
Sum = generated signal(input+output) at each control cycle.
-------------------------------------------------
```

According to the sign of the Sum, the weight of the connection
is increased or decreased. Does it help or are you thinking of
something rather different?

Best regards,
Marcos.

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

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Date: Sat, 8 Jun 91 17:37:28 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: New Model

[From Rick Marken (910608)]

I set up a two level version of the new model, with two control
systems at each level. I set it up as an Excel spreadsheet. What
I learned (I think) is that the reference signal that enters into
the computation of the level 2 perceptions (p2=f(r1-e1)) must be
the reference signal that is the summed result of outputs at level
2. My version of the model controls two level 2 variables, a sum
p2.1 = p1.1+p1.2 (where the first number after the letter is the
level of the variable and the second is the system at that level)
and p2.2 = p1.1-.1.2. Each level two system contributes an
output (o2.x) to the level 1 reference (r1.x) so:
r1.1 = o2.1+o2.2 and r1.2 = o2.2-o2.2. It is these summed reference
signals, r1.1 and r1.2 that are used as the inputs to the level 2
comparators, after the level 1 error signals from all level 1 systems
are subtracted from that reference. Sounds complex but it really
isn't. I have not yet tried making it so that all signals are
positive. I want to set it up so that, when a signal goes negative
it remains at zero, thus functionally removing the system from the
hierarchy while this is true.

The problem now is to find a test that would discriminate the new from the old model (in terms of behavior). My guess is that this will require some kind of test involving two levels of control, since the interesting aspect of the new model is that it uses error signals from the lower level to see how its doing. This will work for all levels, of course, except for the lowest -- the environment does not provide error signals. Perhaps the perceptual signals at level 1 work like -- nope, they can't be error signals. They will not be zero when the input intensity matches the reference.

Boy, we sure move quickly from the Ozone to the nitty gritty.

Best Regards

Rick M
marken@aerospace.aero.org

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Date: Sun, 9 Jun 91 10:31:08 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Re: New Model; Hebb's Rule

[From Bill Powers (910609.0930)

Rick Marken (910608) --

>I set up a two level version of the new model, with two control
>systems at each level.

When I said someone might beat me to it, I didn't think it would be the next day. Wow. This is good news, Rick. In hindsight, of course the feedback model at level 2 has to include the effects of all relevant level-2 outputs. I hadn't anticipated that. To generalize, EACH system at level n has to perceive the output effects of ALL systems Sn.m that affect the perception in question (I admire your new notation and will immediately adopt it). This suggests a reason for the existence of sensory nuclei and sensory areas, doesn't it? Maybe? It would be very wasteful to replicate n signals m times. If all the input functions at one level shared computations in one volume -- well, I can't visualize it yet, but as you explore the new model maybe you'll see how the separate computations could be merged, providing multiple outputs that are different functions of the same set of inputs but not duplicating signals.

I am really astonished at how fast you got that model working.

Joe Lubin (910606 and previous) --

I think I got a little too clever with the Hebb Rule. When I try to see how a model of my version might work, nothing falls into place. It's particularly useless for a perceptual function and an output function. Even for a comparator, just "decreasing the weight" when there's positive feedback doesn't give the right effect. I think that this was one of those ideas that looks great when it first pops into one's head. This

network makes it too easy to go public with ideas that ought to simmer for a few days to see if anything survives.

I hope nobody put in too much time trying to give me the benefit of the doubt. This is a case, I think, where the top-down approach is the wrong one: the problem of how the net gets organized is mainly a bottom-up problem (constrained, of course, by top-down observations of phenomena -- I'm not about to give THAT up). I still don't think Hebb's Rule is the right one, but I think that some of the proposals being explored in the name of that rule (lateral inhibition, etc.) are probably headed in the right direction, at least for perceptual functions. I think I'm going to leave that level of research to people who know what they're doing (unless I get another great idea that just HAS to be right).

While going through the problems of making my idea work, I did come up with one possibly useful concept. It developed from the idea that there are really three kinds of organizational problems involve in the self-organization of control systems:

One is the perceptual problem: multiple inputs entering a functional box, one output leaving it. How do the computations in the box come into being such that the single output is a useful function of lower-level signals? EVERY function will produce an output signal, but not all such output signals would prove to be controllable, or even if controllable, useful to control.

The second is the comparison problem: two inputs with OPPOSITE signs entering a single box that emits the difference-signal. This confluence of afferent and efferent signals with opposite signs is such a special requirement that I think it needs genetic predisposition. You just have to have the right signals coming together in the right place, and one has to be inhibitory while the other is excitatory (comparators can work either way). From the little I know of the nervous system, it seems that comparators are pretty much set up in the basic design (spinal cord, brain stem, midbrain at least) and are physically located in or near motor centers. This is the only place where my version of the Hebb Rule would even have a chance -- and here, it's probably not needed.

The third is the output "fan-out" problem. For all the loops to be closed and for all (or most) to be negative feedback loops, the output signals from a given level must reach the same lower systems that are concerned with producing the perceptual signals associated with the same output signals. Each signal in the output arborization must also either make its connection directly or through an inverter neuron, to keep the feedback negative in each loop.

So the basic problem is really the perceptual one: how do meaningful useful perceptual signals come to be computed? Once a perceptual signal exists, the other two problems have to be solved in a way that is far more constrained. The comparators have to receive copies of the perceptual signals, so the axons carrying those signals simply have to find a nearby comparator input to hook up to. The outputs of the comparators have to find their way to a set of lower-level reference signal inputs, via fan-out (and probably computational) nets. So upgoing afferent signals have to find their way to perceptual inputs of

comparators at the same level and downgoing output signals have to find their way to reference inputs of comparators lower down. Maybe comparators emit "pheremones." There has to be some help from evolution here, I think.

Sorry about the diversion, but I guess that's part of getting our axons hooked up to the other person's dendrites in the right way.

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Date: Sun, 9 Jun 91 12:11:51 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Rheostasis

[From Bill Powers (910609.1100)]

Gary Cziko et. al.

I obtained Nicholas Mrosovsky's book *Rheostasis* (New York: Oxford University Press, 1990) and have been reading it with pleasure. Mrosovsky has carried his analysis of biological control systems to about the point where Clark and I were with overt behavioral systems in the mid-1950s, just as our breakthroughs in understanding were beginning to occur. I don't say that from a sense of superiority, but out of recognition of a fellow-explorer working a different terrain and beginning to make sense of it in the same sequence of steps we went through -- and arriving at the brink of the same understandings for the same reasons.

If this book accomplishes nothing else, it will be the ideal source for persuading biologists to give up their abhorrence of the idea of reference levels. The chapters on Programmed Rheostasis and Reactive Rheostasis contain innumerable examples of set-points that change. The examples in this book, in general, will be a rich lode for control theorists.

What Mrosovsy must learn next is the concept of hierarchical control. At one point in his book he rejects the notion of "strict hierarchy," but he construes "hierarchy" in the sense of priorities among controlled variables at the same level. "Suppose, for example, that water balance were top in the hierarchy, then a starving animal that had only a modest water deficit would look for water rather than food. On telological grounds, strict hierarchy will not be considered further." (p. 21).

I hope this conclusion, which is correct, does not leave Mrosovsky convinced that the very idea of hierarchy has been dealt with once and for all so that a more productive concept of it will be rejected without further consideration. He trembles on the verge of seeing how hierarchy works in control system assemblies, but isn't quite there yet.

One way he approaches it is through "feed-forward." He correctly defines feed-forward as reacting to a disturbance rather than correcting error. But all of his examples involve disturbances of a kind that would need a higher-order control system in order to be resisted. In many examples, the response to the external disturbance results in resetting the reference signal for a lower-level system (as we would see it). Instead

of asking how the organism must be organized in order for this appropriate "feed-forward" to take place, he treats it simply as a reaction.

The same half-blind analysis (how well I remember doing exactly the same thing) is used in the concepts of "programmed" rheostasis and "reactive" rheostasis. It hasn't occurred to him to see the programming as evidence of a higher level of control -- even though he recognizes that the outcome of this programming is a variation in the reference-level of a lower-level control system, and not simply a behavior (an example he uses is hibernation, which involves a well-documented change in temperature set-point and not, as claimed by many others, a failure or loss of gain in the temperature regulation system). In the idea of "reactive" rheostasis he comes even closer, for he has the organism reacting to changes in external conditions by altering set-points.

The war between the old SR viewpoint and the new CT viewpoint is obvious, or would be to anyone concerned with current behavioral control theory. On the one hand, Mrosovsky's grasp of the principles of control is very good, avoiding most of the misconceptions that are rife in the literature (the one exception being feed-forward, and even there he does not see it in an unreasonable way). On the other hand, the idea of "causal variables" is still important in his arguments. Now, however, these causal variables operate one level up from the level at which the control systems work: the causal variables cause changes in set-points! This is the basis of "reactive" rheostasis.

Chapter 6 is about "second-order rheostasis. He says "But then these superimposed controlling systems themselves become a target for still further controls, and the possibility arises for second-order rheostasis, that is for modulation of the way or the rate at which rheostasis is altering the set-point of the basic regulatory systems." He does not see the potential of this idea, because the pattern of regulatory output setting reference levels, and of higher-order controlled variables being build out of lower-order variables, is simply missing. The examples in this chapter are ponderous and complicated, and involve too great a gap between the lowest level (i.e., biochemical regulation) and the highest (timing of developmental changes in at puberty). There is no conception of an experimental approach to testing definitions of controlled variables, a lack that leads Mrosovsky to some improbable conceptions of relationships among control systems. Nevertheless, Mrosovsky is clearly on the brink of seeing how hierarchical control works, and his path to the brink is uncannily like the one I remember taking.

In Chapter 7, Mrosovsky demonstrates his understanding of basic control-system operation in a boxed section (pp.127-128) where he derives the relationship between loop gain and the diminution of the effects of disturbances on a controlled variable. But he fails to arrive at the basic relationships of control, which Rick Marken just recently expressed as "p = r and o = -d/k." I think that taking the step from "reactive rheostasis" to these two relationships is equivalent to taking the final step out of the world of stimulus and response and into the world of control theory. If Mrosovsky follows the same timetable that Clark and I did, he should see the light in about two or three years.

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Date: Sun, 9 Jun 91 17:33:28 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Kenneth_Latta@UM.CC.UMICH.EDU

Subscribe csg-l Ken Latta

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Date: Sun, 9 Jun 91 17:07:28 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Bad News for new model

[From Rick Marken (910609)]

Well, I better post this before people get too excited about the new model. I know I was starting to fall in love with it myself. Perhaps my love for it can be saved but, while playing with it today (I tell ya Bill, spreadsheet modeling is really quite handy and fast for first approximations) I noticed what appears to be a BIG problem. I didn't notice it at first because both levels of my model were really controlling different aspects of the SAME variable. If both levels are not controlling the same variable, there seems to be definite problems for the "imagination control" model. The problem stems from the fact that the model is controlling a perception defined by a reference signal sent to a lower level system. For example, suppose that the level 2 systems are controlling perceptions of the sum and difference of the length of lines on a computer screen. To to this, they send references to the level 1 systems that I had originaly thought of as controlling the lengths of the component lines. In this case the model works becase the two level two systems are controlling line length and they are sending references for line length to the level 1 systems to do this. However, suppose that the level 1 systems are controlling the position of the handle that affects the line. Now the level two reference signals are specifying the handle positions that produce, via the environment, the appropriate line lengths to satisfy the level 2 references for sum and difference. But in the imagination model the references for handle position are used by the sum/difference perception systems -- along with the error from the handle control systems -- as the feedback. The result is that the sum difference system perceptions have nothing to do with the variables "out there" that it is controlling -- the sums and differences. I don't think this is just a scaling problem; but a very deep conceptual problem with the imagination model. I hope I'm wrong. Bill, if you understand what I'm saying could you set me straight (if I'm wrong).

Rick M.

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Date: Sun, 9 Jun 91 18:47:12 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Re: New model problem

[From Bill Powers]

Rick Marken (910609) --

It may be a bug or a feature:

>For example, suppose that the level 2 systems are controlling
>perceptions of the sum and difference of the length of lines on a
>computer screen. To to this, they send references to the
>level 1 systems that I had originaly thought of as controlling the
>lengths of the component lines. In this case the model works becase
>the two level two systems are controlling line length and they
>are sending references for line length to the level 1 systems to
>do this. However, suppose that the level 1 systems are controlling
>the position of the handle that affects the line. Now the level
>two reference signals are specifying the handle positions that
>produce, via the environment, the appropriate line lengths to
>satisfy the level 2 references for sum and difference.

In the second case (Level 1 controls handle), you're asking the second-level systems to add a line-length variable to a handle position error, while what is needed is a line-length (or f(linelength)) error. This can't work because there is no necessary relationship between handle position and line length, sum of lengths, or difference of lengths (due to external disturbances, which I assume you used as always).

In my original conception of this model, higher systems receive copies of perceptual signals from the same lower systems to which they send reference signals. So an f(linelength) controller would send reference signals to linelength controllers. Then the units come out right for the new version of the model to work, too, as you apparently have found.

But if the higher-level system is thought of as controlling perceptions that are NOT derived from the same lower-level perceptions being controlled, the higher perception depends on reference signals sent to the lower in ways that are unknown to the system (and is subject to disturbances that do not affect the lower-level perceptions). Unknown properties of the environment get into the loop. In the second version of your model, you're effectively assuming that there is a necessary regular relationship between handle position and line length. That could be true only if there were no disturbances. Then a scaling factor applied to handle position error would be equivalent to line-length error.

I don't think that the control hierarchy ever depends on the existence of regular or predictable relationships between action and result, beyond preservation of the sign of effect (and as we know, even that can change, and the system can cope with the change). Your first version of that model conforms with that principle; the second doesn't.

Keep 'em coming.

I'm not saying that this is impossible in a natural system, but I suspect that it is unlikely for the very reasons you found in your model testing.

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Date: Sun, 9 Jun 91 19:01:22 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: More New Model Questions

[From Rick Marken (910609b)]

Bill Powers (910609)

I think I understand -- but the constraint of having the level n system control a variable that has the same units as the level n-1 system does not seem to apply to the original model. I set up parallel models -- new and old. In both models, level 2 controlled line length and level 1 controlled handle position. In both models, the level 1 system perceived only the handle position, which as influenced by a disturbance. The level 1 systems were "conventional, perceptual control systems" in both cases -- they controlled the perceived position of the handle relative to their reference inputs. In the standard model, the level 2 systems perceived only the sum and difference of lengths -- not the mouse. It worked just fine. In the new model, the level 2 systems perceived only the sum of the reference to level 1 (which, as you noted, is silly because it is a reference for a differnt kind of variable) and the level 1 error (also a handle position variable). Of course, the model doesn't work -- in the sense that the actual values of the sum and difference do not correspond to the intended values (which the model imagines itself to be controlling).

So that's my problem -- the original model seems to isolate one level from another (in a sense)-- the references to level 1 do no need to take into account the kind of variable they are used to control at the higher level. This is not true of the imagination model. This might be a feature (rather than a bug). But then I want to know how to model tracking with the new model. Am I misunderstanding something. I really like the new model because it seems like a very elegant solution to the problem of how people continue apparently controlled behavior when some of the percpetual information goes away. The original version of the imagination model did this great. The problem was that because the lower level and higher level were controlling the same thing, the system was still actually controlling when it was blinded (when I eliminated the error signal connection from level 1 to 2. I don't think this is really what happens when I carry a glass of water in the dark. I can change the reference for my position based on controlling myself relative to my imagined perception of the room. But if a kid left a chair in a different place I'm dead meat.

Hasta Luego

Rick M.
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Date: Sun, 9 Jun 91 23:31:57 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Re: Rheostasis

From Tom Bourbon
Bill Powers [090691] described his reactions to *Rheostasis: The physiology of change* bu Nicholas Mrosovsky. I have also just finished reading the book (first mentioned by Gary Cziko, who learned of it from Elizabeth Satinoff). I share most of the impressions described by Bill, excluding of course his personal recollections of having been at a similar point in his thinking in the 1950s!

Mrosovsky describes many examples of what can be taken as the resetting of set points in biological control systems. He also presents alternative explanations that might obviate the need to invoke set points for some of the phenomena.

Like Bill, I was impressed by the boxed material on pages 127-129, in which Mrosovsky correctly derived the loop gain of a control system and showed correctly the proportion by which a given loop gain would reduce the effects of a given dirturbance acting on a controlled variable. On that count, there is no problem.

But the derivation is never cited or discussed in the text. It is as though it were an afterthought, or something that seemed important to include, but that didn't really fit in the flow of ideas in the book.

The only other place where a control system is disgrammed or discussed is very early in the book, Again, the details of a control system and how it functions are not discussed that often in the text. As Bill remarked, Mrosovsky seems "just on the brink" of putting it together.

In the early pages of the book, Mrosovsky discusses the classical idea of homeostasis and describes the popular invocation of negative feedback as the means by which homeostasis is achieved. I am not certain how Mrosovsky conceives of negative feedback, because it is several pages later that he first introduces the idea of a set point, as a way of overcoming problems associated with the appeal to negative feedback. It could be that some of the authors he is addressing used the idea of negative feedback as a verbalism, like many of the uses reviewed by George Richardson in his book, *Feedback thought in social science and systems theory*. Many people write of negative feedback in a very general way, with no firm idea of a mechanism that can achieve control via negative feedback.

I think that is also why Mrosovsky so quickly calls in the ideas of "positive feedback" and of "feedforward." For a person who hasn't come across the idea of control by a hierarchical negative feedback system, those ideas seem almost essential, but the phenomena they are intended to explain can be handled effectively by hierarchical perceptual control systems.

An interesting book, filled with good examples -- some of them downright fascinating (in the chapter on "Resolving conflicts" between set points there is a description of a

study in which rats were housed in a warm "home," and could
eat all they wanted in a "restaurant" that was "open" only
two hours a day. The home was warm, the restaurant VERY cold.
Read the book (pages 16, 17) to learn how cleverly the rats
worked their way around those conditions!

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==========
Date: Mon, 10 Jun 91 07:21:51 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Re: new model

[From Bill Powers (910610.0700)]

Rick Marken (910609) --

>In both models, level 2
>controlled line length and level 1 controlled handle position. In
>both models, the level 1 system perceived only the handle position,
>which as influenced by a disturbance. The level 1 systems were
>"conventional, perceptual control systems" in both cases -- they
>controlled the perceived position of the handle relative to their
>reference inputs.

"The" handle position? I presume you mean plural: two degrees of freedom of output are needed to control two degrees of freedom of input.

If the level-2 systems are controlling l1 + l2 and l1 - l2, respectively, then you need two level-1 systems, one controlling l1 and the other controlling l2. Each level-1 system uses a handle (or mouse coordinate) to control its own perception. System 1 varies h1 to control l1 against disturbance d1; System 2 varies h2 to control l2 against disturbance d2. I don't think you meant to "control" the handle positions (not unless you introduced still another lower level of control).

At level 2, the old model says that r1.1 = o2.1 + o2.2 and r1.2 = o2.1 - o2.2, where o is the output signal (k * integral of error). The perceptual signals are p2.1 = p1.1 + p1.2 and p2.2 = p1.1 - p1.2. Of course here p1 = l1 and p2 = l2.

The new model says that the perceptual signal in the first level-two system, p2.1, is equal to the output signal o2.1 minus the sum of the two lower-level error signals:

p2.1 = o2.1 - (e1.1 + e1.2).

Similarly,

p2.2 = o2.2 - (e1.1 - e1.2).

The relationships between o2 and r1 make it seem that the weightings are being applied in the output part of the system. That's just because you're controlling l1+l2 and l1-l2. To show that the input weightings are the only ones that have to be adjusted, you could control al1+bl2 and cl1 + dl2, where a,b and c,d are chosen to keep the two perceptions from being linearly dependent (for example: 2,3 and 3,2). Then these weights would also have to be applied to the error signals being passed upward.

Are we converging?

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Date: Mon, 10 Jun 91 07:57:44 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Re: Mrosovsky & set-points

[From Bill Powers (910610.0800]

Tom Bourbon (910609) --

In your comments on Mrosovsky, you say

>He also presents alternative explanations that might obviate the need to
>invoke set points for some of the phenomena.

The method he uses is one that other biologists also use. Lovelock resisted my suggestions about reference levels using the same argument, and I've seen it elsewhere as well.

Suppose you have two relationships between variables x and y (imposed by different physical paths). In one, the plot of x against y increases to the right; in the other, it decreases to the right. The two lines cross, and at the crossing point is the state of x and y that satisfies both relationships. This is the "pseudo set-point" in question: an effective set-point without a reference signal, or so they say.

What is omitted is any explanation for why these lines do not go through zero. In every case I've seen, there is an offset of both lines so that the intersection takes place somewhere in the first quadrant. The "constant" determining the offset isn't discussed. It is, of course, equivalent either to a disturbance or a reference signal (depending on which curve you mean). If the constant varies, that is like varying the disturbance or the reference signal.

But that isn't quite enough to dispose of the matter. The way these plots are drawn, it's clear that the loop gain is very low. I estimated it for Lovelock's Daisyworld model, and it was about 1. Of course with that low a loop gain, it doesn't matter whether you call this a control system; it's not going to control anything very well and disturbances are not going to be resisted much unless you sneak an integral into one of the equations (as Lovelock did).

One of the two lines on such plots must have a very steep slope in order for significant control to exist. And in that case, the intersection point is determined almost entirely by the line with the steep slope. The

offset in the equation for that line is then exactly a reference signal, and determines the state of the controlled variable that exists with no disturbances (disturbances being the offset of the other line).

So the whole thing is rather silly: the control equations are presented in graphical form, as a proof that this process isn't control. Maybe we should draw the control equations for biologists as two intersecting curves (as I did in the '71 Rat Paper -- maybe that's why it was accepted!). Output versus input via organism for one curve; input versus output via environment for the other.

Mrosovsky did propose a second way of eliminating reference signals. This way invokes two opposing effects without a closed loop. This method, of course, is unable to resist disturbances of the output.

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Date: Mon, 10 Jun 91 09:06:00 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Re: new model

[From Rick Marken (910610.0800)]

Bill Powers (910609.0700) --

>"The" handle position? I presume you mean plural: two degrees of freedom
>of output are needed to control two degrees of freedom of input.

Of course, there were two handle control (or line control, if I change the perceptual input) level 1 systems.

>If the level-2 systems are controlling l1 + l2 and l1 - l2, respectively,
>then you need two level-1 systems, one controlling l1 and the other
>controlling l2. Each level-1 system uses a handle (or mouse coordinate)
>to control its own perception. System 1 varies h1 to control l1 against
>disturbance d1; System 2 varies h2 to control l2 against disturbance d2.
>I don't think you meant to "control" the handle positions (not unless you
>introduced still another lower level of control).

I did leave out a level, I suppose. In both cases the level 2 systems control l1+l2 and l1-l2. In the "line control" case, system 1.1 senses l1 and system 1.2 senses l2. l1 = h1+d1 and l2 = h2+d2. In the "handle control" case, system 1.1 senses h1 and system 1.2 senses h2. I can make it so that the disturbances still influence only the line lengths, as before, or they can influence the handle directly so that p(hx) -- the level 1 perceptions
of the positions of h1 and h2 -- is hx+dx. In the old version of the model, both versions of the model work -- the handle control case works because the level 2 systems (controlling the sums and differences) send references to the handle control systems that have (unsensed) effects on l1 and l2 that produce the appropriate results on the sums. The disturbance can be applied to the handle positions or the line lengths -- the old model works in both cases -- because the disturances (to handles or lines) have effects on the level 2 variables -- and the control systems adjust the references for the level 1 systems (be they controlling lines or handles)

appropriately to get the intended level 2 result. The "handle control" approach does not work with the new version of the model because the new model does not perceive the sums and differences. So, when it sends references to the level 1 systems controlling the handles, it ends up producing
the correct numbers for the handles (that would produce the correct sums and differences) but, due to disturbances, these handle values are not producing the correct results for l1 and l2 -- so the sums and differences are wrong.

I agree that the new model will work if I add an extra level -- when I do the handle control version of the model. In fact, I'll try it tonight. I'll have l1+l2 and l1-l2 be controlled at level 3, l1 and l2 controlled at level 2 and h1 and h2 controlled at level 1. This is obviously the correct way to do it -- the "handle control" version I described above assumes that the controller is directly controlling the sums and differences, not the individual line lengths that make up these variables. The correct version of the new model (directly controlling l1+l2 and l1-l2) is just a single level model -- so it would be equivalent to the old model anyway (because you can't have errors from a lower level if there is no lower level).

>The new model says that the perceptual signal in the first level-two
>system, p2.1, is equal to the output signal o2.1 minus the sum of the two
>lower-level error signals:

>p2.1 = o2.1 - (e1.1 + e1.2).

>Similarly,

>p2.2 = o2.2 - (e1.1 - e1.2).

I used these equations at first but for some reason didn't find them satisfactory. Now I remember -- if you are controlling sums and differneces at level 2 the reference inputs to level 1 must be the appropriate sums and differences (remember your Byte article?). I found that the following equations work just fine for a two level system with system 1 controlling l1+l2 and system 2 controlling l1-l2.

p2.1 = r1.1 - (e1.1+e1.2)

where r1.1 = o2.1+o2.2

and

p2.2 = r1.2 - (e1.1-e1.2)

where r1.2 = o2.1 - o2.2 (NB. Minus sign)

>Are we converging?

Yes indeed. I think the problem is that I have never actually done two level research (with continuous variation of the level 1 by the level 2 system). I have an idea for a simple experiment that seems to involve a couple levels and might be a good test of the new model versus the old one. I think this new model has some fascinating implications. But it might be

a bit challenging to develop the critical experimental tests.

I, of course, will provide status reports as I progress.

Tom Bourbon -- I think this new model might be a better topic for me to do at the meeting than my proposed one. I know we don't hold people to their proposed topics. But I just thought I'd let you know -- I will probably discuss this work on the "control of expectation" model.

Hasta Luego

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Date: Mon, 10 Jun 91 12:36:36 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Heterarchy

[from Gary Cziko]

I forwarded Bill Powers's (910609) comments on Mrosovsky's book to Evelyn Satinoff who recommended it to me. Here are some of her comments about a heterarchy. Anybody know what this is and how it might relate to control systems?--Gary

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>By the way, when I was seriously involved in thinking about all this stuff
>several years ago, I reached the conclusion that 'hierarchy' was not the
>way to think about interactions between various physiological control
>systems. Rather, I cam to the idea of a 'heterarchy', but could not find
>anything like it in the literature (except for an article by Don Broadbent,
>as I recall, and he may have written more on this but I didn't pursue it)
>and never continued with it. But it made (and still makes) a great deal more]
>sense than a hierarchy when more than one system is involved.
>
>

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Date: Mon, 10 Jun 91 18:06:51 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: RE: New model; heterarchy

[From Bill Powers (910610.1800)]

Rick Marken (910610) --

I think I understand now. When the handle is the lower-level controlled variable, disturbances of lines don't get corrected (with the new model) because they're not represented in the perception of handle position or in handle position error. Right?

I'm probably taking words out of your mouth, but it looks to me as though we have two distinct cases here. We might call one hierarchical control and the other instrumental control (not mutually exclusive). Hierarchical control involves controlling functions of perceptions that come from lower in the same hierarchical tree (we need a word for the inverse of tree, meaning "fan-in" but not so ugly). Instrumental control involves controlling functions of one set of perceptions by controlling a different set of perceptions that is in a different hierarchical tree. The link between the "output" set and the "input" set is arbitrary in instrumental control, because in principle there can be invisible links between any two kinds of environmental variables.

The sums and differences of line lengths are in the same hierarchy as the line lengths individually. Hence a disturbance that disturbs either line length creates errors that can be used to correct the sum and difference imagination signals at the sum/difference level.

Handle control, however, controls handle position as sensed, while actual handle position alters line lengths through an arbitrary environmental link that's not represented in perception. The sum and difference controlling systems get their perceptions from line lengths (in one or two stages), a visual input. If the visual input is disturbed by altering line length, the sum and difference signals will be appropriately altered and the reference signal for handle position (kinesthetic input) in the other hierarchy will be appropriately changed. However, handle error is not appropriate for correcting line-length error: two different trees.

The conclusion seems to be that you can use model-based or imagination-based control when the higher-level perceptions are controlled by varying exactly the same lower-level perceptions from which the higher are derived. But you can't use it when the outgoing control acts involve one hierarchy while the incoming perceptions derive from a different one.

Gary Cziko (910610) --

I've heard "heterarchy" before. Gordon Pask uses that idea in conversation theory. In his version at least, a heterarchy is a network or directed graph constructed so that if you "unfold" it in various ways, you get various hierarchical relationships. It sounds neat, but the catch is that the networks aren't physical networks; they're maps of states of a system from one moment to the next. So it makes no sense to ask what is going on in one part of the net while something else is going on in another part. At any instant, only one node of the net actually exists. I got terribly confused about this until I finally understood that the diagrams weren't meant to be a diagram of the system doing the behaving, but only a way of representing the possible behaviors and the way they lead to the next behaviors.

Also the "hierarchy" resulting from an unfoldment is not like ours; it's really a logic tree, where you progress from the general to the specific.

Evelyn Satinoff says she abandoning the idea of hierarchy. WHICH idea of hierarchy? There are lots of different ones, and none of those I have seen in the literature are anything like the one we use. Ask her what it was she called a hierarchy, and why she discarded that idea. I'll bet our present model contains everything she wants from a "heterarchy."

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Date: Mon, 10 Jun 91 18:51:47 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: New Model

[From Rick Marken (910610.1830)]

Bill Powers (910610.1800)

Yes, you said more elegantly what I just figured out based on my modelling efforts. I had always assumed instrumental control. But the new model requires that perceptions at level n be functions of perceptions as level n-1 -- if the control systems are arranged to control their own "error offset references". The new model now works just fine as a quasi three level with level 2 controlling (and perceiving) line length and sending references to the "level 1" handle control system. I put level 1 in quotes because, as you note, it is not really in the same hierarchy as the line control system. This is a big discovery to me -- the difference between instrumental and hierarchical control. You can get away with it, to some extent, with the old model but not with the new one. Now, to try to figure out if the new model is more than just another pretty face.

Hasta Luego

Rick M.

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Date: Tue, 11 Jun 91 01:48:18 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Re: Heterarchy

From Tom Bourbon
Gary Cziko [100691]. You reported that Satinoff said, "heterarchy makes more sense, when more than one system is involved." But that opinion seemed to be grounded on one reference, from some time ago. Bill Powers [100691] asked, better than which hierarchy? My question would be, what makes a heterarchy seem better -- which kind of heterarchy, which kind of hierarchy?

I suspect that Satinoff is not familiar with the kind of hierarchy we employ and I wonder if she has seen any real-time simulations of behavior by a heterarchical model.

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Date: Tue, 11 Jun 91 07:24:51 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: New model; heterarchy

[From Bill Powers (910611.0630)]

Rick Marken (910610)--

>Now, to try to figure out if the new model is more than just another
>pretty face.

I know your type. You just want a theory that will stay at home and cook and clean house and raise babies while you go out every night with those flashy cyberneticians and spend our grant money on slinky abstractions.

What am I saying? We don't have any grant money.

Anyway, yes. The new model has to do something the old one can't do or it's just a complication. I don't even know if there are enough neural connections from the motor side back to the sensory side, internally, to support all this internal modeling.

You may remember that at an early CSG meeting I showed a demo in which the subject was asked to "re-do" a tracking run -- only the second time, the display was a playback of the cursor movements from the previous run and the handle actually didn't affect it at all (my only deceptive experiment). Most people got all the way or most of the way through the one-minute run before realizing that they didn't have any control of the cursor. What happens if you do this to a model? Will the old model be able to deceive itself, or is the imagination connection required to make control seem to work? What happens if you put a little random noise into the model so it can't repeat its previous movements exactly (like a real person)?

In Demo I there is a section on feedforward. Basically you learn to track "blind" by making the handle movements symmetrical with a display of the disturbance instead of the cursor. Going back and forth between the feedback mode (real cursor) and the blind mode (symmetry with disturbance) you gradually get the zero points and scaling factors adjusted so that tracking isn't too bad. How about a variation on that in which both disturbance and cursor are displayed, but the cursor disappears periodically for short to long times? This is a case where controlling the relationship between handle and disturbance can substitute partially for controlling the cursor directly (at a lower level). Would the parameters of control change during the periods when control depended on maintaining symmetry with the disturbance, as compared with the periods of real cursor control? Of course this all depends on having a disturbance that is accessible to the senses. But

maybe the cases where I thought the new model might be necessary (walking across a room when the lights go out) can really be handled by switching to different bases for control -- visual to kinesthetic and tactile.

What we need is an experimental phenomenon that the new model can handle and the old one can't. All previous major revisions of the model have gone into the trash-can when I figured out how the basic model could do the trick more simply. But model-based control isn't in the trash-can yet.

Tom Bourbon (910610) --

I second the questions to Satinoff. I suspect that "heterarchy" is really just another buzzword, in the class of "like, you know, the WHOLE THING." I've never heard of a working heterarchical model, either. Not that I'd recognize one if I saw it -- I don't even know what "heterarchy" is supposed to mean.

Good morning all -- Bill.

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Date: Tue, 11 Jun 91 10:02:21 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: New model

[From Rick Marken (910611.0900)]

Bill Powers (910611)--

>In Demo I there is a section on feedforward. Basically you learn to track
>"blind" by making the handle movements symmetrical with a display of the
>disturbance instead of the cursor. Going back and forth between the
>feedback mode (real cursor) and the blind mode (symmetry with
>disturbance) you gradually get the zero points and scaling factors
>adjusted so that tracking isn't too bad. How about a variation on that in
>which both disturbance and cursor are displayed, but the cursor
>disappears periodically for short to long times? This is a case where
>controlling the relationship between handle and disturbance can
>substitute partially for controlling the cursor directly (at a lower
>level). Would the parameters of control change during the periods when
>control depended on maintaining symmetry with the disturbance, as
>compared with the periods of real cursor control? Of course this all
>depends on having a disturbance that is accessible to the senses.

Yes, this is what I was thinking of as a test of the new model. But there seems to be a problem that I noticed after playing with the sum/difference control model last night. I set up the model so that you can "blind" either the level two (line length control) or the level 3 (sum/difference) control system(s). When you blind level 3, you are just taking away the level 2 error term -- so level 3 is controlling only its own references. When you do this you get some nice results -- if you change the level 3 references (without changing the environmental disturbances to the lines) you get "open loop" control. The level three systems are able to blindly change the references to the level 2 systems that produce the line lengths that satisfy

the level 3 sum/difference references. The problem is that this kind of blinding makes no sense physically -- how do you remove a persons ability to perceive a sum and difference between line lengths without removing the ability to see the lines (in the model the level 2 line length control systems can still perceive and control the lines).

If you blind the level 2 systems (equivalent to removing the display of the lines) then the new model behaves pretty much like the old model. Level 3 is now sending the wrong references to level 2 because the level 2 errors are enormous; and level 2 can't control relative to these referenes anyway because it is blind.

This is all pretty obvious, I guess. It makes me think, however, that the new model may not do what it is intended to do -- provide a graceful way of continuing behavior when some perceptual information regarding that behavior is eliminated (and there are no substantial changes in the environment for some period, as in the "feedforward" demo described above). Any thoughts on this?

Good morning to you.

Rick

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Date: Tue, 11 Jun 91 17:08:45 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: system concept stuff

From Ed Ford June 11, 1991

Joel, you said "Why do succeeding generations always seem to feel somehow exempt from those values which have caused problems in the past?"

Have the values caused the problems, or is it a person's attempt to take those values and apply them. (Even the devil can cite scripture to his means)

Bill, you said "when we think at the system concept level, we are far more likely to be helping to provide a choice of viable futures than when we simply propose clever sets of principles and rules that look as if they might achieve some immediate semblance of order..."

I just don't think you can separate the two levels. They have to be in sync with each other. You can think at system concept level, but

ultimately that thinking has to be translated into some kind of trial and error process which tests the validity of the system concept level. That means you have to set standards, then make choices based on those standards. I think the harmony within us -the real, continuous, long-term, peaceful harmony -has to exist between the levels and within the individual levels. This harmony can exist to some extent even in trying times in the external world (Victor Frankl's Man's Search For Meaning is an example). I agree that dealing only at one level doesn't offer a "viable future." The key is to maintain harmony throughout ALL levels as the system continually interacts with the environment within which it finds itself in order to satisfy the demands it makes on itself and the demands made upon it. As I work with clients (who often are locked into marriages, children, and/or jobs), I am trying to help them establish some peaceful order within their systems that will help them to find as much peace as possible (if this is what they want) in a very trying and stressful environment or set of circumstances. Are there a set of system concepts (and subsequent and corresponding lower levels such as standards and choices) more efficient at achieving these goals than others? For me, I think so. That is my search. For others, my job is to help them search for what might help them. I have known too many people at peace in very conflicting situations (my wife's handling of eight children and her husband when the youngest was still a baby and the older ones where in their teens).

I see problems arising when people set very different standards for the "same" system concept. The recent differences within the Presbyterian and Episcopal Churches are examples. Thus, the need to follow up on an established set of system concepts with standards that will make consequent choices reflect what was wanted. The ultimate test of a set of system concepts within a living control system is its ability to deal with the present and future environmental situation in which it finds itself and the subsequent sense of satisfaction (peace, harmony, whatever) that follows within that system. Ideas just have to be tested in the market place to determine their validity, that's all. And to do that, standards will give specific direction for the choices we make. The ideal may be to have both internal and external harmony, unfortunately we don't live in that kind of world.

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Date: Tue, 11 Jun 91 20:12:37 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: re: new/old model; hiawatha

[From Bill Powers (910611.1900)]

Rick Marken (910611) --

> The problem is that this kind of blinding makes no sense physically --
>how do you remove a persons ability to perceive a sum and difference
>between line lengths without removing the ability to see the lines (in
>the model the level 2 line length control systems can still perceive and

>control the lines).

Yes indeedy. I'm beginning to get the feeling that we're trying to solve a problem that doesn't exist. If you blind level 2 AND level 3, the level 3 systems can go on acting, convinced that they're still controlling the sums and differences -- but they won't actually be doing anything to the environment that resembles the imagined controlled variable. Also the level 2 systems will go berserk, with varying reference signals but no perceptual feedback.

>This is all pretty obvious, I guess. It makes me think, however, that
>the new model may not do what it is intended to do -- provide a graceful
>way of continuing behavior when some perceptual information regarding
>that behavior is eliminated (and there are no substantial changes in the
>environment for some period, as in the "feedforward" demo described
>above). Any thoughts on this?

Yeah, I think we should go back to the source and ask again just what it is we're supposed to be trying to reproduce. I've been closing my eyes and doing things like reaching for a glass, pointing at something, and so on. I "know" where things are with my eyes closed, but I don't get any sense of VISUAL imagination. It's all in kinesthetic space. "Over there" with my eyes closed is a sort of positional sense, but it's not accompanied by a picture. I don't image vividly, though. Can we hear from some others on the net about how they reach for something with the eyes closed?

Here's a possible experiment. Suppose the subject uses the mouse to move a cursor back and forth between two marks far apart on the screen. The required motion should be rather fast -- say two cycles per second, a speed at which configuration control would be getting pretty poor. So a higher level is needed, that monitors the peaks of the swings, compares those positions with the marks, and adjusts the amplitude of a repetitive event-controlling system (a hard way to avoid saying "oscillator"). Now, when you blank out the screen, the only way to approximate the previous actions is to reproduce the swings as kinesthetically felt. So the same "behavior" could be continued for a while, as long as the marks don't move.

The implication is that during the vision-controlled part, something is perceiving and recording the kinesthetically-sensed swings of the mouse. When vision is lost, control has to be switched to the kinesthetic system, relying on the recorded sense of the amplitudes of the back-and-forth efforts or arm movements.

Actually this phenomenon would not call for the "new model" to explain it. As I laid it out just now, it became obvious that this is a totally different problem, although to an external observer it might look like "continued control". All you have to do to prove that the old controlled variable is no longer under control is to move the marks. "Continuing behavior" is not the same as "continuing control." What really happens, I think, is that higher systems say "Woops -- lost it. I'd better just keep doing what I was doing (meaning making action sensations the same as before) and hope I don't get too far off before the picture tube is repaired."

You can drive for maybe five or ten seconds when the windshield goes blank with mud from a passing truck. But if the wipers don't create even a tiny hole to see through, you're done: you hit the brakes and hope.

Before we spend any more time on the new model, we need someone (Wayne? Gary? Anyone?) to come up with some examples where control seems to be continued after loss of the feedback signal. I'm beginning to think that this doesn't happen -- what happens is that we switch to controlling a different variable, if possible. We should be able to detect that experimentally.

A thought concerning the "oscillation" experiment. If the error signal in the higher-level relationship system (detecting the relationship between peaks of the swing and the marks) is INTEGRATED to provide the amplitude reference signal for the oscillator (deep breath), and if loss of the visual signal not only makes the perceptual signal zero but clamps the error signal to zero as well (breathe again), the value of the integral will remain unchanged and a constant-amplitude signal will be sent to the amplitude-control system reference input. The swings will keep going at the same amplitude as before. But all real integrators drift, so we should see a slow change in amplitude during the blanked-out period, if it's protracted enough.

This doesn't require memorizing how the swings felt and switching to a different hierarchy. It just falls out of the model. The OLD model.

So we have a new experimental proposition. Control systems that seem to behave as if they have an integrator in their output functions would freeze their output signals if loss of feedback also set the error to zero. That we can test. If it works out we can then worry about how the design of the control system has to be set up so that loss of perceptual input produces zero error regardless of the setting of the reference signal. Perhaps the neuroscience types could come up with a model.

I'm beginning to feel that the new model, sexy as it is, doesn't apply on this planet (all rights reserved in the vicinity of other G-type stars, however).

Joel Judd (910611) --

Gary said to you, re HIAWATHA,

>It might be OK, but I'd want the complete reference and your
>verification of the original source that Bill has added some of his on
>poetry to it.--Gary

I think you should avoid the company of this man. He is nasty and suspicious.

The author of HIAWATHA DESIGNS AN EXPERIMENT, Marice G. Kendall, is known to me only because his name appears in my copy of this file. I vaguely remember seeing this poem when I was an undergraduate at Northwestern, in the late 1940s. It went around computer departments and appeared on

bulletin boards. The copy I sent (unaltered) came from an astronomy graduate student at NU who got his degree in the late 1950s and passed the copy to me 30 years later. If that's not a good enough reference for Dr. Prof. G. Cziko, nothing will satisfy him.

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Date: Wed, 12 Jun 91 01:07:18 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: United Airlines;New\Old Model

From Tom Bourbon --

There are no discount fares for CSG members going to the meeting. I sent Ed Ford a working draft of the information for the newsletter. In it, I included the message about the discount, which was under negotiation with United. I said the details would be at the end of the newsletter. My plan was to send them to Ed via e-mail as soon as I had them, which I assumed would be in plenty of time for the mailing of the newsletter. I did not make my convoluted plans clear to Ed, who mailed the newsletter before I learned that the discount did not materialize.

I apologize for any confusion and inconvenience. I tried.

Rick Marken [110691] and Bill Powers [110691]. Both of you expressed some second thoughts about the new model, in part due to speculations about what happens when feedback goes away. Do you remember the project I described last year, by a student of mine, Vic Dyer? Vic was working in a hospital setting, with rehabilitation specialists who had all been trained to believe that one must not give feedback to people undergoing rehabilitation for disorders of all sorts. In his project, Vic had people perform pursuit tracking until they were proficient, then, for each person, he selectively eliminated some part of the visual diaplay -- the target, the cursor, or both. For some people, the elimination occurred during an undisturbed trial, with target movements the same as those on which the person had practiced. This would be the condition in which a learned pattern of movement would be most likely to see the person through. It didn't. Within a few seconds, people moved the handle in a pattern that deviated more and more from the path of the target.

You can imagine what happened when the elimination occurred during trials in which the target changed, or the cursor was disturbed. In spite of the obvious, several of my faculty colleagues continued to argue that, given more practice, people would be able to produce the proper movements of the handle even when target and cursor were not visible. That was also the assessment from the rehab professionals with whom Vic worked. All of those authorities must be right. In that case, none of our participants were smart enough to realize they were supposed to perform perfectly. Or maybe they were just too lazy to try. The experts are far more likely to accept those alternatives, which demean the participants, than to recognize the error of their own assumptions.

I think these results offer a pretty stiff challenge to

the assumptions that were driving your new model, as you
both have come to suspect.

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Date: Wed, 12 Jun 91 09:17:30 IST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Marsha Bensoussan <RHFL304@HAIFAUVM.BITNET>
Subject: Re: For fun
In-Reply-To: Message of Mon,
10 Jun 91 09:37:54 CDT from <DAVIDSON@VMD.CSO.UIUC.EDU>

set ltest-l nomail

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Date: Wed, 12 Jun 91 07:34:52 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: meeting, misc

[From Bill Powers (910612.0730)]

Tom Bourbon (910612) --

I saw a newspaper article to the effect that meeting discount fares were being abused: i.e., people who weren't going to the meetings were using them. Maybe that's why United is putting you off without actually saying they're doing so.

It's getting rather late, but perhaps you could tell United that we will notify individuals of the procedure ONLY after they have sent us their registration fee for the meeting. Maybe that will get some action.

Some members (e.g., Chuck Tucker) have indicated that they will fly to Albuquerque and rent a car there. This would save on air fare for others who wanted to share the rental cost of a car. The drive from Albuquerque to Durango is under 4 hours with scenery but no mountain driving. I don't know how people can get together on this: ingenuity, I guess.

Re the mysterious message from Haifa: Does
>set ltest-l nomail
mean that Hiawatha ticked off a statistician?

Re the new model:

I'm sure that after loss of feedback people do keep trying to satisfy the experimenter the best they can. We need an example of a control task that we can reproduce on our computers that illustrates this effect so we can try to figure out what they do when the primary feedback is lost. Suggestions about tasks don't have to be computer tasks -- we can translate. If nobody can come up with a suggestion, I'd be inclined to

let the new model go back where it came from, and drop "old" from "old control-system model."

Rick?

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Date: Wed, 12 Jun 91 10:38:29 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Loss of "Feedback", Rental Car

[From Rick Marken (910612)]

Bill Powers (910611)

>A thought concerning the "oscillation" experiment. If the error signal in
>the higher-level relationship system (detecting the relationship between
>peaks of the swing and the marks) is INTEGRATED to provide the amplitude
>reference signal for the oscillator (deep breath), and if loss of the
>visual signal not only makes the perceptual signal zero but clamps the
>error signal to zero as well (breathe again), the value of the integral
>will remain unchanged and a constant-amplitude signal will be sent to the
>amplitude-control system reference input. The swings will keep going at
>the same amplitude as before. But all real integrators drift, so we
>should see a slow change in amplitude during the blanked-out period, if
>it's protracted enough.

>This doesn't require memorizing how the swings felt and switching to a
>different hierarchy. It just falls out of the model. The OLD model.

>So we have a new experimental proposition. Control systems that seem to
>behave as if they have an integrator in their output functions would
>freeze their output signals if loss of feedback also set the error to
>zero. That we can test. If it works out we can then worry about how the
>design of the control system has to be set up so that loss of perceptual
>input produces zero error regardless of the setting of the reference
>signal. Perhaps the neuroscience types could come up with a model.

Yeah. I think this is the way to go. I was basically starting on this with the spreadsheet model. I think the "loss of perceptual input" problem can probably be modeled in several differnet ways. I'm beginning to think that this "open loop control" stuff is worth doing some work on. I hate it, in a way, because it is so obvious. But I was reading a book yesterday called "Making them move" edited by N. Badler, B. Barsky and D. Zeltzer (Morgan Kaufman, 1991) and it had several articles on "movement control" by psychologists. One was on motor programs (by Young and Schmidt) and it gives all the reasons why "feedback" is not an important factor in movement control. The same assumption runs through all the other articles. This is what the roboticists in this book consider the psychological state-of-the-art undersatnding of how behavior (well, movement anyway) works. Clearly, there is a VERY STRONG bias on the part of psychologists and others trying to make things that behave to see behavior as the control of OUTPUT, not INPUT. So it looks like we have to show these folks that there is NO SUCH THING as open loop control (or "control" of output). Since the main evidence that behavior is control of output comes from the "removal of feedback" exper-

iments, it looks like we have to show why these experiments are not showing what they think they are showing. We also have to show how the control of perception model handles the case (rare as it is in real behavior) where a person is suddenly deprived of feedback regarding at least one perceptual aspect of the variable they are controlling. This does happen in real life occasionally (the lights going off in the room while you are walking to a goal point or the snow on the windshield -- Bill's great example, I've been there) but these kinds of things are rare, and I guess it seems weird to aim one's modeling efforts at handling aberrent cases. But given the attention that "open loop control" is given by psychologists and roboticists I think it's time we spend some serious time dealing with it.

It looks like Tom's student, Vic Dyer, already has made a good start at it:

Tom says (910612)

>   You can imagine what happened when the elimination
>occurred during trials in which the target changed, or the
>cursor was disturbed. In spite of the obvious, several of
>my faculty colleagues continued to argue that, given more
>practice, people would be able to produce the proper movements
>of the handle even when target and cursor were not visible. That
>was also the assessment from the rehab professionals with whom
>Vic worked. All of those authorities must be right.

Is there a write-up on Vic's research? If so, I'd like a copy.

Bill Powers says (910612)

>I'm sure that after loss of feedback people do keep trying to satisfy the
>experimenter the best they can. We need an example of a control task that
>we can reproduce on our computers that illustrates this effect so we can
>try to figure out what they do when the primary feedback is lost.
>Suggestions about tasks don't have to be computer tasks -- we can
>translate. If nobody can come up with a suggestion, I'd be inclined to
>let the new model go back where it came from, and drop "old" from "old
>control-system model."

>Rick?

I think you (and Tom's student) have suggested (and done) some good computer tasks. I'd just do something like the one you described and start looking at what happens when you switch the visual variable in and out for different amounts of time (with and without disturbance). I guess I will reluctantly start this program ASAP.

On CSG meeting

>Some members (e.g., Chuck Tucker) have indicated that they will fly to
>Albuquerque and rent a car there. This would save on air fare for others
>who wanted to share the rental cost of a car. The drive from Albuquerque
>to Durango is under 4 hours with scenery but no mountain driving. I don't
>know how people can get together on this: ingenuity, I guess.

I am planning to fly to Albuquerque and rent a car. I am arriving in

Albuquerque on Tuesday, Aug 13 at 11:00 AM. I'm leaving from Albuquerque on Saturday, Aug 17 at 7:30 PM. Anyone want to rent a car with me? Let me know. Thanks

****

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Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)

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Date: Wed, 12 Jun 91 14:29:15 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Lungs & Muscles

[from Gary Cziko]

Wow! I was amazed to see all the activity that my "stumped rat" question elicited. Now that the old model has been reborn, let's see where another question might lead to. (I'm controlling for keeping the network active, I suppose.)

With all the recent comments on Mrosovsky's book on rheostasis and physiology, I got to thinking about physiological adaptation (my neighbors also just returned from Mexico City and said that they felt the effects of altitude).

When a sea-level (or Lake Michigan-level) person moves to a higher altitude (like Durango), certain changes are supposed to take place to compensate for the disturbances caused by the lower air pressure. I forget what all these are, but I seem to remember an increase in the number of red blood cells and perhaps an increase in the surface area of the lungs. Another obvious example of physiological adaptation is that brought about by exercise, such as lifting weights.

It would be interesting to speculate about how these changes are result of controlling variables. I can imagine how controlling the O2 or CO2 content of the blood at high altitude could cause adaptations in the lungs and blood, but I'm a bit stymied by what the controlled variable would be for weight lifting, particularly since only 8 to 12 repetitions lasting less than a minute done twice a week can make a significant difference in muscle bulk and strength.

Hm, I can now think of all kinds of examples in this vein. Callused feet from walking around with no shoes; losing one's hearing from listening to rock music; getting a suntan. Can all these adaptations be seen as the functioning of control systems?--Gary

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Date: Wed, 12 Jun 91 14:49:22 -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: car and csg mtg

Anyone,

Of late there have been a number of good car rental deals advertised. Ingeneral, these have offered a week of rental for $100-150. For someone within a couple days' drive, you can't beat it (if you like driving, and who knows, you might come across some new perceptual insight for CS. Cars seem to provide those, you know). This is what I'm planning to do.

Joel Judd

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Date: Wed, 12 Jun 91 15:45:24 -0500
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: Lungs & Muscles

Gary, all of your examples represent the functioning of control systems except losing one's hearing from listening to rock music; that is a result of damage to the hair cells of the inner ear by the loud noise. Making more red blood cells at hi altitude involves regulation of erythrocyte production by the hormone erythropoetin; I think the controlled variable is O2 or CO2, as you suggest. For muscle hypertrophy on use I think the c.v. is lactic acid concentration, tho I'm not sure. Foot calluses and suntan: ?? Organisms regenerate missing body parts, often (my area of work for many years); here the controlled variable seems to be, for each cell, a set of neighbors with normal position-specific markers. Absence of normal neighbors elicits the regeneration process, which proceeds until normal neighbors are restored. This system probably operates in embryonic development as well, tho just how is obscure. best, Jay Mittenthal

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Date: Wed, 12 Jun 91 15:56:10 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: system concept stuff

[From Rick Marken (910612.1500)]

Ed Ford (910611) says:

>Are there a set of system concepts (and subsequent and corresponding lower
>levels such as standards and choices) more efficient at achieving these
>goals than others? For me, I think so. That is my search.

The same set in all situations? For all people? If one takes the control model seriously (as an approach to understanding human nature) then system concepts are perceptions set to particular values to maintain other variables at particular values. The model implies that even at the highest level of the control hierarchy there is no absolute "right" set of references (absolute across people and environmental situaltions) that can achieve control. The only possible variables that might qualify as "absolute" in the control model are the intrinsic variables -- things like CO2 and O2 concentrations in blood and tissue, etc -- that must be at particular values of the physical system itself stops being able to function -- and there is death. Looking for a best set of system concepts, principles, or whatever has been, in my opinion, the main cause of problems among humans. After all, if there really were a best set of system concepts then the only right thing to do would be to teach them to others. But there is always the annoying possibility that other people won't buy into these concepts the way they should. This leads to ostracism, prejudice, and, of course, genocide. I think its better to look for the right model of systems -- and forget about the right systems concepts that systems should have.

> The ultimate test of a set
>of system concepts within a living control system is its ability to
>deal with the present and future environmental situation in which it
>finds itself and the subsequent sense of satisfaction (peace, harmony,
>whatever) that follows within that system. Ideas just have to be
>tested in the market place to determine their validity, that's all.
>And to do that, standards will give specific direction for the choices
>we make. The ideal may be to have both internal and external harmony,
>unfortunately we don't live in that kind of world.

We certainly don't, and we never will if the only test of a set of systems concepts is the extent to which they give the system the ability to deal with present and future situations (ie. internal harmony). As Bill pointed out, there have been people with lots of internal harmony (as far as anyone could tell) who created enormous external conflict. Slavery made it in the marketplace for years (again, I point out that the practice is NO WHERE condemned in the Old or New Testament -- what I presume is one of your sources of "standards" and "values"). System concepts, values, standards and whatever have been changing over the years as the demands of the marketpalce have changed -- human sacrifice used to be a very big item in the marketplace of values.

I think people are frightened to realize that system concepts, values and standards are not absolute -- never were, never will be -- because they feel that it means that things will quickly get out of control with no absolute, correct standards. The control model shows that this is precisely the opposite of the truth. Changes in these variables indicates that control is going on -- and that the principles, standards and values are simply part of the means of controlling some other variable -- something that we can name and experience but not very easily describe -- what we have been calling system concepts. But even system concepts may vary to control something even more basic. I argue that if these standards and values were absolute then things would definitely be OUT OF CONTROL. The "things" I means are the things that are most basic (and elusive) about human nature. Again, I note that trying to keep your standards, values,

principles or whatever at one absolute level puts you as out of control of whatever is defined by those variables as if you decided to keep your hand in only one fixed position while you are playing tennis. Variablility of means is as important a part of control as is consistency of the ends.

There, Gary, maybe that will start a little activity on the net.

Absolute (or fixed) references at ANY level of the hierarchy means the end of control and the beginning of intra and/or inter personal conflict.

Maybe

****

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Date: Wed, 12 Jun 91 19:10:26 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Misc comments

[From Bill Powers (910612.1800)]

Rick Marken (910612) --

All right, we're agreed that we need some experiments with interruption of visual feedback. My problem is that whenever I think up such an experiment I just want to throw up my hands and say "But it can't possibly work -- what's the point?" When you can't see the controlled variable, how can you control it when there are disturbances?

>Clearly, there is a VERY STRONG bias on the part of psychologists and
>others trying to make things that behave to see behavior as the control
>of OUTPUT, not INPUT. So it looks like we have to show these folks that
>there is NO SUCH THING as open loop control (or "control" of output).

That idea just makes me want to go take a nap. I don't see any way to overcome strong biases. About all I can think of doing is to collect a series of "control of output" experiments and write a long article showing that they wouldn't have worked (if they did work) if there had been disturbances. Of course this means replicating the experiments WITH disturbances as nearly as possible. It also means, in most cases, redesigning the experiment so it actually demonstrates something, which means doing what the original experimenters should have done in order to have something publishable, and so on and so on. It also means deliberately seeking a confrontation, telling other scientists they're sloppy dilettantes, and in general burning our bridges. Are we ready to declare war? I don't know. I guess my idea is to keep building on what we know, recruiting other people who are willing to understand how control

works and don't have a stake in control of output, and eventually leaving all those others wondering where everyone went.

So tell me what we should do.

Gary Cziko (910612) --

We still haven't dealt with the stumped rat, have we? I guess that's one of the phenomena we have to put aside for now. Maybe it's an example of control of output.

>I got to thinking about physiological adaptation ...

I think there's a general principle, long known to physiologists, that every major organ's principal output feeds back to suppress production of that same output. This implies that the output is sensed and controlled. I also think that it's generally known that if the output of an organ is CHRONICALLY removed at an unusual rate, that organ will hypertrophy; likewise, if the output fails to be removed so its concentration is chronically elevated, the organ will atrophy. The implication is that there is a fast and a slow control system concerned with the same output (which is really a sensed input). The fast system drives the production of the output in the usual way, guided by biochemical reference signals (entering the pituitary in most cases, I believe). The slow system, probably a lower-order local control system, responds to chronic error within the organ by growing more organ tissue or replacing it at a slower rate as cells die. This alters both the loop gain and the range of control. This, of course, suggests how organs grow in the first place. I suspect that the feedback from the product acts via enzymes that turn genes on and off. When the product concentration is too low, more cells are grown with the appropriate gene turned on, etc..

It's interesting that one example of this kind of phenomenon has recently turned up in the White House. Both Bushes have been diagnosed as having overactive thyroids. The feedback loop is thyroid-stimulating hormone (TSH, made by the pituitary) --> thyroid gland --> circulating thyroxin --> suppression of the pituitary's production of thyroid-stimulating hormone (TSH again, closing the loop). Negative feedback. Reference signals enter from the hypothalamus into the pituitary via the neurohypophysis. Maybe they enter elsewhere, too.

When I was a kid, everyone who was a little sluggish got a metabolism test, and if it measured low, the doctor prescribed thyroid pills. The thyroid pills elevated circulating thyroxin, which suppressed the pituitary's output of TSH, which reduced the thyroid's natural output of thyroxin, so the circulating thyroxin returned to its former level unless the dose of thyroxin was raised enough to assure a permanent elevation. Then, of course, the thyroid started atrophying away. After a dozen years or so of producing patients with thyroid glands like peas and total dependence on external sources of thyroxin (and almost complete loss of normal regulation of thyroxin in response to varying demands), the medical profession stopped giving thyroid pills for this problem. It did not, however, reach the obvious conclusion, which is that you don't cure

a control problem by pushing on the output.

In the case of the White House residents, medical science treated an excess of circulating thyroxin with radioactive iodine, which is selectively absorbed by the thyroid and destroys thyroid tissue. This lowers the loop gain of the thyroxin control system and reduces the ability to counteract large disturbances. If you weaken the output function enough, it doesn't matter that the reference signal is set too high: the system's output is now too feeble to bring the feedback signal up to the specified level. This cures the elevated thyroxin level, but of course destroys control of thyroxin level. Whatever was causing the elevation -- loss of feedback signal, something else setting the reference signal abnormally high -- is still TRYING to elevate the thyroxin level, but because the control system can no longer control properly, it does not succeed. The President will find that his body no longer adapts properly to cold, exercise, or altitude -- someone outside him will have to start playing with thyroid pills to get his metabolism to response properly to demands on it. And all the while, the cause of the problem is somewhere else.

Jay Mittenthal (good to hear from you), is this roughly correct from the biological-systems standpoint?

Joel Judd (910612) --

Tom Bourbon has a couple of students who are too poor to go to the meeting. Maybe they could go stand in the middle of Oklahoma City and wait for you. Maybe they could chip in a little toward the car and also, then, afford the conference cost. Maybe the conferees would be willing to throw three or four bucks each into a kitty to help out, too. Tom and Joel, are enough of these maybes feasible?

Tom Bourbon (private line). We'll look into the banquet thing, as our poor (D.C.) President would say. To heck with United. Everybody rent cars.

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Date: Thu, 13 Jun 91 00:19:15 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Re: Loss of feedback

From Tom Bourbon --

Rick Marken [910612a] asked if Vic Dyer's work was written up. It is a master's thesis: "The control of limb movement: an examination of negative feedback and efferent motor commands," from Stephen F. Austin State University, 1990. It is listed in thesis abstracts and is available through University Microfilms, or via interlibrary loan service from here.

BAsically, what he did was have 20 people perform simple pursuit

tracking, with a regular triangular target path. After everyone was competent (a few practice runs), each person ran in two conditions. There were 10 replications of each condition by each person.

In the first condition, each of the 20 people simply ran 10 replications of the original task, for a total of 100 reps. The mean correlation of 1800 positions each of cursor and target was .967 (SD=.032), so everyone did well. There were five different "condition 2s" with four people running each variant. One set of four merely repeated condition 1, with no changes. They performed the same as in condition 1. (Who would expect otherwise, but Vic had to earn the approval of a faculty that had to be allowed to say SOMETHING!)

One set of four ran 10 reps of a condition in which they had no cursor visible. (4 X 10 = 40 reps altogether) There were some choice comments from participants the first time they encountered the missing cursor -- they were not told in advance that it would be missing. For 40 reps, the mean correlation of cursor and target positions was .860 (S.D. = .161). Of course, since there was no disturbance acting on the cursor, that was also the handle-target correlation. Obviously these folks did the most reasonable thing in a dumb situation -- they made their handle movements feel like the target movements they saw on the screen. Of course. some of them made grossly larger movements that when they saw the cursor, some made much smaller movements, some kept the invisible cursor near the proper position vertically on the screen, others had it way at the bottom, and so on. But it is clear people can and do match body motions to target motions -- a nifty kind of cross-modal matching.

Another set of four encountered a missing cursor, and Vic reversed the relationship between the handle and the (invisible) cursor -- moving the handle up now moved the "cursor" down; handle down, cursor up. For 40 reps, the mean correlation of handle target positions was .770 (S.D. = .167). Of course, the mean correlation between CURSOR and target was the inverse of that: -.770. Obviously, people matched their movements "somewhat" to those of the target, but with no sight of the cursor, they could not know that they were moving opposite the direction they should have been moving. (Remember, apart from this study, Vic earned his salary in a hospital place where rehab. people, physiatrists and others all insisted that, under no conditions shold one give "feedback" to people undergoing therapy -- feedback is known to interfere with motor plans.)

In another set, there was no cursor, the handle-cursor relationship remained "normal," but a random disturbance acted on the invisible cursor. The mean correlation for cursor and target was .363 (S.D.= .258). They did not -- could not -- do very well, given the obvious inability to see tone of the variables in the relationship they wanted to control. But they did the natural thing: they matched their handle movements to movements of the target -- mean correlation of handle and target was .896 (S.D.= .074), very much like the group that oculd not see the cursor, but had no disturbance.

When questioned after their runs, people in all of the no-see-cursor conditions were able to state quite clearly and directly that they felt helpless to control the cursor, so they made their handle movements match target movements. (The idea that people do such things occurs to ordinary people, too, not just to PCT people.)

The grand finale was a set for which the screen was blank during condition 2. ("You've got to be kidding!" "What!" "@#)*(%&^#&^&@!")

Then they settled down and tried. In this group, everyone tried to REMEMBER how they had moved their hand during the several practice sessions and the normal runs in condition 1. This would be a test of purely program-driven actions -- control of output, so that it matched the previously established "program, schema, plan" or whatever. If plans and schema drive outputs so that they "are the same as when feedback was present," as is claimed in the literature on deafferentiation, you could not tell by the results Vic obtained. For 40 reps, the mean correlation between handle and target was -.090 (S.D.=.391). They were all over the place. MOst did not even keep the unseen cursor close to the middle of the screen. Many moved the handle much closer to their own bodies, so that the cursor would have been off the bottom of the screen.

All in all, when all of one's senses remain intact, and only a small part of what one would ordinarily see is missing, control is severly disrupted and people try to control relationships between whatever vsriables remain that were related to the original controlled controlled relationship. There is not much support here for the more popular theories or models of movement control.

Best wishes,

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Date: Thu, 13 Jun 91 07:53:23 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: let me level with you

[From Bruce Nevin]

A few quick responses to the rich and complex conversation I've been drowning in:

Going up a level:
When my 3-year-old is in a snit I'll ask her a series of questions to all of which she shouts "NO!", then I'll ask "Can you say `no'?". Long pause, visible boggle, giggle. Conversation continues on a different plane. Now her response is a somewhat peeved "Daddy, I don't want to play that game" but she's still up a level talking about the game one might or might not play and no longer playing the "NO!" game.

Knack:
Thinking about when my now 9-year-old learned to ride a bicycle, it took her a while because she dwelt on too low a level. Skill seems to require a shift of focus to attend to just the critical perceptions (center of gravity, seat of pants?) and automatization of subordinate processes seems to be a prerequisite.

Stacking on same level:
Consider a 3/4 rhythmic pattern. It is I think a configuration:

Rum tum tum Rum tum tum . . .

Likewise a 2/4 rhythm:

Rum ti Rum ti . . .

It is possible to combine the two, one with one hand and one with the other:

Left left left Left left left Left . . .
Right right Right right Right . . .

This is possible only when you attend to the pattern as a whole:

Rum tum ti tum Rum tum ti tum Rum . .

The "Rum" downbeat is both hands, the "tum ti tum" alternates the other two hands. The primary focus is on getting the downbeat right with both hands together, and the "tum ti tum" alternation is automatic. Once you get this it is easy to swap hands--reversing roles for the "tum ti tum" routine.

Once this is familiar, you can stop either hand and experience the simple rhythm (3/4 or 2/4) carried on by the other, then resume with the second hand. This compels shifting levels in a way reminiscent of face/vase gestalt images. It seems useful for experiencing levels.

But is this not a configuration comprising two configurations? Hence, stacking on the same level (configuration)?

Drummers execute much more complex rhythm and counterrhythm patterns, so an indefinite degree of nesting seems possible. A problem?

This is of particular interest to me as I look at phonology, the control of complex articulatory patterns involving precise timing of movement and position of different articulators (glottis, pharynx, tongue, velum, lips) together with air supply from the lungs. I want to look at modelling done at Haskins Laboratory and consider ways of recasting it in CT terms. (In my present circumstances it will take a long time to get to that.)

Blinding:
Seems to me blinding on level n with routine continuing (at least for a while) on level n+1 is very similar to automatization. A difference is: with blinding one attends desired return of level-n perception which was previously under control, shifts focus to control alternative perceptions but always ready to return to those preferred; with automatization, conscious attention is not required or desired, so it doesn't matter on level n+m whether level-n perception is there or not until it creates error on level (n+m)-1. But familiar strategies used for automatization may be recruited for unexpected and hopefully short-term loss of controlled perception. Notice that it works best if one just continues through the darkened room without pause when the lights go out, but if one pauses then some deliberate visualization and orientation is required. The gestures of ambulation were automatic and

attention was on the purpose for walking; when the lights went out, attention fell to the gestures of ambulation. These can become awkward, somewhat reminiscent of a 5-year-old trying to make a bike go.

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Date: Thu, 13 Jun 91 07:57:41 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Output control; rhythms

[From Bill Powers (910613.0700)]

Tom Bourbon (910612) --

Re: Vic Dyer's thesis. Another interesting study would be a report on the reactions of the "motor control" people to Dyer's thesis. The experiment refutes the motor control hypothesis and shows that feedback control explains normal behavior in this experiment. The obvious thing to do now is to get another student to take Dyer's results around to all the other people who saw them, and interview the others under the unbrella of a social-science study of "the response of scientists to counterexamples." I think the result will be that they are unfazed by seeing the motor control hypothesis demolished. They will go on using it anyway.

I'd love to see examples of feedback that interferes with learning motor plans! I'l bet it's along lines of "Keep it up, you're doing fine" or "Better...better ... worse...worse...". The "feedback" would, naturally, be under the experimenter's control.

Tell Vic that if he ever replicates this experiment, he owes it to the Control Theory Joke Book to tape-record the participants' remarks when they're asked to control missing perceptions.

Bruce Nevin (910612) --

Keep paddling, you're gaining on it.

I love the "up-a-level" experiment with your little daughter. Her reorganizing system is evidently in splendid condition.

Knack:

There's a critical relationship to learn in riding a bicycle (in addition to the lower-level control problems). To turn left, you twist the front wheel to the right, the wrong way, first. From a balanced position, this is the only way to get into a left bank: you move the bicycle out from under you to the right (and then immediately turn the wheel a little the "right" way, which the bicycle will tend to do by itself). If you try to turn left by steering left, as you would in a four-wheeled vehicle, you will naturally fall over to the right.

Stacking on same level:

What you have is a configuration only when you write it out. The way to understand variables of different levels is to ask about their steady-state condition. The steady-state condition of a configuration is an unchanging perception. As soon as change through time shows up, the configurations no longer are static. A new level of perception becomes possible.

I would call the patterns you talk about either "transitions" or "events." I haven't specifically brought rhythms into my hierarchy, although they've nagged at me for years. These can produced by oscillators, which have been part of other people's theories for a long time. They really belong in the control-system model, too, but I've just never got around to placing them in the hierarchy. I've been held up because there are also a-rhythmic events -- probably there are two levels collapsed together here.

The key to your two-against-three pattern is to realize that once it has been going for a short time, its steady-state condition appears in experience as a single unchanging pattern. This means that the perceptual signal is CONSTANT. As long as this signal is present, you experience "the same pattern."

It isn't necessary to perceive the 2-vs-3 pattern as a single pattern. Experienced pianists can split their brains, keeping one steady rhythm with one hand and a different one with the other hand, and attending to them in parallel. But your "stacking" of the two patterns is also correct; a very advanced pianist, Sam Randlett (of the CSG -- we are extremely eclectic) recommends your method for learning to play 3 against 3, or 7 against 8 (which is about the limit).

We haven't experimented with control of rhythms. I like your approach to the subject. Are you a modeler? It seems to me that experiments from the CT viewpoint might prove very interesting. Some simple experiments along these lines might give you some ideas for how to bring the CT viewpoint into phonology. By now you no doubt realize that a control theorist would look skeptical at being told that language production is a matter of producing the right output acts with articulators. Articulator configurations are VARIED in order to control -- what?

Blinding:

I like your suggestions. There's a hint here about how to handle the "motor program" idea. Clearly, a complex act like walking across a room between pieces of furniture gets to be very automatic, so you can think of something else while doing it. You set the sequence in motion and it unreels, including automatic control of rhythmic movements. As long as there are no disturbances, you don't need visual feedback (although all the other feedbacks have to be there if you're not to collapse into a heap).

This brings up again that "model-based" control problem, which is not going to go away. Here's what bothers me. The basic control-system model says that control is ALWAYS error-driven. I'll stand by that. But as we

learn tasks, the output part of the task seems to adapt so that it tends to produce the required PATTERN of output for a CONSTANT error signal. Maybe this can be handled in terms of levels of control. Some of this problem certainly can be. But I think that living control systems even at one level develop output functions that are more than simple amplifiers and signal-distributors. They take on dynamic properties that make the control task easier. This isn't an unsolvable problem, but it's hard to get a handle on. Somehow within a single control system there is something amounting to a model of the properties of all the lower-level systems (as they relate to controlling a particular variable). I've tried three or four ways to get that feature into the model, but none of them have worked. I'm sure the solution isn't going to be very complicated, but it's going to involve tricky feedback relationships that don't just jump out and grab you.

It's good to have your comments; keep 'em coming.

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Date: Thu, 13 Jun 91 08:34:45 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Misc comments

[From Rick Marken (910613.0800)]

Bill Powers (910612.1800) says:

> I don't see any way to
>overcome strong biases. About all I can think of doing is to collect a
>series of "control of output" experiments and write a long article
>showing that they wouldn't have worked (if they did work) if there had
>been disturbances. Of course this means replicating the experiments WITH
>disturbances as nearly as possible. It also means, in most cases,
>redesigning the experiment so it actually demonstrates something, which
>means doing what the original experimenters should have done in order to
>have something publishable, and so on and so on. It also means
>deliberately seeking a confrontation, telling other scientists they're
>sloppy dilettantes, and in general burning our bridges. Are we ready to
>declare war? I don't know. I guess my idea is to keep building on what we
>know, recruiting other people who are willing to understand how control
>works and don't have a stake in control of output, and eventually leaving
>all those others wondering where everyone went.

>So tell me what we should do.

Keep on keepin' on, I suppose. I actually have published enough of the "why output control models don't work" kind of experiments already. My latest paper (the rejected "hierarchical control of perception" one) just assumed control of perception and described research related to that perspective -- no fighting. By the way, that paper was rejected by Psych Review for two main reasons: 1) the ideas are not "new" and 2)it is not up to Psych Review standards. Rather than fight about it, I just asked the editor to help me by pointing out where he had heard these ideas before so that I could give proper credit and then I resubmitted it (pretty much as is) to Psych Bulletin (which, I hear, has lower standards than Psych Review).

Actually, one reviewer liked the paper very much; he seems to have rejected it because he didn't see how hierarchical control theory could explain an aphasic condition he was aware of -- where people cannot discriminate phonemes but can produce them. This seemed like a great case study (reflecting a disconnect between at least two levels of control) so I asked for the reference to that study. I guess the next time I submit a paper I'll have to remember to include an explanation of every behavioral observation ever made.

So what to do? I'll keep trying to add to that wonderful catalog of controlled
variables that you (Bill) posted. I do think, however, that it might be worth trying to study and model the "walking in the dark" phenomenon. It does happen and, as you noted in you latest post (910613) it may have important implications for how living control systems are actually implemented.

Bruce Nevin (910613)

>Going up a level:
>When my 3-year-old is in a snit I'll ask her a series of questions to
>all of which she shouts "NO!", then I'll ask "Can you say `no'?". Long
>pause, visible boggle, giggle. Conversation continues on a different
>plane. Now her response is a somewhat peeved "Daddy, I don't want to
>play that game" but she's still up a level talking about the game one
>might or might not play and no longer playing the "NO!" game.

Wonderful example -- and one that I remember quite well from when my kids were little. I did the same kind of thing with them many times and never realized that it is a perfect example of "going up a level". Bravo.

Hasta Luego

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Richard S. Marken USMail: 10459 Holman Ave
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Date: Thu, 13 Jun 91 12:50:25 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: Output control;Level with you

From Tom Bourbon --

Bruce Nevin [910613]. Thank you for sharing the delightful examples of levels. Long before we began speaking of "going up a level" in the CSG, I played similar "games" with my son and daughter (now 25 and 20). The effects were often as you described: boggling, followed by an entirely new level of discourse. What is more, I

noticed them doing similar things with their peers -- they became aware that the actions they were seeing from others were most often not the real story.

Like Bill Powers [910613], when I read your example of the combined rhythms, I immediately thought of Sam Randlett, the CSG pianist of note. I believe he would agree with Bill's comments.

Rick Marken [910613] agreed with Bill that the study of rhythms would be worth some attention in the PCT model. That is especially true, given the wide interest in rhythm and rhythmic coordination that is evident in the contemporary literature on motor control. Bill and Rick, do you recall the article on coordination, by Kelso I believe, that I showed you at the last CSG meeting? Kelso, Turvey and their colleagues have produced a flood of articles on that topic in recent months. One of the latest issues of *J. of Experimental Psychology* looked like a monograph -- three or four papers in succession by that group. As I type this, I realize that papers on the topic written by PCT people probably would not make it into the same journals -- after all, what would be new about anything we could say?

Bill Powers [910613] suggested that Vic Dyer tape the comments of participants in any follow up of his thesis work. In fact, I am trying to beg, borrow or ... well, not quite that ... video equipment with which to tape many of the projects underway or planned in our laboratory. The comments, gestures, facial expressions, postural changes and the like that occur when people first encounter a task, or when people in my "social tracking" tasks interact, are fascinating. In many cases they reveal the boggling followed by insight that Bruce described seeing in his young daughter. I am aware that these actions have been studied extensively in social psychology and in sociology (a fact well documented in Clark McPhail's excellent volume, *Far from the madding crowd*), but none of that earlier work employed thoroughly quantified tasks such as our simple stick-wiggling exercises, and none of the work employed modeling on a par with the modeling in PCT. The contents of the Joke Book will be no joke, as Bill knows. The spontaneous actions of people confronted by new tasks, or by patently ridiculous conditions, give clear evidence of disturbances to some of the highest levels in their hierarchies of perception and control. Any right thinking behavioral scientist would realize that a person who spontaneously says, "You've got to be kidding!," immediately after the scientist asked the person to perform an impossible task, is taking the scientist up a level. But I do not see such things discussed in the literature. Perhaps participants do not feel free to make such remarks around most behavioral scientists?

Regards,

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Date: Thu, 13 Jun 91 13:43:59 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: syscon/phonology/car

[from Joel Judd]

Rick (910612 replying to Ed) says,

>If one takes the control model
>seriously (as an approach to understanding human nature) then system concepts
>are perceptions set to particular values to maintain other variables at
>particular values. The model implies that even at the highest level of the
>control hierarchy there is no absolute "right" set of references ...

>...(re. "correct refs") But there is always the annoying
>possibility that other people won't buy into these concepts the way they
>should. This leads to ostracism, prejudice, and, of course, genocide.
>I think its better to look for the right model of systems -- and forget
>about the right systems concepts that systems should have.

This is why I keep wondering if I'm thinking about sys concepts in the same way others are. "Mechanistically" I can see how there wouldn't be a specific, unchanging value for every level in each control system. But by the time you reach higher levels, the very ref itself, while we give it a name, is "variable," isn't it? I mean I could argue that a certain definition of "family" (eg. mother and father and children) is the best. But of course every single instance of family would not be exactly the same. In one both parents may work; in another only the father. One may have three chiildren; another six. But a "family" of mother, father, and children may be the "best" social organization for having and raising kids, continuing the species, whatever. A single parent is not. Orphanages are not. Living with your aunt is not. That doesn't mean those things don't happen. At intrinsic levels you can say that certain O2 levels are best (even necessary). At higher levels why can't you say similar things? The difference is in the variability (degrees of freedom?) allowed by something like "integrity." The things I do and say are going to be different than the things you do and say, but wouldn't you rather deal with someone that has integrity than one who is untrustworthy? (and don't come up with some bizarre example where one might). Is this idea of greater latitude as one goes up levels accurate? Is there a better terminology for it?

>Slavery...(again, I point out that the practice is
>NO WHERE condemned in the Old or New Testament -- what I presume is one
>of your sources of "standards" and "values").

No, but neither is mistreatment of them condoned. Masters ARE advised to be just, and slaves are advised to be honest and faithful. The standards and values offered in "books of scripture" are of course couched in certain times and situations in history. That doesn't make the values any less valuable. Again, just because HUMANS can't always do things right, doesn't ipso facto mean there isn't a right way to do things (with the individual variability alluded to above).

>I think people are frightened to realize that system concepts, values and
>standards are not absolute -- never were, never will be -- because they
>feel that it means that things will quickly get out of control with no
>absolute, correct standards.

It's the meaning of that word ABSOLUTE that I'm asking about.

Parts of this discussion, and Rick's latest post, have smacked (I've always liked my own personal perception of this verb--it's so alive) of some comments made by a visiting behaviorist (Uh...I'm not comparing YOU to a behaviorist, but your arguments, Rick) last month. In fact Gary can probably reconstruct his line of argumentation better than I. The seminar was on Education, and he was asked about his views regarding the model he used. Some of these questions led into aspects of curriculum decision-making. Whenever this would happen, he would deflect them by saying something like, "That's a political question. I leave those decisions up to parents, school administrators, politicians. If you want to ask ME, as a person, what I think I can give an answer. But my (behaviorist) model simply describes/explains learning, decision-making, whatever. It doesn't imply WHAT learning, WHAT decisions, etc. would be 'good' or 'best.'"

That's the kind of message I get sometimes from this discussion. I can understand it. But I have to wonder at what point (and others might say "if ever") we allow those "political" questions back in. Much of the attraction for me of CT is the implication that there is reason to argue for better ways of doing things. Bill mentioned a few weeks ago that this starts to leave the realm of modelling, inasmuch as WHAT a sys concept IS isn't necessary to an understanding of how that level and others might work. But people in counseling, law, education, etc. have to work with real systems every day. That teacher is deciding the right thing to learn, as well as the right way to do it (hmmm, sounds familiar...). What kind of help do we provide them with?

(re. Marken paper rejection):

>he seems to have rejected
>it because he didn't see how hierarchical control theory could explain
>an aphasic condition he was aware of -- where people cannot discriminate
>phonemes but can produce them.

This seems to relate to the automization/blinding discussion. If it's aphasia then the damage occurred to functioning control systems. Wouldn't he have learned to discriminate the phonemes before insult? Phonology is so automatic by the time one reaches puberty (what WAS the age of the patient?) that inability to discriminate after insult doesn't surprise me. It would be interesting to know if even the passive ability deteriorates after a while. As opposed to most language learning studies, aphasic studies tend to be more longitudinal, so there might be some good information to be gained from them.

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The car deal might work out, but I won't be able to say for sure for a

couple more weeks, if that's not too late. The catch is I might not be travelling one or both ways alone.

Chau for now.

Joel Judd

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Date: Thu, 13 Jun 91 14:03:08 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: Madding;Categories

From Tom Bourbon -

Holy Toledo! I did it again! In my previous post [Bourbon, 910613a], I cited Clark McPhail's book, but I used the wrong title. The book is, *The myth of the madding crowd*, not *Far from the madding crowd*, which was the title of the poem that inspired the name of the theory of crowds Clark is trying to refute. I conclude that Clark will never be far from *Far from the madding crowd*!

Martin Taylor [many posts, with many repiles], this seems to be another example like yours of the swinging doors: I KNOW Clark's title, and I know he is trying to distance himself from the myth of the madding crowd, so he is trying to assume a position far from that of the madding crowd, Hence, my persistence in citing the wrong title.

Martin, another topic on which you posted long ago (several months?) was the idea that control often involves categorical decisions, such as are treated by signal detection theory. Some categorical decisions are correct: the target condition does or does not exist and its state is so identified by a person. In contrast, some categorical decisions are incorrect: a target that exists is not so identified, one that does not exist is said to exist. As you said, such circumstances occur quite often, and the decision a person makes, whether right or wrong, can give rise to an entire complex program of actions that would have been quite different had one of the other categories occurred.

Neurosurgeons read the results of the various imaging and diagnostic procedures applied to a patient and decide, yes or no, about the existence of pathology. If the decision is yes, the course of actions that follows is not at all what would have occurred had the decision been, no. Of course, all the while they follow the path that branches off from the categorical decision, they act to control their perceptions -- their moment-by-moment actions reflect continuous simultaneous interaction with their environment. If the path leads them to opening the skull of a patient, what they discover there sometimes differs from what they expected, an occurrence that leads to still more categorical decisions -- continue; initiate a new procedure; close.

The deaths of coalition forces that resulted from friendly fire, during Operation Desert Storm, like all deaths from any fire -- friendly or not, result from categorical decisions: is someone there; friendly or hostile; in range or not; and the like. In

every case, the course pursued after the decision differs from that had another decision occurred.

I know Bill Powers prefers to speak of continuous, rather than categorical perceptions, as was the case back when you raised the issue of categories and decisions. But categories, and branching points in programs, are part and parcel of PCT, so I agree with your thoughts concerning the need to include them in some of our discussions.

Besides, this long discourse might lead Clark to forget that I botched his title, yet again!

Best regards,

Tom Bourbon <TBourbon@SFAustin.BitNet>
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Date: Thu, 13 Jun 91 14:21:14 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Egg-Rolling

[from Gary Cziko]

Along the lines of the stumped rat, here is Lorenz's description of the egg-rolling behavior of the greylag goose. What is so interesting about this is that there does seem to be what appears to be a combination of control of motor output (drawing the egg back toward the nest) and control of perception (keeping the egg balanced on the underside of the bill).

"The fixed motor pattern used in the action consists of a stretching forward of the neck, bending the head downward so as to touch the egg with the underside of the bill, and then rolling it toward the nest by means of a slow bending of the neck. Concomitant compensatory movements of head and bill to each side keep the egg in balance and prevent its deviating from the intended path. The fixed motor pattern can be isolated by deftly snatching away the egg after the movement has been released. The movement then continues to run smoothly all the way through to the nest cup, staying strictly within the median, that is, along the bird's plan of symmetry. Once the movement has been released, it can only run its way through to the end and can be changed neither in its coordination , nor in its strength. If one offers the goose an object much too large, such as a huge cardboard easter egg, the movement literally "jams"; the goose proves unable to move the object in any other than the prescribed way--for instance by walking backward. If the object is not heavy enough, it is lifted off the ground; if it is too heavy by even only a slight amount, the movement fails to budge it. This is remarkable because a goose's neck is capable of producing a prodiguous amout of power, for instance, enough to pull a table cloth loaded with a complete tea set off a table, or, in a more teleonomic way, to tear heavily rooted plants out of the bottom of a pond. However, the power at the disposal of the fixed motor pattern is strictly measured to serve its single function.

"As can easily be demonstrated, the movements to each side, which during

the whole procedure keep the egg balanced on the underside of the bill, are elicited by tactile stimuli emanating from the object. Whenever the egg deviates to on side, the bill immediately follows it and guides it back into the right direction. It is possible to make the egg "run on rails' during the rolling process by arranging a bundle of reeds obliquely across its path. Then the movement tries to overtake the egg in order to correct the "wrong" direction and sometimes succeeds at the moment when the pressure of the bill acts at a right angle to the obstacle. If the goose "rolls"a square object that facilitates the establishment of a stable contact with both branches of the mandible and thus not diverging fom the straight line either to the right or to the left, the balancing movements cease altogether and the fixed pattern alone predominates, just as it does when the object being rolled is removed altogether." [Lorenz, Konrad Z. (1981). _The foundations of ethology_. New York: Simon & Schuster. (pp. 236-237)]

So if Lorenz's description is accurate, disturbances are compensated for side to side but not far to near. Can the far to near movement be explained as anything but the unreeling of a fixed behavior pattern which is not sensitive to feedback control?

I would really appreciate some help with this one since in my book I'd like to critique Lorenz's idea of innate behaviors, but examples like this are making that tough to do, particularly since Lorenz does reveal instances of when perceptual control appear to be involved, as in the side-to-side movements described here.

--Gary

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Date: Thu, 13 Jun 91 17:20:55 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Re: egg-rolling

From Tom Bourbon --

Gary Cziko [910613n], in the case of the egg-rolling critters, instead of control of output, what about control of perceptions around a fixed reference to "feel it coming straight back, but not any harder than this." That would account for the corrections of lateral deviations and would explain the relatively feeble efforts, compared to the forces we know the animal can generate.

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Date: Thu, 13 Jun 91 18:06:23 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Open Loop Goose

[From Rick Marken]

Gary Cziko (910613)
I like the Lorenz description of the egg-rolling goose but I don't think I understand exactly what is going on. I get an image of a goose backing toward a nest balancing an egg under its bill. The duck compensates for the odd movements of the egg -- keeping it rolling in a straight path. So the duck seems to be controlling for the perception of the force exerted by the egg against its bill. If the egg is removed after it starts rolling it to the nest it just continues until it gets to the nest? Is that right? And if the egg is too big it has trouble moving it at all and it just stays in one place? What does Lorenz (that quaint little Nazi asshole) think is the "fixed motor pattern"? I doubt that the muscles that move the duck to the nest are flexing in the same way on each egg rolling occasion. Is it the path taken (straight to the nest)? if so, there were no disturbances applied to test this. I do think this is a very interesting example and. I am sure, if you could get a hold of one of these geese you could quickly show which perceptual variables it was controlling. It is interesting because a behavior pattern does seem to be running off open loop. But I'm sure you would not be getting whatever consistency is seeing if, indeed, it was running open loop. I've never seen a goose or duck that bumped around the environment like one of those toy robots, which does move open loop.

I tentatively go with Tom Bourbon's description of the controlled perceptual variables in egg rolling (I especially like the "not too hard" reference to explain the goose's reluctance to push too hard -- a good reference to have when you are pushing eggs arounnd with your nose). But I still would like to get a better concept of what the bird is doing and what Lorenz thinks the bird is doing.

Thanks

Rick

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Date: Thu, 13 Jun 91 19:49:04 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Models; System concepts; geese

[From Bill Powers (910613.1930)]

Rick Marken (910613) --

>I do think, however, that it might be worth trying to study and model
>the "walking in the dark" phenomenon.

We seemed to run into modeling trouble when the control was instrumental. So let's try an instrumental control model. The mouse controls a small circle on the screen directly; that is, the circle position is the mouse position, give or take slip on the table. The circle is connected via a rubber band to a weight. The acceleration of the weight is proportional to the length of the rubber band (minus a resting length, I suppose, although not necessarily), and in the direction of the rubber band. All sorts of tasks can be devised, such as making the weight move from a starting circle toward an ending circle, making the weight travel around a big square, and so on, or even just making the weight stay in a moving square. The last would be easiest to model, because the speed is defined. In the first two, the participant could choose slow speeds or fast speeds, so it would be difficult to make a model pick the same speed of movement. Anyway, once experiments like this are set up, we can play with cutting off the information about the controlled variable or the instrumental variable for various lengths of time to see what happens to "control." Does the person build up a model of the weight/rubber-band combination so that appropriate movements continue to be made when the instrumental variable can't be seen? Etc.

Tom Bourbon (910613) --

The video tape approach sounds great, not only for what it can say about the experiments, but as you say for what it can reveal about higher-level controlled variables. After a participant says "You've got to be kidding," it would be nice to see what strikes the person as impossible or ridiculous -- and why.

Joel Judd (910613) --

>I keep wondering if I'm thinking about sys concepts in the same way
>others are. "Mechanistically" I can see how there wouldn't be a
>specific, unchanging value for every level in each control system. But
>by the time you reach higher levels, the very ref itself, while we give
>it a name, is "variable," isn't it? I mean I could argue that a certain
>definition of "family" (eg. mother and father and children) is the best.
>But of course every single instance of family would not be exactly the
>same.

The "variable" aspect of a system concept perception, as I've imagined it, would simply be the degree to which the perceived situation (principles, programs, etc.) qualifies as an example of the given system concept. In other words, I assume the "pandemonium" model, in which there are various system-concept recognizers all working in parallel, and they all receive lower-order information. They all respond to some degree by producing perceptual signals, but some hardly respond at all, while others respond maximally. The alternative would be to say that there is ONE system-concept recognizer, which responds to inputs by producing a perceptual signal that is somehow encoded to indicate the presence of one

system concept or another one -- but only one at a time.

I think the first one, although probably too simplistic, is closer to the way real perception works. I can say that a strange animal is a little like an elephant and a little like a snake, but perhaps more like an aardvark. What I can't do is say it is like some animal I have never seen. I don't perceive a single thing that is somewhere on the scale that goes aardvard ... snake ... elephant. I have to figure out what it is by looking at simultaneous responses from a number of recognizers, each set to recognize something I've experienced often enough before to recognize again. Same for system concepts. I can say that a particular parent-society-child system is a little like a nuclear family, more like an extended family, and very little like a state-controlled family (take 'em away at age 5 and raise them in an institution). I don't see a given family arrangement as a single point on a scale of different kinds of family arrangements.

Just what makes these different perceptions different can't be seen at the system-concept level. You have to look at the different principles employed, the different strageties of rearing, and so on down the levels. The hierarchical model does with levels what the multipurpose single-signal model does with a lot of internal complexity and memory in a single system -- less informatively, I think.

So the KIND of system concept is fixed in any one control system at the s-c level. What is variable is the degree to which a given environment exemplifies that system concept (this agrees, I think, with your proposal). This means that we judge the environment at this level in terms of several, even many, different system concepts, all at the same time, in parallel.

The reference signal CAN be a constant. If you want to see a "nuclear family," you choose the degree to which this perception is to be sensed. Do you want a "pure" nuclear family that excludes teachers, friends, honorary uncles, and so on? Or is some degree of nuclearity less than the maximum more preferable?

In general, different system concepts may be derived from overlapping subsets of principles. For example, in the nuclear, extended, and state-controlled families, one principle in common might be that of keeping the child safe; another might be that of educating the child; another might be that of giving the child a sense of success and approval. Other principles might not be shared: giving the child a strong sense of self; providing experiences of equal love and trust with many adults; teaching the child to subordinate self to society. Various principles are chosen to be consistent with each other under a particular system concept; different system concepts are built from different subsets of the principles one knows how to perceive.

Contrary to what Ed Ford said a few posts ago, I don't believe that we choose a system concept IN ORDER TO promote principles. That would make principles into a higher level than system concepts. I think we select principles so as to fit a given system concept. Of course we entertain more than one system concept, and the ones we choose to defend can easily require selecting contradictory principles. Christian businessmen have

problems like this all the time, whether they ever reflect on the contradictions or not. I go along with Ed to the extent of saying that we have to revise our system concepts to eliminate such contradictions, but we do so to eliminate conflict, not to preserve any particular principles.

You say
>... wouldn't you rather deal with someone that has integrity than one
>who is untrustworthy? (and don't come up with some bizarre example where
>one might).

I don't think that words like "integrity" and "trustworthy" can serve as system concepts. They have to do with principles that are necessary to make system concepts (particular social ones) work, but they say nothing about the system concept itself under which they are applied. Hitler wanted trustworthy aides to be in charge of getting rid of the Jews. The interrogators of the Spanish Inquisition may well have shown integrity in not pretending to have obtained a confession that was not actually obtained before the subject died. No matter whom you ask about system concepts, you will find those concepts defended in terms of uniformly noble principles, principles that most people would agree with. For a long time, the United States Government hesitated to extend the right to vote to black people, for fear of violating states' rights and overextending the reach of the central government. Opponents of gun control don't argue that they should have the right to shoot anyone they please; they talk about the Constitution, a man's right to defend his home, the need to retain the ability to resist dictatorships, the right of self-defense. They cite all the principles that people with other system concepts are likely to share, thus making it difficult for others to say that the NRA is wrong about something.

People are pretty fuzzy about system concepts; they get them mixed up with principles and often get the order reversed, as if the principles were more important than the system that makes sense of them and selects them. When people come right out and describe their real system concepts ("This is a white Christian nation"), they tend to leave their opponents discombobulated -- it's hard to say what you don't like about that (if you're white and Christian) other than that you just don't like it. System concepts aren't justified by principles; they determine what principles you will employ. I think we sense that when we come across a bigot. The bigot's problem, from our point of view, is in the basic premise. You can't argue anyone out of a basic premise because it isn't controlled by something at a higher level (as far as I know). A system concept is part of a world view, and world views are very hard to budge. They determine what looks like Truth and Right to you. So everyone, even the KKK, thinks that Truth and Right are their property. We say they're doing Bad things; they say they're doing Good things. They even quote from the same Bible.

>Masters ARE advised [in the Bible] to be just, and slaves are advised to
>be honest and faithful. The standards and values offered in "books of
>scripture" are of course couched in certain times and situations in
>history. That doesn't make the values any less valuable. Again, just
>because HUMANS can't always do things right, doesn't ipso facto mean
>there isn't a right way to do things (with the individual variability

>alluded to above).

But isn't this an example of what I was just talking about? Nobody (much) is going to say that justice, honesty, and faithfulness are bad principles. But under what kind of system concept are such things seen as defining a good society? I think slavery implies a system concept that extends principles such as ownership to include the ownership of human beings; that allows belief that some people have a divine right to be in control of others. Principles of justice, honesty, and faithfulness are not sufficient to define a "good" system concept. They can be subordinated to concepts we might approve of, and to others we would abhor. And what "we" means depends of which patch of earth you happen to occupy, an accident of birth.

So what is the right system concept? I agree with Rick. There isn't one that can be proven objectively right. If human beings don't know the right one, then nobody does. The rest of the universe is not designed to "know" anything.

And I don't think that anyone right now is in a position to say which one or ones are empirically right. The whole picture is just too muddled; as I say, few people even discern a difference between principles and system concepts. Before any concerted effort to revise and improve our system concepts can be made, people have to acquire at least some notion of when they are talking about system concepts and when they are talking about the means of implementing them.

Religion has preserved an interest in questions like these that science abandoned long ago. So I'm glad that religion is still around. I can even see merit in some of the system concepts implicit in various religious beliefs. Love thy neighbor is a pretty good principle, especially if the neighbor is me. I'm even willing to take it on as my own principle, within reason, because it seems to fit with a workable system concept of a society of human beings. But I don't think it's going to do anyone much good if it's taken as a command from God. If you take it that way, you will never try to work out WHY it's a good idea to love your neighbor. So you'll never grasp the system concept within which this principle makes sense. You might even conclude that in order to love your neighbor, you had better stay in the right neighborhood and not let inferior unlovable people move next door.

Gary Cziko (910613) --
Re:
>Lorenz's description of the egg-rolling behavior of the greylag goose.

Remember that ALL behavior is a process of controlling perceptions. Even bending the goose's neck. The question shouldn't be "are some behaviors designed for controlling perceptions and are some simply innate?" but "Are some control systems learned and are some innate?" The goose may have to learn how to keep the egg rolling in a straight line, because the shape of the egg and the terrain can't be predicted. But given that the lateral control system is designed to work in a variable environment, there's evidently no need to control the egg for distance: if the right

force is applied to the egg in the right direction, that is normally sufficient (often enough) to bring the egg closer. So the inherited reference signal "bring the egg closer" can be achieved by an inherited control system using a pattern of variation of reference signals for lower-order neck-bending systems. From Lorenz' description, I would guess that the main control system senses the force applied by the beak, moving the neck until sensed force equals reference force, with velocity feedback limiting the speed. When the object is heavy, the required force is reached before the object moves. When it is light, the object has to be accelerated to make the force reach the required level -- this lifts it into the air and probably throws it. It's clearly not a beak-position control system in the radial direction. There could be some evolutionary advantages in this seemingly stupid behavior. It would keep objects both heavier and lighter than normal eggs from being pulled into the nest.

Note that if you remove the egg, the motion will persist (with negative velocity feedback) until a limit is reached, because the sensed force never does reach the reference level.

The force vector is evidently always aimed at the nest, so the directional aspect of the control system is clearly working (else what does "toward the nest" mean?).

If you don't accept ANY description of behavior as a description of emitted output, you can usually find a reasonable control system that will entail producing the observed actions. Of course that's just the start. You have to apply the Test to see if I have guessed right, or more likely, whether a more complex set of controlled variables is involved. Now that I've provided a testable hypothesis about the goose, you, of course, are now required to do the experiment. Unless you think that answering this question is not worth the effort.

Sorry to go on so long, but you guys pulled a whole lot of triggers. It's not MY fault.

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Date: Fri, 14 Jun 91 06:09:00 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Dag Forssell <0004742580@MCIMAIL.COM>
Subject: Books, MCI mail.

Just a quick note with a suggestion for related reading:

"THE FIFTH DISCIPLINE; The Art & Practice of the Learning Organization" by Peter M senge. (Director of the Systems Thinking and Organizational Learning Program at MIT's Sloan School of Management). Doubleday 1990. ISBN 0-385-26094-6. $19.95

Excellent intro to Systems Thinking. Complete with "Circles of Causality". Shows conflict where two control systems attempt to control the same variable. Also effect of delay in feedback from the environment.

"DISCOVERING THE FUTURE; The Business of Paradigms" by Joel Arthur

Barker. ILI Press 1989. Call 1-800-328-3789, Charthouse Learning
to order. $17.50 ea + $4.00 handling.

Shows plainly how we can only see that for which we understand the
rules. "I'll see it when I believe it!" Easy reading!

Cites "The Structure of Scientific Revolutions" by Thomas S Kuhn.
Univ Chicago Press 1962. From the excerpts, it appears that the
pattern he has identified applies to the CSG group.

MCI mail:

Did my report on MCI in response to Cunningham get through? I neglected to
check. Tried at the time to send to the net and direct both.

Dag Forssell
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Date: Fri, 14 Jun 91 09:01:04 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Converted from PROFS to RFC822 format by PUMP V2.2X
From: "Bill CUNNINGHAM - ATCD-GI (804"
              <CUNNINGB%MON1@LEAV-EMH.ARMY.MIL>
Subject: MCI Mail

Dag,
MCI info received and passed to party who wanted it. Thanks very
much. Thought I had acknowledged it. Sorry.

Bill Cunningham
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Date: Fri, 14 Jun 91 09:36:34 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: System concepts

[Rick Marken (910614.0800)]

Joel Judd (910613)

I was about to try to reply in detail yesterday but I didn't have the time.
Then Bill Powers (910613.?) said much of what I would have said -- only
better. So I will just make some general observations related to the
following point:

>Parts of this discussion, and Rick's latest post, have smacked (I've always
>liked my own personal perception of this verb--it's so alive) of some
>comments made by a visiting behaviorist (Uh...I'm not comparing YOU to a
>behaviorist, but your arguments, Rick) last month. In fact Gary can
>probably reconstruct his line of argumentation better than I. The seminar
>was on Education, and he was asked about his views regarding the model he
>used. Some of these questions led into aspects of curriculum
>decision-making. Whenever this would happen, he would deflect them by
>saying something like, "That's a political question. I leave those

>decisions up to parents, school administrators, politicians. If you want to
>ask ME, as a person, what I think I can give an answer. But my
>(behaviorist) model simply describes/explains learning, decision-making,
>whatever. It doesn't imply WHAT learning, WHAT decisions, etc. would be
>'good' or 'best.'"

My reluctance to recommend specific reference levels for system concepts, principles, programs, etc as being the ones that people should control for is not based on "political considerations". It's because my current understanding of human nature leads me to believe that they do not exist. The "right" reference levels for any controlled variable depends on 1) the context of disturbances in which higher order variables are being controlled (and in which those variables are among the means used to control other variables) and 2) the context of other variables being controlled by the system. Claiming that some principles are better than others is as meaningless as saying that some postures are better than others. (By the way, this can all be made more tangible by watching the behavior of my spreadsheet hier-archy. It really help you get a picture of how a multilevel hierarchy of control systems, with many systems at each level, really works. The behavior of the modelk is really quite amazing).

It is possible, in principle, to say things about the result of controlling a variable at a particular level in a particular context. For example, I could say "if you take a step forward when you are standing on a cliff, you will fall". Does this mean that it is now possible to say " never take a step when standing next to a cliff"? Of course not, because the person might WANT to fall off the cliff -- like the divers in Acapulco. Saying you know the "right" references for systems concepts, principles, etc presumes that you know everything about a person's entire hierarchy of goals and, more importantly, the current and future state of the world in which they live. I don't think anyone imagines that such knowledge will ever be possible, even in principle. So, the hierarchical control model implies that it is only the system itself, not anyone outside it, that can determine the right setting for all perceptual inputs that it is controlling.

I suggest that this implicaiton of the control model is one reason people will always find it hard to accept it (just as the implications of the evolutionary model make it hard to accept it). People (well, most of them) seem to want nice rules to live by. And they have them -- in the reference signals to the program level from the principle level of their own hierarchy (I see rules, like "thou shalt not kill" as programs, the particular instances of which are selected to instantiate principles, like "life is to be valued"). But, as we noted in the "11th order" discussion, people tend to imagine that these references for principles come from "out there" -- and they are, with respect to the lower levels of one's own hierarchy. Moreover, people tend to think of them as "right" -- because they ARE right for that person. But somehow people go on to assume that these references for principles MUST BE right for others too. Part of this results from the fact that most people understand that they must cooperate with one another to some extent in order to succeed individually. So there is always the fear that if everyone sets their own references for system concepts, principles, etc. there would be chaos -- everyone would run around killing each other and stealing stuff. There is no question that people must agree on some high

level variables that "must be" kept at certain levels or coorperation will fail. But that's the problem that control theorists are talking about -- there is no magic solution to this problem, no set of clever rules from on high that will result in everyone getting along. People have done pretty well at cooperating for quite some time -- the control theorist has "faith" that an understanding of what kinds of variables people control and why they control these variables could lead to approaches to personal and interpersonal interaction that will produce better results from everyone. But I am sure that solutions can only be defined from the point of view of the participants themselves -- who are living in an ever changing environment. So I am sure that improvements in personal and interpersonal control will not result from the discovery of the "right way to behave". I'm afraid it's a bit more complex than that -- whether we like it or not.

Hasta Luego

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Date: Fri, 14 Jun 91 15:07:28 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Cooked Goose

[from Gary Cziko 910614.1430]

Bill Powers (910613b); Rick Marken (910613b); Tom Bourbon (910613):

Thank you all for your replies for your to me question about egg rolling in the greylag goose.

Yes, I can understand your points about this behavior being the control of perception, but perhaps at a lower order than we might think to be adaptive (What's the use of rolling an egg that is not there? I suppose usually the egg IS there.).

But I still have a problem figuring out what THE TEST would look like to see what was being controlled by the goose's far to near movement of the head/neck.

Lorenz effectively applied THE TEST to the side to side movements by setting up disturbances tending to roll the egg at an angle to the nest. But what would it take to show (and convince him if he were alive) that the far to near movement was also the result of a controlled perception. If this movement is the result of very low level control, wouldn't it almost mean having to get inside the goose to apply disturbances?

I have a feeling that this is a pretty dumb question and that I'm missing

something quite obvious. I obviously need some more help. That's why i set up CSGnet!--Gary

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Date: Fri, 14 Jun 91 14:01:48 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: ed ford <ATEDF@ASUACAD.BITNET>
Subject: system concepts

From Ed Ford June 14, 1991

Rick says: "Looking for the best set of system concepts, principles, or whatever has been, in my opinion, the main cause of problems among humans. After all, if there really were a best set of system concepts then the only right thing to do would be to teach them to others. But there is always the annoying possibility that other people won't buy into these concepts the way they should. This leads to ostracism, prejudice, and, of course, genocide.....Systems concepts, values, standards and whatever have been changing over the years as the demands of the marketplace have changed..."

First, I don't believe people are any different today than they were 200 or 2,000 years ago. I don't think the demands of the market place change people, I think people themselves create their own demands and are responsible for them.

But to the heart of the problem: I spend most of my time counseling others and working as a consultant in various social service facilities, especially treatment centers and schools. I've been married 41 years, raised eight children and I work with couples and families who are trying to establish or restore harmony to their lives. My total experience leads me to believe that there are certain values and standards from which people make choices and upon which people base their lives that provides them with a great deal of peace within their family and within the community in which they live.
I use CT daily with my clients to help them reflect on their created system concepts, standards, choices. I don't ask people to buy into my concepts. Frankly, most people really don't care what I believe, but whether I can teach them how to rebuild their own lives.

I teach them to reflect on what their present values are and how they've prioritized them, whether their standards reflect their values, and the current choices they are making. I deal solely with their internal living control system. My system concepts are not dealt with. Their values represent their present blueprint for how

they believe their life should be lived. Rick says (910614.0800) "the hierarchical control model implies that it is only the system itself, not anyone outside it, that can determine the right setting for all perceptual inputs that it is controlling" I couldn't agree more. Anyone who tells people they're wrong, tries to convince them to do such-n-such, to follow these external rules, etc. is doing irreparable harm (to quote Bill in B:TCOP they are doing violence to the system).

However, if when their lives are not going well and there is conflict within their system, then my job is to teach them how to review and then evaluate their system in light of their own hierarchy. I don't believe it is possible to force my ideas on anyone (anyone with children should know that). For example, a man may be having an affair (program level) and have a belief system that says it is wrong (principles level). He has put himself into conflict. Or, I had a man who was trying to work at his marriage and his wife's priorities were work, alone time, children, husband, in that order. Guess what happened to that marriage? Rick, theorizing is one thing, but taking control theory into the market place and trying to apply it there is quite another thing. And what does that involve? I think it involves teaching clients how to deal more efficiently with their system as they presently have created it so the conflict from which they are suffering can be reduced.

You quoted me as saying "Are there a set of system concepts....more efficient at achieving these goals than others? For me, I think so. That is my search." Rick, I am not talking about my specific religious convictions nor am I trying to force anyone to conform to my specific religious beliefs. I am talking about the system concepts, the values and beliefs, the priorities, the standards, the choices of the hundreds of people I see yearly and whose lives are a mess. They are looking for help. I believe from my experience of working with families and individuals over the past 25 years that there are certain principles that work much better than others. I don't force my specific values on others. My experience with others shows me what values seem to work at restoring harmony and which don't. I watch people struggle and I teach them how to rebuild their lives. From this experience, I can only say this: you bet your sweet life there are values that really work well. Such values as respect for one's spouse, seeing value in one's children, having respect for the integrity and worth of another human being (read living control system). What I do is to teach clients to evaluate whether the implementation of their concepts and principles is getting them what they want (peace, happiness, whatever).

Last night a woman called me asking for help on dealing with her husband whom she had just learned was having an affair. Ultimately, her husband is going to have to come to terms with his system concepts, his standards, his choices, and all those things with which all of us have to deal. That's what I am talking about when I say there are certain values that seem to be universal, that work well for most people. I'm not on a crusade to get Rick to conform to my standards, I'm just trying to figure out how to help those in need more efficiently by using control theory, and, in the process, look for universally accepted standards.

Bill said (910614) "Contrary to what Ed Ford said a few posts ago, I don't believe that we choose a system concept IN ORDER to promote principles." If I said that, I was certainly wrong. I've always felt that principles should reflect the higher order. But, when we are building an understanding of a system concept, don't we move from a lower to a higher order? Your explanation of the difference between system concepts and principles helped.

I'm going on vacation till June 27th. I shudder to think what the volume of mail is going to look like.

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Date: Fri, 14 Jun 91 16:38:05 -0500
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: Egg-Rolling

sorry, Gary, no good ideas. what especially surprised me about the goose example is that its neck doesn't deliver enough power to roll egg-shaped obe
objects slightly heavier than eggs. Perhaps naively, that isn't what I would have expected of a control system. best, Jay

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Date: Fri, 14 Jun 91 14:57:32 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Cooked Goose

[From Rick Marken (910614.1500)]

Gary (910614)

>But I still have a problem figuring out what THE TEST would look like to
>see what was being controlled by the goose's far to near movement of the
>head/neck.

What are the "near to far movements? What variable does Lorenz think is not controlled? What variables (results of the gooses muscle contractions) do you think MIGHT be controlled? I'm asking because I was telling the truth -- I really don't understand parts of the Lorentz description. It would really help if you would give me your own description of what is going on and what the "open loop" motor program might be. Once you have a reasnable idea about what variable MIGHT or MIGHT NOT be controlled then you can think of a test more easily, I think.

Thanks

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213 336-6214 (day)
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Date: Sat, 15 Jun 91 01:44:46 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: 1 Re: cooked goose; 2 "Big deal"

From Tom Bourbon --
Gary Cziko [910615]: you asked again whether the front-back movements of the goose's neck might not be open loop. Like Rick, I don't quite get the point of what Lorenz said. It has been many years since I read that work. Could you refresh us on it?

"Big deal!" That was the remark from one of my faculty colleagues when one of my thesis students presented his research proposal to our faculty today -- at least he presented it to the portion of the faculty that will even attend a proposal on PCT. The remark came after the faculty member, a cognitive scientist, had asked the student, Wade Harman, if he was telling us that the model "merely predicted a person would do what she did the first time?"

Wade's task, which he programmed himself, is a "4-D" tracking task. The target is an arrow head, like the emblems on Star Trek uniforms. It moves around the screen, driven by random waveforms that move it in X, Y, Z (size, or apparent distance), and A (or angle relative to the vertical). The participant uses a mouse to control the cursor in X and Y, and a joystick to control A and Z. People learn the task relatively quickly and perform it reliably, which prompted some of my colleagues to inform Wade that it must be easy, therefore predicting that people would continue to perform well was trivial. (No, they did not get the point!)

Wade models the particpiant as four independent control loops, deriving a reference signal (mean cursor-target distance) for each dimension of movement and deriving an integration factor for each loop by the tried-and-true procedure. Correlations between the four models and the person's movement of the cursor in each dimension run from .99+ down to some low .89s, for a few people who have some "problems" doing the task.

During the proposal, Wade was demonstrating the next step in the run, which was purely for demonstration purposes: The four models were moving the cursor to track the target, which was driven by a new set of random numbers for the second run -- he was showing the performance the models predicted for the up-coming run. I was watching in amazement, even though I have seen it run many times, when my cognitive colleague remarked, "This is all just circular reasoning -- you are predicting this with parameters you derived from what she did a while ago." Of course, he was right, but why do I still believe he missed the point?

Wade gets to go ahead with his project, but only because the

faculty knows better than to say no for no good reason.

Rick, several years ago, you had a "coming out," in which you told your faculty colleagues you would not go on playing the role of psychologist, teaching people things you did not believe. I was sorely tempted to do the same, but chose instead to stay at it. Now, with a growing body of literature on PCT, on feedback processes and on experimental design and statistics from the PCT perspective, it is easier to teach the material. But that is a mixed blessing: the kids in my classes (most of them ARE kids, compared to me!) make up their own minds about what they hear, read and do in my classes, compared to what they get elsewhere, and increasing numbers of them are interested in, or downright excited about, PCT and its implications.

But these people are getting B.A.s, or at most M.A.s, and there is no place for them in the world. Several of my colleagues are somewhat tolerant of me and of students who turn on to PCT, but some of the others are not so open or supportive. The person who asked seniors in a statistics course to present a talk on some controversial topic concerning uses of statistics in psychology was not prepared to have one student give a reasoned discussion of the "coefficient of failure," discussed on CSG-L a while back. Nor was he ready for another student who, by all accounts, gave an elegant review of Phil Runkel's critique of abuses of the method of relative frequencies.

Students are told by some people that they don't care what kind of evidence he (me) might present, PCT isn't right and it isn't psychology (I beleve that!). During a discussion with several students who invoked PCT as part of a challenge to his pet theories, another faculty member blurted out, "What does he (me) do to you people, brainwash you?"

Hugh Petrie is talking of attending CSG in Durango. If he does, he wants people who teach about PCT to share ideas and experiences. Bill Williams (an economist who is not on CSG-L) wants to talk about sample curricula for teaching people about PCT. This is all well and good -- I share their interests, but for several of us, the issue of what happens to the students we expose to PCT looms larger with every new development in the model and in its applications.

Something for several of us to discuss at Durango.

This has turned into free association, and on the net, no less. I guess I was wishing some of you could have been there this afternoon, when Wade deserved a better audience.

Best wishes,

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Date: Sat, 15 Jun 91 08:50:52 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: ed ford <ATEDF@ASUACAD.BITNET>
Subject: Get CSG material into school libraries

From Ed Ford June 15, 1991

Tom, my students at the School Of Social Work are having the same problem, especially during the oral defense of their thesis. Fortunately, they arm themselves with the references based on the list you sent me. What helps is to get all the various books on control theory by members of the CSG including Rick's American Behaviorial Scientist (Sept/Oct 1990) publication into the libraries. If they are not in the school library, they are not going to be read. I've found that kind of exposure really helps.

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Date: Sat, 15 Jun 91 11:09:45 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Geese; Harman's experiment

[From Bill Powers (910615.1000)]

On Geese:

Here's another slant on the "open loop" problem. There is lots of open-loop behavior. For example, when you turn the wheel of a car, the result is to alter the wind noise, isn't it? So wind-noise-altering behavior is open loop (wind noise doesn't affect steering). When you open a can of soup, you create a particular pattern of serrated indentations around the rim of the lid. Serration-creating behavior is open loop.

Almost every act that an organism produces generates behavior in an open-loop manner -- if you define behavior as any effect of motor activity that you happen to notice. This is what happens when you take the observer-centered view of behavior. The naive observer has no way of knowing in advance which effects of another organism's motor activities are important and which are not. It's an undeniable fact that when you hear a person in the next room typing messages on a terminal, that person is creating clacking sounds in rapid succession. So knowing nothing about what the person at the terminal intends to be doing, you can only say that the person is producing clacking behavior. You pick the effect that strikes you as "salient." From a theoretical point of view, that is equivalent to picking effects at random.

The chances of stumbling across a true controlled variable in this way are minuscule. Most of the effects you notice will be side-effects, of no interest or importance to the system doing the behaving. The observer simply notices what his or her own perceptions are concerned with. That is why those who study E. coli's mode of locomotion can marvel at its ability to "navigate through space." Of course E. coli does no such thing: all it can do is control concentrations of various substances at its chemosensory inputs (or their rates of change). A human being, however, sees that E. Coli is "swimming up a gradient," a concept that involves spatial concepts that are not part of E. coli's world. E. coli simply cannot control spatial variables, even though we can see it

affecting them. E. coli doesn't even know that it's moving.

When Konrad Lorenz sees a goose arching its neck and using its bill to move an egg, he is seeing all sorts of things that probably mean nothing to the goose. The arching of the neck creates a graceful pattern in human perception, because we are looking sideways at the goose's neck with our eyes, a view that the goose can't possibly have. The human observer sees the neck and bill cooperating to bring the egg "closer to the nest." Maybe that concept is part of the goose's world, and maybe not. The only way to find out what the goose is controlling is to test hypotheses -- and to be prepared to enter a perceptual world that is completely different from ours. The goose is not controlling the curvature of its neck as seen from outside the goose. It may be varying it, but it isn't controlling it.

From the standpoint of traditional science, point of view and perceptual interpretation simply don't arise. The scientist, after all, looks at the world the way it actually is. If the goose's neck fits a curve that is expressible as a cubic function of several variables, then that is a measure of "behavior." The goose's nervous system must be hooked up to compute that cubic family of curves. The idea that this curve is a completely accidental and meaningless side-effect of the goose's actual control processes would never occur to a scientist who automatically assumes the existence of a single objective universe -- the one he or she experiences, of course.

Most of the "problems" that people throw up against control theorists are of this same nature. They concern effects of motor activities that look interesting to a particular observer, but have not been shown to have any significance to the behaving system. And whenever open-loop behaviors are pointed out (often just to try to find a hole in control theory), somehow the question never comes up as to how open-loop behavior could ever have become organized. If an effect of motor behavior never reflects back through some external closed loop to inform the system of the effects of that behavior, how could it be that an organism can produce that same effect over and over by using motor actions that never repeat themselves? Even if you consider control theory to be only a vaguely-possible interpretation of behavior, how can you put up against it an explanation that depends on something even less possible?

In Gary Cziko's quotes from Lorenz, did you notice how Lorenz describes the outputs that produce the visible effects without apparent concern for just how those outputs instead of others come to be produced? To speak of movements that bring the egg closer to the nest is like speaking of steering-wheel movements that bring the car closer to the center of the lane. If you focus on outputs alone, you skip right over the central question of behavior, which is how just the right outputs happen to be produced. Control theory faces this question and answers it. All the other approaches dodge it, or beg it.

Tom Bourbon (910615) --

Don't feel too sorry for your students. They may face difficulties later in life through having come to understand control theory, but it is up to them to choose between continuing with that understanding and doing what

they can to advance it, or seeking a more comfortable life by giving in to the majority opinion. I have seen plenty of evidence that those who understand control theory elect to continue with it, considering the rewards of understanding greater than the material rewards available elsewhere in life. Why shield them from the problems that all control theorists have faced? They can handle them.

I hope that Wade Harman can come to the meeting and show us his experiment. At the very least he deserves to experience the reaction he will get from people who understand what he has done.

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Date: Sat, 15 Jun 91 11:50:31 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Geese, Control

[From Rick Marken (910615)]

Bill Powers (910615.0100) beat me to the punch once again. Throughout the course of this "open-loop" brouhaha I forgot to invoke my own favorite mantra -- "control theory is a theory of CONTROL". If a result produced by an organism is not a controlled result then the theory of control is obviously irrelevant. Open loop behavior is simply uncontrolled results of action -- like the clicks made while I type. They are accidental side effects. That's what my whole "Mind reading" demo is all about. If, indeed, something the goose is "doing" is open-loop then control theory just doesn't apply. There is no reason for alarm about this -- there are many things that the goose does that are unquestionably better explained by open loop models. The goose's acceleration as it falls off the goose coup is one example. I think this is also relevant to Tom's comments about the lack of acceptance of control theory by colleagues -- something I am indeed quite familiar with. I forget that one nice appoach to dealing with it is to just say to yourself (and your colleagues) -- "I'm just interested in a different kind of behavior than you. I am interested in studying control. And there is plenty of it to study. You go ahead and study that other kind of behavior -- the kind that is just emitted output. Have a good time and work hard. Just don't point to the existence of your kind of behavior as evidence against control theory. Control theory isn't trying to explain your kind of behavior. It just tries to understand control. Have a nice research program. Bye"

Love

Rick

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Date: Sat, 15 Jun 91 22:24:13 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Geese and Monkeys

[from Gary Cziko]

Note: This note is a bit of a ramble and I think I've answered my own questions in writing it, so it might not be worth the trouble reading through if geese, mango-slurping monkeys, ethology and evolution are not your interests.

Tom Bourbon; Bill Powers; Rick Marken (all 910615):

Thanks so much for your attempts to understand my problems with Lorenz's goose. I can follow without difficulty your reasoning, but I still have a problem with using THE TEST with the goose's far to near egg-rolling behavior, and the computer terminal clacking behavior now raises another problem for me.

Concerning THE TEST of the controlled variable, let's say that the goose is controlling for a given amount of push (force) against the egg in a certain direction (back toward the test). We have already seen how the lateral movement of the egg seemed to be controlled by the goose (Lorenz calls this taxis). Now, if I wanted to make a robot arm that could do the same as the goose, I can't see how I could use anything but a control system for lateral movement, but it still seems to me I could use an open-loop system to bring the egg closer. I would just have it set up to provide a given amount of force to the egg, no more, no less and let it go. So what can I do with the goose to show that the far-to-near movement is controlled? What would be THE TEST that would make it clear to us, and hopefully to an ethologist as well, that it is not a fixed motor pattern? Would it just be the observation that the goose's neck is not arched exactly the same way each time? Hm, perhaps it would be as simple as putting a weight on the goose's neck to see if it could still roll the egg back. Hey, I think I've just answered my own question. Can it be that simple? Just add some mass to the systems and if it is still successful it is clearly not open loop?

Now, let's get to the open-loop behavior that accompanies the closed-loop stuff. Let's say that in eating mangoes, a monkey makes a certain kind of slurping sound. Of course, he is only controlling for getting the mango in his mouth. But the slurping sound is loud and attracts and turns on all the surrounding females. Of course, he takes advantage of the queue of receptive females waiting for him to finish he meal and so leaves lots of progeny including males with an appetite for mangoes and female with an appetite for mango-eating males. So mango-slurping can become very important evolutionarily even though it is not a controlled variable. The monkey is not eating mangoes or slurping to attract females, but these behaviors in fact turn out to be a very important behavior. Somehow, this doesn't seem right.

But wait. I bet these male monkeys are too smart not to catch on and will start to slurp even when there isn't a mango in sight but females are. And then it will be the males who ARE controlling for slurping sounds (and successfully so against disturbances, such as lack of mangoes) who will be the most successful to reproduce. Of course, what they REALLY are controlling for is attracting females, with slurping just part of the program. Have I answered my own question again? Let me know. It seems like a perceptual control theory perspective on the evolution of controlled behavior could provide some real insights.

All of a sudden, I feel better about taking on Konrad Lorenz again!--Gary

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Date: Sun, 16 Jun 91 08:58:03 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Geese and monkeys

[From Bill Powers (910616.0730)]

Gary Cziko (910615) --

Re: Lorenz's Goose:

It's turning out to be an interesting exercise.

>...if I wanted to make a robot arm that could do the same as the goose,
>I can't see how I could use anything but a control system for lateral
>movement, but it still seems to me I could use an open-loop system to
>bring the egg closer. I would just have it set up to provide a given
>amount of force to the egg, no more, no less and let it go.

How do you set up a robot arm to "provide a given amount of force to the egg, no more, no less?" The force applied to the egg doesn't depend on the arm position alone, but on the inertia of the egg combined with resistance from rolling the ovoid (what else?) egg over bumps and from friction caused by the rolling movement at the point of contact with the arm (beak). If the arm is not in contact with the egg, no force is applied to the egg. If there is contact, the amount of force applied depends on how the egg rolls.

It would be possible to set up the robot arm as a position-control system, and then give it a smoothly varying reference signal sweeping from "far" to "near." This control system, however, would not behave as the goose does when a heavy egg is substituted or when something stops the egg. When the motion is disturbed by a large opposing force, the control system would simply increase its output force enough to keep the position matching the changing reference signal. So the force on the egg would rise if the egg hit an obstacle or became heavier. This would also happen if the control system controlled velocity of movement. Anything that slowed the arm would result in an increase in applied force.

What we need is for the velocity of the arm to depend on the amount of resistance that is felt. This can be accomplished in several ways using

tactile sensors on the arm. One way would be to have the reference-velocity be determined by the output of a force-sensing control system of higher level. With zero sensed force the velocity would be at some maximum (not very high). When the arm comes in contact with the egg, starting to compress the force sensor, the sensed force begins to rise rapidly toward the reference level and the velocity slows. Velocity ceases to decrease when the egg starts to roll; the sweep then continues at a slower speed. If we measured the velocity of the goose's beak during the rolling, we should observe that it decreases as the egg rises over obstacles and increases as it rolls down the slope after passing over an obstacle. If the obstacle is high enough, or if a heavy egg is substituted, the velocity will slow until the sensed force exceeds the reference force by some amount, at which point the velocity will drop to zero: the beak will stop moving.

This isn't the only design that will work, but the point is that without sensing the resistance of the egg to a push, there is no way to control the force applied to the egg. There must be a force-control system. In a real goose we would probably find the effects of touch occuring right in the spinal reflex loops; in human beings at least, tactile sensors feed back negatively to spinal motoneurones. But I'm just trying here to find a sufficient model, not a realistic one.

If Lorenz had been inclined to quantitative measurements, he might have used a high-speed camera, or a pair of them, so that the position and velocity of the beak and the orientation of the egg could be measured instant by instant. The above hypotheses then could be tested, and a better picture of the controlled variable could be obtained. Even better would be to equip the goose's beak with force transducers so we could get an idea of what the goose is feeling. It isn't likely that a naturalist would do experiments this way, though.

Have you tried this with a real egg? I think it always helps to put yourself in the control system's place if you can. Put an egg on a blanket (on a table) with some folds in it. Use your extended forefinger to pull the egg toward you -- with your eyes shut (I assume the goose can't see the egg under its chin very well). I think it will be pretty clear that you control the way the egg feels against your finger as you pull it toward you and that you sense where the egg is in terms of the kinesthetically-sensed position of your arm and hand. If you use a raw egg and a nice clean expensive blanket, you will be quite careful not to apply too much pressure. You will also understand how the goose uses multiple tactile sensors to sense the direction in which the egg is rolling and keep that under control. If the egg rolls ahead of your finger, you will probably increase the velocity a little until you feel the egg again. If the egg rolls all the way to the target, or if someone reaches in and snatches it away, your finger will execute the whole sweep until it reaches the target position. You're feeling for the egg, but you don't want to be moving too fast when you encounter it again. If someone reaches in and stops the egg, you will push a little harder, but not TOO much harder. And your arm will stop moving.

Most observations like those of Lorenz need to be done all over by a control theorist. The necessary observations just weren't made.

Monkeys Munching Mangos --

There's something in this example and the way you worked it out that could be interesting in studies of learning.

The implication is that there can be SYSTEMATIC learning of new control systems. Of course the effect of the side-effect has to relate to some variable for which the animal already has a reference level, and perhaps something that is already under control (but not completely satisfactory control). Slurping on the mango proves to reduce the error in some other control system. The next thing is for that other control system to get connected so that when it experiences error, the error signal is routed to the slurping control system as well as to systems already being used. I would predict, therefore, that the monkey would slurp AND do all the other things it was used to doing to attract females. The eating of mangos becomes a lower-order control system (one of several) with respect to the system that attracts females (as well as being in the hierarchy concerned with getting food). I suppose that, as you say, the actual mangos might drop out if the slurping alone attracted them -- but if the mangos attract the girls, maybe the monkey just imagines the mango part when there aren't any real ones, not being smart enough to know that nobody else can experience what it's imagining. On the other hand, slurping alone might well alert females that somebody in the vicinity has found a mango, through their own imagination connections. I should think, though, that hungry males would tend to become a problem. They'll respond to the prospect of mangos too, although they wouldn't respond to sexual innuendos from another male (usually). The slurping strategy might result in more conflict than is acceptable.

>It seems like a perceptual control theory perspective on the evolution
>of controlled behavior could provide some real insights.

Naturally, I agree.

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Date: Mon, 17 Jun 91 10:57:16 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: control, attention, phonology

[From Bruce Nevin]

Bill Powers (910613.0700)

(Thanks for including the ref number, it doesn't appear in my message headers over the Internet.)

>There's a critical relationship to learn in riding a bicycle (in addition
>to the lower-level control problems). To turn left, you twist the front
>wheel to the right, the wrong way, first. From a balanced position, this
>is the only way to get into a left bank: you move the bicycle out from
>under you to the right (and then immediately turn the wheel a little the
>"right" way, which the bicycle will tend to do by itself). If you try to

>turn left by steering left, as you would in a four-wheeled vehicle, you
>will naturally fall over to the right.

This is excellent, as I would expect. But the problem I had in mind is prior to the control of "wanting to turn". It is even prior to control of the perception of a balanced position, perhaps, since it concerns what to do if you perceive yourself starting to fall. A normal reaction on starting to fall is to thrust forward the arm and hand on that side. I imagine this is a pretty basic thing. On a bicycle, you must learn to counter this and pull the fall-side handlebar back toward you.

>The key to your two-against-three pattern is to realize that once it has
>been going for a short time, its steady-state condition appears in
>experience as a single unchanging pattern. This means that the perceptual
>signal is CONSTANT. As long as this signal is present, you experience
>"the same pattern."

2/4 is a pattern (call it transition, event, whatever); 3/4 is a pattern of the same kind. Continuing steady, the signal is constant. The 2-over-3 combination is also a pattern *of the same kind*, which continuing steady its signal is constant. Therefore it is possible to have constructions on the same level, such that the relationship between a simple pattern and a construction comprising more than one such simple pattern is analogous to the relationship between levels. However it does *not* involve a change of levels. Instead, involves a relationship among parallel control processes on the same level. Hence the perception of experienced musicians of "splitting their brains" to control multiple threads simultaneously. (One of the adjectives on Ned Herrmann's brain-dominance inventory is "simultaneous" referring to differential ability to attend to parallel control loops simultaneously. Not everyone can do it.)

>Are you a modeler?

I have made no computational models. Yet.

3-4 years ago I resumed my PhD program in linguistics at Penn, after a 20-year lapse. I work as a writer in the Customer Documentation Department in a division of BBN, the division that invented the network technology we are now using, and that makes and sells networks and internets. I have very little socalled spare time for things I consider important, and keeping up with your incredible rolling online seminar is one of them. I will be a sporadic and unreliable participant, which is really unfair all around since the quality of CSG discourse involves a lot of reciprocal exchange, typically with much shorter notes than this.

BTW, I have in mind sending a descriptive note and a diskette full of the past couple of months' Email to Brain/Mind Bulletin in response to a request from them for new ideas and exciting new directions. Not that CT is new, but it's news to most. Any objections? Brain/Mind Bulletin is Marilyn Ferguson's periodical, and lots of people you want to reach read it, as well of course as lots of gee whiz the WHOLE thing folks. It is one of the very few subscriptions that I have *always* kept up over the years. (Another is CoEvolution Quarterly, now Whole Earth Review.)

>Articulator configurations are VARIED in order to control -- what?

The phonetic description of speech is usually given in both articulatory and acoustic terms. The articulatory specification may be secondary, so that even the speaker (vs. hearer) is really controlling perceptions of the acoustic signal by articulatory means. This accords with the status of language as social fact. However, it is relatively most straightforward to specify speech for measurement and modelling in articulatory terms, and the relationship to acoustic properties is not simple. Making a working physical analog model of the vocal apparatus is daunting and it probably is not the best plan to set up control systems to control the acoustic signal produced by such a model. Manipulating components of complex wave forms directly (formants, transients, bursts of noise) is tricky, difficult to do well, and usually less than convincing intellectually precisely because the articulatory dimension is lacking.

As to what, there are many layers of variables to control. By one's speech one produces recognizable words with an informational purpose, which could be represented equally by writing; membership in a geographical dialect (or salad of same) and social dialect; attitude and relationship toward hearers, toward the information, and toward oneself; intended and unintended self-image. Probably more.

I once had a monograph from the Speech Department at the University of Okahoma describing experiements with vocal quality. Professor had written a text isolating different controllable components of speech--nasality, orotundity, breathiness, speed (pace), etc. Several students, male and female, who had shown good independent control of these factors were recruited to make a series of recordings of an emotionally neutral text, "riding down the rainbow trail." These were randomized and played to various audiences of other students, who were asked to evaluate the personal characteristics of the speakers, using various adjective pairs (honest-dishonest, fat-thin, etc.). They were virtually unanimous in their assignments of attributes. The writer pointed out that these judgments were exploited by old-time radio actors.

It seems to me that people construct their persona by making choices which features of vocal quality to incorporate; these attributes are all controllable. The choices, furthermore, are not consciously made. The same considerations apply to habits of gesture, body language, posture, variables of vocabulary and diction, social and geographical dialect, etc. etc. Labov's sociolinguistic data show that people make these choices predominantly at puberty.

So the great difficulty in language research is that so very much is being controlled within the relatively narrow band of events we experience as speech.

>of something else while doing it. You set the sequence in motion and it
>unreels, including automatic control of rhythmic movements. As long as
>there are no disturbances, you don't need visual feedback (although all
>the other feedbacks have to be there if you're not to collapse into a

>heap).

>   Here's what bothers me. The basic control-system model
>says that control is ALWAYS error-driven. I'll stand by that. But as we
>learn tasks, the output part of the task seems to adapt so that it tends
>to produce the required PATTERN of output for a CONSTANT error signal.
> . . .   But I think that living control systems even at
>one level . . . take on dynamic properties that make the
>control task easier. . . . Somehow within a single control system there is
>something amounting to a model of the properties of all the lower-level
>systems (as they relate to controlling a particular variable).

The idea of automatization has to do with the relationship of attention to control. This relationship is paradoxical at first blush because "control" normally implies consciously attended control. But as I understand you most control is automatic, that is, out of conscious awareness. Control with awareness is typically slower and less apt than control without. This is generally assumed to accord with gaining skill until control is automatic. But is it not the case that self-awareness involves something like modelling, and that this encumbers the modelled system?

It is a commonplace of vipassana meditation practice that one can become aware of bodily sensations that normally never rise to consciousness, and that one would not think accessible to awareness. It would appear from this experience that consciousness (whatever it is) can assume the point of view of any control system in the body if it is not kept busy elsewhere. This occurs when one simply witnesses the sensations, feelings, and thoughts that ceaselessly arise and fade, with no attempt at conscious control. Intervention by the control system that we know as consciousness interferes and obscures. The key experience the Buddhists call aniccha (changefulness, impermanence) rests in consciousness without intervention, that is, without (the illusion of) self-conscious control. This it seems to me corresponds to problems of micromanagement, to continue the bureaucratic analogy. Control systems are autonomous, and the direction they receive from higher levels is for each of them an internally-held reference value, not a message from another entity. As Lao Tse said, the best leaders so act that the people say "we did it ourselves." So must be the interrelation of control systems within one's person. But consciousness is a control system that is empowered to wander around and to intervene--or interfere.

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Some thoughts on the relation of CT to other fields.

Bill Powers has spoken eloquently for a "truth wins out in the end" view of science. But not all scientists and in particular not all members of science faculties control perceptions of "truth" but rather control perceptions of belonging to the established field and of fundability, which social and political matters.

Case in point: my mentor in linguistics (1966-70) was Zellig Harris, Chomsky's teacher. Years ago, he joined a kibbutz that provides its

members complete support in whatever they do, in exchange for their turning over all their assets and income to the kibbutz. It is one of the wealthier kibbutzim. For this and other reasons (Benjamin Franklin chair in linguistics at Penn, for example), he has been immune to pressures of conformity and has just done science, defined in pretty idealistic terms. However, his work has been politically marginalized by Chomsky and his followers, sometimes quite viciously so, and now he has effectively been forced out of the department he founded at Penn in the 1930s (first linguistics department in the US). An Oedipal tale for sociology of science to exhume one day. Meanwhile, Harris just keeps on doing his work and publishing his books. Has been working at Columbia in recent years.

Getting heard or understood or published with a Harrisian perspective is much as you describe for your students, Tom.

Dilemma: to work successfully with a majority who are controlling for perceptions of their continuing tenure-as-majority, must one also control those perceptions? And if so, is one's control of perceptions of truth (motivating the minority view) diminished in consequence?

Or is there a perspective encompassing both?

++++++++++++

I recommend again to your attention Rokeach's work on closed-mindedness vs. rigidity. In his "experimental cosmology," participants are told about Joe Doodlebug and his rectilinear, planar world. Joe can jump north, south, east, or west, no fewer than four unit-length hops at a time. Joe starts out facing north, he's hungry, and food is in a certain location specified for the experiment, which is posed as a puzzle question: what is Joe's shortest path to the food. There are several blocking beliefs or disbeliefs that must be recast ("overcome") to find the optimal solution. Rokeach determines which is a stumbling block for individual participants by giving "hints" at intervals and observing reactions ("aha!" followed by renewed activity, vs. sage nod and and continued pause for cogitation). Some are stumbling blocks for a participant who is more closed-minded (reluctant to entertain an alternative belief-disbelief system--interpreting the puzzle in familiar "real-world" terms instead of in terms of Joe's world as defined for the puzzle), others for a participant who is more rigid (reluctant to entertain a new belief or disbelief in an already-accepted belief-disbelief system).

I suggest this work could and should be reinterpreted in CT terms, and then applied reflexively to the relations of CT to "normal" psychology, To find more effective ways of working with or around these people. Ref. Rokeach _The Open and Closed Mind_.

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Consider: what perceptions was Harnad controlling when you all sent him mail urging a review of Powers' book?

++++++++++++

It sounds to me like you have good motivations for *not* being accepted and understood by the main stream. Acceptance may mean cooptation, understanding may entail reinterpretation, assimiliation, engulfment. On the other hand, you have this marvellous on-line seminar ecompassing grad, post-grad, and mature research workers, and an annual meeting. The perception of belonging is taken care of, though funding is I assume tougher. The role of knowing that the emperor has no clothes is a pleasant one. Who would want to give it up? So perhaps this is why you have not applied CT reflexively to meeting the institutional needs of the CSG and its members more effectively.

[I am splitting this file in half to be kinder to your mailers.]

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Date: Mon, 17 Jun 91 10:57:52 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: control, attention part II

[From Bruce Nevin--continued]

Part of my checkered career has included a 2-year training program in family therapy. My perspective there has been that family systems appear to be living organisms. Probably it is best to phrase this in terms of the unconscious participation of individuals in family processes that continually recreate and sustain the family system. The actions that are matters of conscious individual choice pertain to the individuality of family members; the actions and inactions that make up the fabric of "being a member" and the fabric of that of which one is a member involve distinctions that don't make any conscious difference to the ordinary individual (dialect, body language, posture, voice qualities). I have slipped here from family to broader constructs of social class, community, ethnos, and culture, so let me explicitly say that I believe we as individuals participate in the same sorts of processes continually to reconstruct and sustain our social reality in all its aspects.

The point is that this participation is out of conscious awareness, except for where individuals are specially trained or adapted to control some aspects of these processes consciously, such as salesmen, politicians, actors. As Bateson pointed out, this is why we distrust salesmen, actors, and such. He was referring specifically to how the body language expressing a given interpersonal relationship cannot be subject to conscious control without thereby losing its ability to convey that relationship sincerely.

This sets up a dilemma for study of higher levels of control. Ask a fish about water. You can (experimentally) ask the neuron or the muscle what it is controlling, because the neuron or the muscle is not itself framing the experiment. But in asking about perceptions controlled by yourself, the experimenter, or by your peers, fellow humans, you require awareness of differences-that-make-a-difference of which you must not be aware if you are to continue to control them appropriately.

In family therapy, the perspective is to perscribe actions to the individual family members that do not make sense to them as individuals, or which may seem paradoxical, because the only way you can address the family system as patient is through the individual membes of that system. As a family, they understand and learn, even while to them as individual persons the prescriptions continue not to make sense, except that their relations and communications with one another improve. Something of this addressing of human systems through their constituent members is I think required for experimental work with higher levels.

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The following survives from a dialog in another context about the virtue of competitiveness. My interlocutor challengingly asked how there could be any success in the world without competition and without the dynamic of victor and vanquished. I offer it as a contribution to the discussion of the evaluation of values and system concepts. In my view the root of ethics is this: that which tends to unity is preferable to that which disintegrates--a dictum to be interpreted in terms of systems and levels if it is to be sensible.

Some of this is preaching to the choir in this forum, but the info about anthro and Ruth Benedict's work is I think news here. There is also a subtext about Buddhism and the experience of meditation that I would like to take up at another time.

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The Hopi don't fight, they have lost surprisingly little of their resources given the vicious history of their territory, and the members of their communities are not suffering in any obvious ways because of their pacifism. They don't kill because it is obvious that it is not an appropriate thing to do, same as you would not eat feces.

Judgment is a tricky matter. There can be no judgment without a point of view. There can be no "objectivity". But taking a point of view imposes a perspective in light of which some things appear more favorable, others less.

The usual way of approximating objectivity is a process of consensus. You have your perspective, I have mine. If by reciprocal explorations we discover some commonalities, they are more likely to be "true" than points on which we disagree. To accomplish this, one's own perspective must become an object open to inspection and potential falsification, on an equal footing with the perspectives taken by other participants. Something of this is what is called scientific method, honored alas more by verbalism than by practice.

Evaluation of cultural differences is especially tricky. It is extremely difficult to bring one's own cultural perspective to a conscious level, where it is open to inspection and potential "falsification" on an equal footing with other cultural perspectives. It requires enormous effort, and that effort in my experience can only be mounted if one is motivated by a commensurately enormous desire for a greater grasp of truth, at whatever cost.

The costs are great, partly because co-members of one's own culture may not take this distancing and "objectification" of the givens of their world lightly or even kindly, but mostly because it bucks the stream of one's own desire, as a mammal, to belong, to be in proper relation with one's peers. All mammals share this very deep requirement for relationship. (I refer you here to some of Gregory Bateson's writings on the cybernetics of human and cetecian social systems, for starters.)

If you really want answers to your questions--can there be success without competition, what metrics for success can there be other than dominance over one's peers--I suggest you become acquainted with some of the varieties of culture and begin the struggle to understand, first that alternative perspectives are possible, then beyond that perhaps that they may have genuine validity, and maybe even that the alternative perspectives are *not* in competition, one does not have to be proven "best".

(Note that this judgment of "best" cannot possibly be bestowed without first taking a point of view, and that amounts to a pre-judgment that one's point-of-view-for-the-sake-of-judgment is in fact the best. One may believe that this "neutral" point of view is in some way set apart from the set of perspectives being adjudicated, but that is only the gesture that cements the prejudice.)

What one may achieve is not "objectivity" (one of the illusions spawned by the conviction that one is/has an independent, separated ego), but rather the ability to recognize ambiguity and work constructively with it. Think of the now familiar gestalt-shift images, like the black vase which turns into a pair of white faces nose to nose then back into faces, or a 3D drawing of a cube, or Escher's work. A useful initial hypothesis is that *everything* is ambiguous, that is, capable of alternative interpretations from alternative perspectives. (Email flaming usually hinges on failure to handle ambiguity properly, though it is fuelled by emotinal needs such as a desire to demonstrate power--and thereby the reality of one's ego--by provoking a reaction from others.)

The place to start is by becoming better acquainted with the work of those who have tried to understand other cultures, workers in anthropology for the most part, in subfields like ethnology and the ethnography of speaking.

What might an alternative metric for the relative "goodness" of different cultures look like? We have to clear some confusions out of the way first.

Recall that the unit of survival in biological evolution is not the individual but, minimally, the mating pair. Among mammals, survival of a more extended group is the focus. (The mammalian emphasis on relationship I noted earlier is both an outcome and a contributor to this--what the Buddhists call mutual causation.) "Survival of the fittest" very definitely does not reduce to survival of the fittest individual. Indeed, individual fitness as measured by likelihood of mating with progeny that survive is well correlated with the

individual's contribution to survival of the extended social group that provides a matrix supporting survival of the mating pair plus progeny. Darwin emphasises in _Origin_ the importance of cooperation as being at least as important as competition and probably more.

Nonetheless, "social darwinism" followed on publication of _Origin_ essentially as justification for conservative social and political agendas that included racism and sexism as unexamined tenets, as justification for destruction and forcible assimilation of "primitive" peoples for their own good. When most of us hear the expression "survival of the fittest" we assume this social and political analogy to an erroneous view of nature, "red of tooth and claw". Survivers of abusive parenting in particular have strong emotional attachment to this perspective as means of reconciling hatred and rage at their abusers with the ineluctable love of parents and family that comes with the package when you are born as a mammal.

Again: what might an alternative metric for the relative "goodness" of different cultures look like?

In 1941, the anthropologist Ruth Benedict gave a series of lectures calling attention to the correlation between social structure and character structure, especially aggressiveness. She compared cultures for their differing capacities to support or humiliate the individual, to render the individual secure or anxious, or to minimize or maximize aggression. She borrowed the term "synergy" (independently of the somewhat divergent borrowing by R. Buckminster Fuller) from medicine, where it had long referred to combined action. "In medicine it meant the combined action of nerve centers, muscles, mental activities, remedies, which by combining produced a result greater than the run of their separate actions."

The quote is from "Synergy: Some Notes of Ruth Benedict", Maslow & Hoenigmann, _American Anthropologist 72(1970):320-333, and parts of the preceding paragraph are paraphrased from the intro by Margaret Mead, who was Benedict's literary executrix. I have an old photocopy that I could duplicate if you are interested.

Though the point is left tacit in this document, it is clear that U.S. culture, like many of its most influentual tributary cultures, is toward the low end of the synergy spectrum (though not so low as the aptly named Ik, whose dreadful degeneracy was documented by Turnbull). For us, self-interest is clearly opposed to altruism, and accounts of cultural realities for which these notions are so closely identified that there can be no distinct vocabulary for them strike many of us as the wishful thinking we may associate with fairy tales.

(Benedict's immediate impulse seems to have sprung from the widespread destruction of indigenous cultures across the Pacific, and a concern how to counsel policymakers in making choices for culture change when two cultures confront one another. Given our low-synergy view of things, however, policymakers could view it only as one side winning and the other losing, and guess which side they favored--to be sure preserving some exotica as a sop to the anthropologists and the natives and eventually as tourist attractions. Discouragement as to the ability of

scholars to influence policy is I suspect a reason she never returned to the topic, after writing _The Crysanthemum and the Sword_ about Japanese culture in change, to write the book she had envisioned on synergy.)

A simple example: Hopi and Navajo children do "poorly" in school in part because e.g. when the teacher sends a group to the blackboard to do a math problem, with instructions to turn around when finished, they wait until all have finished and then turn around together. I mean, how can you grade on a curve when the aim of their game is to present a flat profile, and the only way they can do that within the schooling framework is by the smart ones staying back with the slowest?

I encountered this issue when I worked with the Pit River people up in the Idaho corner of California. I studied their (moribund) language for four years, 1970-74. For a few months I was engaged by the local school system to teach the language K-12. There is much more that could be said, but it is not simple or straightforward, in large measure because one cannot understand the issues (or even that there are issues) without first moving at least somewhat toward being able to shift perspectives.

Through Benedict's students (notably Herbert Marcuse), the term "synergy" has become an icon of the "human potential movement." Most centrally what the human potential movement is about, I think, is bringing about, through change of individuals' values and cybernetic patterns of evaluation and reaction, an amelioration of our culture toward greater synergy, a change that is increasingly seen as crucial for our collective (and therefore individual) survival.

People stuck deeply in being isolates react with deep distrust to exercises they see as "touchie-feely" (no matter if there is no physical contact between participants). The reason, I think, is that in a low-synergy culture group membership is perceivable only in hierarchical terms, as subordination to superiors and dominance over inferiors. The expectation of abuse following vulnerability is too strong. That a high-synergy situation *empowers* you as an individual precisely through your participation in a team or other group is beyond comprehension. We see this in the mistrustful reactions of many of us to the TQM initiatives being undertaken in many of the companies in which we work.

We understand power, in our culture, only in terms of dominance and submission, and competition and violence are necessary consequences of that low-synergy way of framing situations and events, not antecedent causes. Beating criminals into submission is both expression and reinforcement of a low-synergy frame of interpretation. But many people stuck in a low-synergy perspective take it as "obvious" that human character is not susceptible of change. Given that assumption, steps to remediation (of criminals, kids in schools, employees) appear to be patent nonsense. It is a bitter and bleak reality these folks project onto our shared situations and events. When by ruthlessness they have achieved positions of relative dominance, it is hard to keep clear of the same cognitive traps in dealing with them, harder still to be compassionate for their genuinely tragic plight, like the king in the old tale who went mad and insisted on living in the basement of his palace. ("Better the devil that you know," he said, "than possible ones that you don't.")

But people are capable of sometimes radical change. Even those of us who are most stuck. Were this not so, I would be a sad and forlorn man today, were I alive at all.

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Date: Mon, 17 Jun 91 14:43:56 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: clips from _New Scientist_

[From Bruce Nevin]

In _New Scientist_ for 25 May 1991 is a review by John Bell of _Machines With a Purpose_ by Howard Rosenbrock (Oxford, I guess 1991). Quoting the review in part:

> Howard Rosenbrock, emeritus professor of control engineering at the University of Manchester Institute of Science and Technology, offers ideas that scientists may well find uncomfortable. . . .
>
> The last chapter explains that industry is usually based on the scientific principle of cause and effect. Sometimes it is based on a purpose, such as meeting people's needs for satisfying work. The difference between the two types of industry is that one automates where possible and treats people as machines to serve machines, while the other makes machines subservient to people and their purposes.
>
> the author says that the former approach is based on the myth of cause and effect while the latter depends on the myth of purpose. He defines a myth as "a description of events which carries with it implicit values and may serve a social purpose."
>
> He points out that the scientist's view of nature, as intrinsically causal, does not necessarily suit the technologist, who has to serve purposes that are defined by industry and society. The professor also chooses the word "myth" to emphasise that causal and purposeful views of nature are equivalent; that is to say, what proves or disproves one proves or disproves the other.
>
> . . . society's usual "explanations are causal--whether in physics or chemistry or biology or social science and to the extent they are verified by facts they suggest that nature itself is inescapably causal. Medieval society in Europe had much the same relation to Christian theology."

I can't tell how much of Rosenbrock's point Bell (a technology journalist, according to the byline) missed. The book may be interesting from a CSG perspective and may identify an ally.

In the preceding issue, for 18 May, is a feature article on liquid crystals and cell metabolism. The liquid crystal physics of cell membranes seems to suggest the physical basis of very low-level control systems. The myelin sheath of nerve cells is a liquid crystalline material.

Amphiphilic molecules (hydrophylic on one side, hydrophobic on the other) form bilayers separating watery from oily media. These bilayers may form simple sheets, or may curve, depending upon the relative concentration of substances to which they are sensitive (which varies, depending upon the molecule), and the curves range from simple globular "protocells" which may help to explain origins of life to porous structures that are topologically quite complex. The article summarizes research suggesting that these more complex structures ("non-lamellar phases") are fundamental to regulating the activity and stability of cell membranes. The following quote is taken from near the end of the article:

> Let us consider a cell in which membrane proteins regulate the production of a specific lipid, you can see how the cell might control the lipid composition of the membrane by negative feedback. Imagine a really simple cell membrane made of <substance A> and <substance B>, with an embedded protein that regulates the production <A>. When the protein is "switched" on, <A> is produced at full pelt. As more <A> is produced, the stress due to the <A> wanting to change phase build up in the membrane, and eventually cause the protein to switch off. Less <A> is then made, the stresses reduce, and so once again the protein switches on.

[Substance A is dioleoylphosphatidylethanolamine or DOPE, and B is dioleoylphosphatidylcholine or DOPC. I changed the label to avoid the confusion of the tag DOPE throughout the passage.]

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Date: Mon, 17 Jun 91 14:19:16 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: system concepts

[From Rick Marken (910617.1030)]

Well, I'm back to my editor so I can reply to Ed Ford's (910614) post.

Ed says, in answer to my claim that there are no "right" principles or systems concepts:

>My total experience leads me to believe that there are certain
>values and standards from which people make choices and upon which
>people base their lives that provides them with a great deal of peace
>within their family and within the community in which they live.

But are these always the SAME values and standards? Are you saying that only a particular set of values and standards leads to inner and community peace? If so, why keep them a secret? Why not tell what they are -- for the sake of those people (probably nearly everybody) who seeks those ends (inner and community peace).

> Rick, theorizing is one thing, but taking

>control theory into the market place and trying to apply it there is
>quite another thing.

I think we all live in the same "marketplace" (which I take as a synonym for the "real world"). We all operate on the marketplace based on theories of how it works. You imply that my theorizing is not tested against the realities of dealing with adulterers, murderers, or whomever it is you deal with who you consider "the marketplace". From my oint of view, controlling a line on a screen is as real as controlling the number of extramarital affairs one has. If the theory of control doesn't apply to everything purposeful that people do -- from controlling lines to controlling crimes -- then the theory must be fixed to handle it. But I don't believe that control theory is all well and good for understanding computer experiments but inapplicable to the big mean world outside the lab. Your statement implies that there are very important phenomena that occur in your therapy sessions that control theory can't handle. What are they?

> I believe from my experience of working with
>families and individuals over the past 25 years that there are certain
>principles that work much better than others. I don't force my
>specific values on others. My experience with others shows me what
>values seem to work at restoring harmony and which don't. I watch
>people struggle and I teach them how to rebuild their lives. From
>this experience, I can only say this: you bet your sweet life there
>are values that really work well. Such values as respect for one's
>spouse, seeing value in one's children, having respect for the
>integrity and worth of another human being (read living control
>system). What I do is to teach clients to evaluate whether the
>implementation of their concepts and principles is getting them what
>they want (peace, happiness, whatever).

> That's what I am talking about when I say
>there are certain values that seem to be universal, that work well for
>most people. I'm not on a crusade to get Rick to conform to my
>standards,

I know. I don't feel that you are. You wouldn't need to anyway -- most of the values you mentioned above sound alot like what I would think of as mine also. The question is whether ANY particular values of any controlled variables can ever be considered absolutely RIGHT from a control theory perspective. Variables (in theory) are always controlled in order to control other variables. The only absolute, fixed references for variables in the model are intrinsic references -- and those are references for variables that reflect the viability of the organism itself. I could accept the idea "right" references for system level variables (not principles or anything lower that is used to control system level variables) if you could convince me that a particular level of a system concept was required for survival of the organism. Actually, based on something Bruce Nevin just posted, one could make a case for certain levels of systems concepts being required for GROUP survival. Maybe that's what is going on. I agree that the principles you listed above could be seen as a reflection of a system concept that could be described as "belief that the rights of other humans to control their own perceptual variables -- as long as this does not deprive me of the ability to control

my own perceptual variables" -- ie -- a system concept that is instatiated by principles that have to do with COOPERATION. Maybe thse are the absolute references -- the "right" references -- you and Joel are talking about.

I guess I agree that, when you take a group survival perspective, there may be "right" references for non-physiological controlled variables -- variables that don't have to do with individual survival. But I do think that 1) these "right" references must be for variables that are at the top of the hierarchy (system concepts) and 2) that the consequences of selecting "not-right" values of these references is not necessarily a problem for the systems that adopt these "wrong" references. I think this is what we have in the so-called "psychopath" or "sociopath". This is a person who is perfectly well organized to control system concepts relative to references that are set to the "wrong" level. These individuals experience little internal conflict -- but they create enormous external conflict -- by pushing strongly and effectively agaist the variables that others are trying to control.

But remember, in order to keep perceptions of system concepts at the "right" level, it will be necessary to vary references to the lower levels -- and this means changing PRINCIPLES, if necessary. Bill said it well a couple days ago:

> I can even
>see merit in some of the system concepts implicit in various religious
>beliefs. Love thy neighbor is a pretty good principle, especially if the
>neighbor is me. I'm even willing to take it on as my own principle,
>within reason, because it seems to fit with a workable system concept of
>a society of human beings. But I don't think it's going to do anyone much
>good if it's taken as a command from God. If you take it that way, you
>will never try to work out WHY it's a good idea to love your neighbor. So
>you'll never grasp the system concept within which this principle makes
>sense. You might even conclude that in order to love your neighbor, you
>had better stay in the right neighborhood and not let inferior unlovable
>people move next door.

The last part here is the important one -- principles do vary in order to preserve system concepts. Look at what happened to some of the nicer principles (what I thought were the principles) of early christianity; things like live a simple live, the meek shall inherit the earth, it's easier for a rich man to get through the eye of a needle than through the gates of heaven. Well, there was another system concept that demanded some conflicting priciples. We now live in a christian, capitalist country where it's a positive virtue to work hard to get rich. My wife (a recovering catholic) said that the nuns had no problem with that "eye of the needle" principle -- she was taught that it was hard for the rich man to make it through (because he was carrying all that stuff) but he could still make it.

There is nothing "wrong" with varying principles in this way. It must be done to control the system concepts that demand that certain levels of principles be perceived. If there really are certain system concepts that are better than others (for group survival) then it might be helpful to try to articulate what they are, rather than claiming that certain principles (which are used to control these concepts) are absolutely correct. The

latter could prove problematic for individuals. I bet that most of us who are in on this discussion are controlling for the same level of one very important system concept -- the "mutual respect" concept that I tried to articulate above. There are likely to be slight differences in the levels of certain principles that we all set in order to control that concept; for example, I believe it is perfectly possible to control that system concept by controlling the principle "trust in the lord thy god" at many different reference levels. For whatever reason, the level at which I control that principle is different than the level at which Ed or Joel (I think) controls it -- but I bet we all end up perceiving about the same intended level of the "mutual respect" system concept.

Respectfully yours

****

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

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Date: Mon, 17 Jun 91 19:18:47 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Misc reply to Nevin

[From Bill Powers (910617.1800)]

Bruce Nevin (910617) --

First, Bruce, thank you for your long and fascinating introduction to your knowledge and interests, which seem to know no bounds. I hope that your pursuit of a PhD leaves you enough time for at least occasional participation in CSGnet, and that after you have your ticket, you will throw your lot in with ours. Linguistics is underrepresented in the CSG, and beyond that we would benefit greatly from the breadth of your interests.

Re: rhythms, etc. --

>Therefore it is possible to have constructions on the same level, such
>that the relationship between a simple pattern and a construction
>comprising more than one such simple pattern is analogous to the
>relationship between levels. However it does *not* involve a change of
>levels.

But if the RELATIONSHIP among these patterns is important, then we are talking about perceiving relationships, not patterns -- a different level of perception.

I have talked about this same problem at a lower level -- configurations. Given the sensations that make up a chair, we can perceive an arm, a leg,

a back, a seat, and a chair -- all at the same level. I have assumed that these perceptions are not (as they appear to be) hierarchically related, but are the result of applying the same kind of computation to different subsets of sensations. They all occur in parallel. So there is no such thing as a configuration of configurations of configurations .... There are only configurations.

Concerning rhythms, I argued with our piano expert Sam Randlett about his way of teaching dual patterns like 3 against 2 (actually 3 against 4, to be specific). He claims that the best way is the way you showed: to reduce 3 against 4 to a single complex rhythm A&B--A-B--A--B-A----A&B. When you actually try to do it this way, I believe you end up misplacing the middle B's, trying to make this into an 8/8 pattern (i.e., hearing an underlying constant eight-beat rhythm with accents at various points in it). In reality the B's should be triplets, and if there's any underlying steady rhythm it would be the common demoninator of 1/3 and 1/4 -- twelfths. I certainly can't do twelfths on a piano, although I can do 3 against 4. When I do that I put the bass on automatic and play triplets in the right hand -- evenly spaced, more or less. I can't split my brain like a real pianist (reputedly) can.

Anyway, my conclusion would be that we can perceive/control the constant rhythms of quarters and thirds separately, in parallel, at the same level, and ALSO (if we go to the trouble to learn how) the combined complex rhythm (which I claim won't actually come out very close to 3 against 4). To perceive that the complex rhythm is related to the single rhythms, we have to go up a level and employ relationship perception. Relationship in this model is a mode of perception, not a fact of nature.

>Control with awareness is typically slower and less apt than control
>without. This is generally assumed to accord with gaining skill until
>control is automatic. But is it not the case that self-awareness
>involves something like modelling, and that this encumbers the modelled
>system?

I have hypothesized that awareness and reorganization go together: systems operating out of awareness not only operate automatically, but they operate in an unchanging way (save, perhaps, for deterioration). I would see "modeling" as an activity of the learned systems, not of awareness. I don't think that awareness itself does anything but receive. But this is all very conjectural.

>It is a commonplace of vipassana meditation practice that one can become
>aware of bodily sensations that normally never rise to consciousness,
>and that one would not think accessible to awareness. It would appear
>from this experience that consciousness (whatever it is) can assume the
>point of view of any control system in the body if it is not kept busy
>elsewhere.

I use "consciousness" to mean the combination of awareness and perceptual signals, so I would substitute "awareness" in the above. Otherwise, you have precisely stated my hypothesis. I'm sure I got the germ of this idea from readings in Eastern philosophies. But it came mostly from my own experiences. Unscientific.

>It sounds to me like you have good motivations for *not* being accepted
>and understood by the main stream. Acceptance may mean cooptation,
>understanding may entail reinterpretation, assimiliation, engulfment. On
>the other hand, you have this marvellous on-line seminar ecompassing
>grad, post-grad, and mature research workers, and an annual meeting. The
>perception of belonging is taken care of, though funding is I assume
>tougher. The role of knowing that the emperor has no clothes is a
>pleasant one. Who would want to give it up? So perhaps this is why you
>have not applied CT reflexively to meeting the institutional needs of
>the CSG and its members more effectively.

My, you do cut close to the bone. You are also perfectly correct, if by "you" you mean me singular. Rather, however, than saying that I have a significantly large reference signal for being rejected, I would say that I have a very small reference signal for being accepted into any mainstream school of thought. My personal institutional needs are very modest -- about all I need for now is a logon to bitnet or internet. I have all the students I need to bolster my ego, although when they pass me up (as they're beginning to do) I don't know what will happen. I am retired in enough comfort to last for a while. The only reputation I care about is that which exists among people who understand me: I do care quite considerably what they think, so I try not to say foolish things more often than they are willing to tolerate. It is very pleasant to be one who knows that the emperor has no clothes, although it bothers me that he does not. Being one of those who doesn't know it would eventually be very embarrassing. I really detest being embarrassed although I don't always succeed in avoiding it.

As to serving the institutional needs of members of the CSG, I'm willing to go a certain distance there, say as far as helping with ideas for experiments and even occasionally writing programs for others to use. I don't know how to get grant money for people, and I have no interest in academic politics, so in those regards, CSG people are on their own. My idea of serving the institutional needs of the CSG is to start an Institute for the Study of Living Control Systems that can operate without the shackles of academia. But I don't know how to do that, either, and I definitely wouldn't want to run it. It's funny how few ambitions I have other than just going on with control theory as long as a few neurons are in working order. A very great deal can be accomplished with nothing more than a home computer and time to think.

Of course funding is a problem for most CSG members, who are raising families, paying off houses, and so on. But it isn't really a problem for CSG research, not now when we are still working with simple ideas. You might say we're the ultimate in Small Science: no support at all (beyond what we can cadge on the sly).

I know it can't go on that way, but as long as it does I can enjoy it.

The second half of your post convinces me more than ever that you belong in and to the CSG. You're a boundary-crosser, precisely what is needed to

bring control theory into the study of living systems without regard to the walls that separate disciplines. You will probably be better at helping with the "institutional needs" of CSG members than I am (although that isn't much of an accolade considering the above).

Your "checkered career" is no worse than mine: 7 years as a medical physicist; 13 years as an electronics systems engineer in astronomy; 5 years as a consultant in designing and building control systems; 11 years working as a microcomputer specialist in the Tech Services department of a large newspaper. Didn't get my PhD, either, while you're evidently going to bite the bullet. And like you, I never earned my living doing anything directly related to my real work. When I complained to Don Campbell about that once, he replied (unsympathetically) that this was the only way that I could have developed my ideas about control theory. He believes in artists starving in garrets, of course. So it goes ...

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Date: Mon, 17 Jun 91 21:28:24 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: The Mechanics of Behavior

[from Gary Cziko (910617.2100)]

Bill Powers (910616.0730)

Re: Lorenz's Goose:

>How do you set up a robot arm to "provide a given amount of force to the >egg, no more, no less?"

I must admit that I thought it would be a lot simpler. I see automatic doors opening and closing all the time (building, garage, bus, elevator) and I just assumed there were using open loop with a constant force. Now I realize that they can be one or the other, but not both. I really enjoyed the physics lesson. Lorenz should be no problem now.

But this now raises another question. Most psychologists (like me) don't seem to know a whole lot of the physics of behavior. But aren't there others who do (or at least should), such as people in the fields of kinesiology and biomechanics? Why are they not using perceptual control theory (PCT) in their work? Or perhaps some already do? If the stuff you mentioned in your reply is really basic mechanics (as it appears to me), it would seem that anyone who has at least this level of knowledge about the physics of behavior would have to use PCT to make sense out of what is happening. Why don't we have lots of these people on CSGnet?--Gary

P.S. A "cute" observation (maybe). Perhaps psychology has spent so much time trying to get the ghost out of the machine, it hasn't realized what the machine has actually been doing all this time.

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Date: Mon, 17 Jun 91 21:02:20 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Levels of perception

[From Rick Marken (910617.1000)]

I'm posting this now, with no editor, because I don't want to forget it and because I might actually have a lot of work to do tomorrow. However, my last little tirade about system concepts and Bill's discussion with Nevin about rhythm control, made me realize that, eventually, we control theorists are going to have to start explaining how you can tell that a variable is of a particular type when you see it. We also have to explain how we can show in some relatively convincing way (if we can) that, say, configurations are all dealt with at the same level (and that there are not configurations that are nestings of configurations) and that relationships are always a higher level of perception than configurations (for example). The model predicts this -- but, as far as I know, there have been no persuasive (objective?) tests suggested for showing the predicted nesting that exists between levels of controllable perceptual variables. My little "number rate" study suggests something about the possible levels (in terms of the ability ot experience certain perceptions when numbers occur at different rates (slower rates for "higher order" variables) but this does not really get at the nesting. Most important, I was throwing around concepts like "system level" perceptions and "principles" and surely mixing them up. How can we make a convincing stab at sorting this out. I suggest that "paying attention" to one's own perceptual experience is a good start. But I would feel more comfortable about this (rationally, not just intuitively) if there were some nice criterion for determining hierarchical relations between perceptual (behavioral) variables. Having a list of variables that have been shown to be controllable (like the one posted by Bill -- with my tiny addition) is a good start. But now the task, I think, is to show that, say, "area" is indeed a configuration (controlled by manipulating LOWER ORDER perceptions -- like sensations) and not just a kind of perception that is controlled by manipulating other perceptions, possibly of the same kind.

I hope to have an answer to this problem by 4:00 pm tommorrow or I'll dock you 10 bonus points.

Hasta Luego
Rick

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Date: Tue, 18 Jun 91 08:47:27 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: reply to Bill

[From Bruce Nevin]

Bill Powers (910617.1800) --

[I just figured out that the reference numbers are only date stamps, and I can reconstruct them for myself from Internet-style message headers. But still, thanks for including them.]

>My, you do cut close to the bone. You are also perfectly correct, if by >"you" you mean me singular.

Oh goodness no, I had in mind what you collectively are making of CSG as you continually reconstitute it by the structure and nonverbal aspects of your communications. I think that you (individually and collectively) have reason not to institutionalize yourselves (pun intended). Bunny Duhl (family therapist) used to talk about the family two-step: presenting patient makes a change; family members are compelled to a back-door change by virtue of staying in (changed) relationship with him; family members resist change in themselves in the way that is obvious to them, viz. by resisting change in their relationships with him; for him, that amounts to opposition to and sabotage of the change he has made; presenting patient changes back. This is why family therapists think it important to work with all (contiguous) members of the family overtly, since they will be doing so covertly in any case. It is the family system that is the patient. Here, you have another human system, much larger and more complex than the family, that like a family in trouble has an epistemological malady. By marginalizing yourself wrt established science and academia (or by enjoying their marginalization of you) you-collective are freed to explore and strengthen your application of a more apt epistemology to experience and thereby your grasp thereof. But there is also a craving to validate and be validated. The common strategy for the adolescent is to distance herself, either emotionally or geographically or both, so as to get the space to find herself. But sooner or later she has to come home and make some reconciliation. Often this happens around age 28-30, a coming into owning her own maturity and her own inner authority, which being no longer externalized no longer needs to be distanced in their person. Sometimes it only happens in a late reconciliation with aged parents nearing death, who after all were doing the best they knew how. When death comes first it is no error for her to feel the loss of reconciliation tragic. It may be that the prodigal comes home, still with those peculiar ununderstandable notions but loved and feted, or it may be that there is just continued incomprehension, no celebration, but the elder must acknowledge the autonomy of the now grown child. And you are right, the child must grow to adult autonomy (master in own realm) first.

Breaking the analogy is the fact that CT is no progeny of psychology or biology but rather something more like a sibling now coming of age. The connection is not through a new paradigm of biology or psychology etc. but through a new metaparadigm of science that seats the experimental

point of view within the experimental system, not outside it, and indeed shows how this can be so which was a conundrum before. So yes, it is well to stay in the eggshell, and then in the nest, until the wings are strong and fledged. Even if you are maybe a cuckoo, no progeny of the nest-builder. (I can see the puns coming on that image!)

[BTW, and reflecting the use of he/she pronouns above, what do you make of the paucity of women here?]

Thanks for the warm welcome. I'm an intuition-based person. Harris has grabbed a good chunk of truth. You and CSG have another. I don't intend to lose track of either, if I can help it.

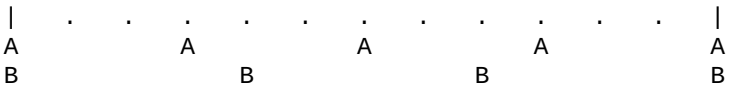
>Your "checkered career" is no worse than mine: 7 years as a medical

I'm no basis for any ad verecundiam arguments. Let some reviewer turn up my 1982 book on astrology and whiff! there goes my scientific credibility. My wife is a channel, and has been teaching psychic development for 15 years. I have those experiences to account for in any comprehensive model of perception and cogninition. I can't just make them go away by the usual non-arguments. I know my wife to be a sincere and honest person, I have seen the changes in her students and clients, I have had conversations with Emmanuel (whom she channels) and have heard him lecture. So be careful. You might not want the CSG to be associated with me.

(You see of course the power of institutions here.)

I'll do some thinking about rhythms and the horizontal construction of configurations into more encompassing configurations vs the vertical relationship of levels.

>reduce 3 against 4 to a single complex rhythm A&B--A-B--A--B-A----A&B.



I agree that for starters I have to run a 12-beat so as to know where the quick A-B and mirroring B-A sequences come on either side of the central A stroke. For me, I would have to work up to grasping the whole pattern--unless I first heard it perhaps, which is how I learned the 2/4 over 3/4 pattern from my dad when I was a kid. I am not a pianist--no room for a piano in a 30' trailer, alas. But with practice with various combinations I might gain more skill in perceiving complex counterrhythms as single gestalten, a skill that some musicians seem to develop much earlier in life or maybe even come in with. So far as speculation goes--that's all it is in my case--we seem to be in agreement.

But the question of pattern is central to any understanding of personality, language, and culture, and the patterns in question are all continually reconstituted moment by moment in myriad interpersonal acts involving unconscious control of unconscious perceptions, as Edward Sapir (my mentor's mentor) showed many years ago.

Got to get to work, don't want to lose my net connection and
coincidentally my job!

Bruce Nevin
bn@ccb.bbn.com

PS--regrettably, I won't be able to sustain this volume. I'll do the best I can, and I continue to play catch-up with hard copy on the train home to Gloucester, an hour and a half each way.

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Date: Tue, 18 Jun 91 10:26:15 -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: back again

Hello everyone,
I've been back for a while but haven't got over to getting on the network--it was especially difficult to get back to it after going to a Greatful Dead show. Anyway, I am not on the net and will probably stay off and use the news until Fall since I don't get to the computers much. I had a lot of comments on your comments on my query concerning being referenced for error (a month ago) but I have since forgotten most of them (why don't I write them down?).

One of you (I forget who) likened my desire for intellectual error to skydiving. I found this to be an interesting observation. Another compared it to setting a goal to writing a book and creating temporary error in order to accomplish that goal. Another said that it was like not being bothered by the pain of putting out a candle with bare fingers--"you identify with the pain; it's not undesirable." These are all interesting and make sense, yet the "indentify" answer is completely different than the "writing a book" or "skydiving" answer.

The book example is the easiest to understand. Set goal at one level and allow errors at lower levels to exist in order to accomplish the higher goal.

I don't know how to explain the "indentify with the pain" metaphor in CT terms. Can anyone do that for me?

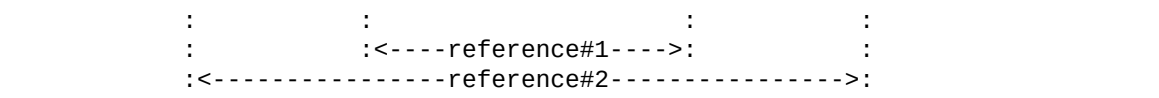
The skydiving example brings to mind another question. Why do we enjoy those things which create the greatest amount of error (which is eventually eliminated). If we want to have fun, we will skydive, ride rollercoasters, climb mountains. I am no different before I do such things than before, yet for some reason I (human beings) enjoy the creation of that temporary error. Sure, we could ride in a balloon, enjoy the scenery and remain safe (no error)--that would be fun, too. But its a differnt fun. We're not skydiving exclusively for the view, but for the satisfaction of experiencing the diffusion of a huge error (at a very high level I might add). Why why why??? To feel immortal? What does that mean in CT terms?

Enough questions?

Carpe Diem
Mark Olson
==========
Date: Tue, 18 Jun 91 12:34:47 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Converted from PROFS to RFC822 format by PUMP V2.2X
From: "Bill CUNNINGHAM - ATCD-GI (804" <CUNNINGB%MON1@LEAV-EMH.ARMY.MIL>
Subject: Importing references

The following thoughts evolved out of a growing sense that HCT must either explain or accommodate interaction between individuals operating in separate hierarchies. Failure to do so would ignore a distinctly human characteristic.
Joel Judd's questions on learning triggered the approach.

Suppose we consider learning as expanding the internal reference set for use in subsequent perceptions. We can do this by individual experience, or we can learn indirectly through the experience of others. The latter is part of what makes us humam. Failure to import information and convert it into internal references would severely constrain our perceptual variety (a polite description, by the way, of closed-minded nonbelievers in HCT). So now the problem is to explain this communication process in a way at least acceptable to HCT. Consider the following familiar communication model using nested encoders/decoders:



The key here is that each matched encoder/decoder pair must have the same reference. No communication can take place with out the common reference. Once an elementary common reference has been established, however, it is possible transfer additional references from the sender to the receiver to expand the coding structure. This is very much like booting up a computer. Using another metaphor from communications, once a carrier has been extablished, it is possible to impose a second order code that conveys more information and is independent (in content) of the first order code. Thus a second order reference can be transferred from sender to receiver. And so on. A child learning to communicate (needn't be spoken) establishes the elementary reference by mimicry (or reinforcement of genetic tendancy or whatever) and begins a continuous bootstrapping process, lasting (we hope) the rest of its life. Those who fail, or cease to import new information, are severely handicapped. Those who are skilled at the process are called good communicators. Those who are good at incorporating new information into their own reference set are called good learners.

I'm not exactly sure how this works, but I think I've described in HCT terms what must take place for information transfer. Part of our individual hierarchy must be organized to import potentential additions to our reference set, and an additional part of our individual hierarchy must compare these candidates with the existing set and accept/reject/modify the incoming and amend the existing set. So now

to explore a little further.

The common reference is the keypart of the communication model, but the references are assumed different for each individual in the HCT model. Recent net traffic talks about common subsets of what are individual reference sets. These common subsets must provide the essential key for encrypting and decrypting. The idea that the common references must also have a common level has not been addressed.

Proposition: For successful encoding and decoding (communication) to occur, not only must the sender and receiver use the same code references, they must apply at the same level. Level identification is part of the code.

This extends the communication model into HCT and allows the individual to import/export references. Part of the "bootstrap" reference exchange, or "spinup" (my previous term) or "getting to know you" involves not only establishing common coding rules, but also common level. When I ask you a question, your response "answers the mail" if you are on the same level, "speaks over my head" if you are responding at a higher level, or "never gets to the point" if you are responding at a lower level. If I receive something clearly, but don't understand (no content reference), I may store it while searching for the appropriate level/reference or I may seek to import additional reference so that I do understand. I'm not a behaviorist but a lot of our interpersonal behavior involves giving and receiving feedback to "calibrate" the communication process. Seems to me, we are dealing with both a transmission level code (say vocabulary) and a content level code that keeps us on the same level.

The result, after significant experience with a given (other) individual is a rapid "calibration" or "spinup". After a while, that process becomes automatic/subconscious and we deal only with content. Long married couples "read each other's mind", diplomats spend time "getting to know you", etc. At this point, I think I could raise a strong argument for requiring HCT consideration in a speech course.

Does that help anybody? Seems it helps any model of teaching-learning. It provides a basis for interaction between individual hierarchies. As for my problem, it would argue that any organizational communication and interaction requires common codes and levels at each interface or else become disfunctional. That does a lot to help me with how my information collection and processing system is designed, how it works, why it fails at certain points. It doesn't matter how the organizational structure is modeled, but it does matter how the information exchanges are modeled. And that opens a whole Pandora's box of information theory questions that would have to consider noise introduced by slight misallignment of the coding references, but which currently are attributed to channel noise. Another day.....

This may seem trivial, but it has taken a while to get here becaue my internal organization for importing/assimilating new references is faulty (slow learner).

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Date: Tue, 18 Jun 91 11:56:09 -0500

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Resolving conflict/sys con

Rick (910617) re Ed and recent sys con discussion:

In order to resolve the conflict I experience each day as I spend time reading and replying to mail, I've decided to change my dissertation topic and submit this for approval: "The effects of pontificating on electronic mail: delayed graduate degrees and iconoclastic theories of human controlled perception."

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>I know. I don't feel that you are. You wouldn't need to anyway -- most of the
>values you mentioned above sound alot like what I would think of as mine
>also. The question is whether ANY particular values of any controlled
>variables can ever be considered absolutely RIGHT from a control theory
>perspective.

This is the point (I think). For a model of a control system, the answer is no. For a control system CUM human being, I'm not so sure.

>I could accept the idea "right" references for system level variables
>(not principles or anything lower that is used to control system level
>variables) if you could convince me that a particular level of a system
>concept was required for survival of the organism.

Isn't there more to (human) existence than just surviving, though? And the remarks about GROUP existence which followed the above make sense. We are creatures of society, not individuals.

>But remember, in order to keep perceptions of system concepts at
>the "right" level, it will be necessary to vary references to the lower
>levels -- and this means changing PRINCIPLES, if necessary.

This is why I asked about "variability" with regards to sys concepts, as well as the origins of SCs and their developmental time frame. I can see principles and other levels varying around unified SCs...

>Bill said it
>well a couple days ago:
>
>>But I don't think it's going to do anyone much
>>good if it's taken as a command from God. If you take it that way, you
>>will never try to work out WHY it's a good idea to love your neighbor. So
>>you'll never grasp the system concept within which this principle makes
>>sense.

This sounds like one of the main objections to many religious practices: unquestioning compliance. As adults, we like to analyze (well, at least most of the cultures with which most of us are familiar do) the things we do (although there is a current beer commercial that says, "Why ask why?").

But for the unquestioning, naive, (suckers?) and children among us, it would seem that principled action, generally directed by someone more mature, is one of, if not the way, to develop SCs. And so churches and schools and TV and friends and politicians and famillies all try to instill in us their standards.

>There is nothing "wrong" with varying principles in this way. It must be done
>to control the system concepts that demand that certain levels of principles
>be perceived.

Perhaps not, except that one returns to the debate about whether there are unchanging SCs (eg. religious ones) to which newer SCs like "capitalism" should be subjected, or vice versa, where religious principles get modified to fit the times. When this happens, things like religion begin to get relative with respect to man's socio-historical whims--which seems to suit most people fine, since God is man's creation, anyway.

>If there really are certain system concepts
>that are better than others (for group survival) then it might be helpful to
>try to articulate what they are, rather than claiming that certain principles
>(which are used to control these concepts) are absolutely correct.

I think this would be fruitful, for two reasons. One, as I asked before, teachers and others are doing this all the time anyway; are we all satisfied with such influences? How can this issue be addressed (if not providing specifics, then increased awareness of the mechanisms at work). Two, it seems like these could provide testable hypotheses.

With similar respect,

Joel Judd

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Date: Tue, 18 Jun 91 12:24:11 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: phonolgy and culture

Bruce Nevin (910617)

It is good to see some more language material appearing. I'm especially glad to see that you have some expertise in phonology, which continues to carry some special status, especially in the case of SLA. One of the things I'm trying to do is decide what a Hierarchy for language might look like, and phonology is right there at initial levels. You ask

>Articulator configurations are VARIED in order to control--what?

It would seem that an unsatisfactory, yet correct answer might be "It depends on the level of perception." At the very highest level, they are

varied to produce a perception of 'language,' and so on. Yet because it is the level at which spoken communication interacts with the external environment, phonology carries with it some unique baggage. To produce a given phone, we must perceive that we have produced it. What that means, exactly, in CT terms, I'm not sure yet. But to complicate things phonology is intimately involved in other aspects of communication, as your anecdote shows. It reminded me of Alexander Guiora's "ego-permeability" experiments on L2 learners and his ideas about the one's pronuniciation reflecting one's personality. Taking this psycho-analytic attitude, SLA becomes, at a fundamental level, the "discarding" of one's native ego in order to efficiently function in another cultural/linguistic setting. Perhaps few people are really ready to do that, hence foreign accents.

Re your comments on the family "whole" being greater than the sum of its "parts", and cultural comparisons, brought to mind a book I've mentioned before, Jerome Bruner's _Acts of Meaning_, as well as a book he references and a faculty member recommended, Reynato Rosaldo's _Culture and Truth_. Bruner's concluding study of a "family" and Rosaldo's ethnographic perspective I think you would find attractive.

Joel Judd

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Date: Tue, 18 Jun 91 14:30:10 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: phonology

[From Bruce Nevin]

Joel Judd <jbjg7967%UXA.CSO.UIUC.EDU@vmd.cso.uiuc.edu>
Tue, 18 Jun 91 12:24:11 -0500

Thanks for your comments, just a very quick and fragmentary reply. The question

>Articulator configurations are VARIED in order to control--what?

was Bill's addressed to me. He was testing whether I was looking at output or at control.

>is intimately involved in other aspects of communication, as your anecdote

I guess you mean the Oklahoma Speech Dept. stuff?

> Alexander Guiora's "ego-permeability" experiments

I'd like to know more about this.

>one's personality. Taking this psycho-analytic attitude, SLA becomes, at a
>fundamental level, the "discarding" of one's native ego in order to
>efficiently function in another cultural/linguistic setting. Perhaps few
>people are really ready to do that, hence foreign accents.

Reluctance to undergo status of an infant again, more like.

Thanks for refs to Bruner, and to Rosaldo of whom I had never heard.

Bruce Nevin
bn@ccb.bbn.com

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Date: Tue, 18 Jun 91 16:22:12 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: saturn.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: manuscript available

From: David Goldstein
About: Towards A Perceptual Control Theory Psychotherapy
For those CSGnet people who are interested in psychotherapy, I wanted to let you know that the above manuscript is available upon request. Just send me a note indicating your interest and your mailing address and I will send you a copy.

I will not be attending the CSG meeting this summer and would have probably presented it there.

So let me know.

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Date: Tue, 18 Jun 91 17:23:02 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Converted from PROFS to RFC822 format by PUMP V2.2X
From: "Bill CUNNINGHAM - ATCD-GI (804" <CUNNINGB%MON1@LEAV-EMH.ARMY.MIL>
Subject: We need a lobby like the NRA

First posting seems to have gone astray. I'm not into chain letters, but this one is perceived as grist for CSG at the CS level.
Bill Cunningham

From: CPT PAUL K. DARRAGH, 680-2438
To: BB --MON1 DOIM BULLETIN BOAR POULINJ --MON1
PANKOKET--MON1 BB --MON1 ISO BULLETIN BOARD
SCOTTIS --MON1 WITTYR --MON1
MARKEYV --MON1 DAURIAC --MON1
PABSTJ --MON1 MESSERSD--MON1
WARDD --MON1 BROOKSR --MON1
BECKMANE--MON1 SAROR --MON1
MUGLERS --MON1 DEMPSEYH--MON1
ATCCR --MON1 LASSITES--MON1
AVERSANF--MON1 MCDANIE2--MON1
GARNERA --MON1 SURLESC --MON1
PARNELLJ--MON1 HUMEC --MON1

FROM: DON GREGORY, X-2578
*** Resending note of 06/03/91 08:51
To: GREGORYD--MON1

FROM: TOM BURKE
*** Resending note of 06/03/91 07:50
Recommend BB posting for this note, tks
Subject: Modem-user Tax

This message is importance to anyone who uses a modem for access via commerical phone lines.

Date: Fri, 24 May 91 12:43:52 PDT

Dear Friends,

I received the following message at a recent conference organized by the Southern California Regional HP Users' Group (SCRUG). Everyone at the conference agreed to pass on the information verbatim.

FROM: MATT DOMSCH SUBJECT: MODEM TAX

A new regulation that the FCC is quietly working on will directly affect you as the user of a computer and modem. The FCC proposes that users of modems should pay extra charges for use of the public telephone networks which carry their data. In addition, computer network services such as Compuserve, Tymnet, & Telenet would also be charged as much as $6.00 per hour for use of the public telephone network. These charges would very likely be passed on to the subscribers. The money is to be collected and given to the telephone company in an effort to raise funds lost to deregulation. Jim Eason of KGO newstalk radio (San Francisco, CA) commented on the proposal during his afternoon radio program during which he said he learned of the new regulation in an article in the New York Times. Jim took the time to gather the addresses which are given below.

What you should do: First, take the time to download this message and the letter which follows. Next, find three or more other BBS systems which are not carrying this message and upload this text. Finally, print three copies of the letter which follows (or write your own) and send a signed copy to the three addresses. It is important that you act now. The bureaucrats already have it in their mind that modem users should subsidize the phone company and are now listening for public comment. Please stand up and make it clear that we will not stand for government restriction on the free exchange of information.

The three addresses to write to: (a letter to send follows)

Chairman Federal Communications Commission
1919 M Street NW
Washington, DC 20554

Chairman Senate Communication Subcommittee SH-227
Hart Building
Washington, DC 20510

Chairman House Telecommunication Subcommittee B-331 Rayburn Building
Rayburn Building
Washington, DC 20515
Dear Sir:

Please allow me to express my displeasure with the FCC proposal which would authorize a surcharge for the use of modems on the public telephone network. This regulation is nothing less than an attempt to restrict the free exchange of information among the growing number of computer users. Calls placed using modems require no special telephone company equipment, and users of modems pay the phone company for use of the network in the form of a monthly bill.

In short, a modem call is the same as a voice call and therefore should not be subject to any additional regulation.

Yours truly, ...

<end of message>

********** Robert (Bob) L. Stringfield, Computer Systems Analyst Mainz Army Depot Truth: IGNORANCE hates knowledge....
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Date: Wed, 19 Jun 91 07:20:37 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Misc replies

[From Bill Powers (910618.1830)]

Mark Olson (910618) --

The moral may be that going to Grateful Dead concerts conflicts with being a control theorist. Oh, woe!

>One of you (I forget who) likened my desire for intellectual error to
>skydiving. [Etc.]

I'll offer a new one: dangerous pastimes are practice for getting your control of physiological systems coordinated with control of motor behavior. Eventually you reorganize so that knowledge that you are actually safe (despite the apparent danger) is sufficient to prevent a panic reaction at the physiological level (superseding primitive built-in control systems). The "thrill" subsides to mere preparedness, leaving you organized for effective action. Unless you have become addicted to the thrill for extraneous reasons, you then cease the activity because "the thrill is gone."

>Why do we enjoy those things which create the greatest amount of error
(which is eventually eliminated).

A short answer is that we enjoy such things if we set our reference levels for them relatively high.

I don't think we do seek maximum error unless something is drastically wrong inside. If you enlarge your view to include more than little errors

that are normally considered entertaining, I think you will agree that we avoid situations that create the LARGEST errors. You avoid skydiving with partially-collapsed parachutes. You don't climb overhangs with a frayed rope. You don't get on a roller coaster with a "Condemned" sign on it. You don't put out a blowtorch by pinching it with your fingers. You don't even put out a candle G. Gordon Liddy's way if you're going to need that hand to do something delicate (and important) afterward, or even if you're just going to play a guitar for some friends. Broken blisters make the strings too slippery. People who seem to court danger normally go to great lengths to make sure there isn't any danger. They're developing skills, not trying to end it all.

Seeking danger -- real danger, where you can be maimed or killed -- requires overcoming, or never developing, an accurate sense of the consequences that are likely. A person who plays Russian Roulette either has no plans for the rest of his or her life, or has developed a sense of invincibility based on a run of good luck or on a mystical sense of protection from normal consequences. I have known people who felt that enduring and escaping danger proved that they were special in some way. I was one of them, once. I am still alive because I finally grasped the actual possiblities and because I found some very attractive reasons for staying alive. Some of those who didn't grasp this point or find such a compelling reason are dead. Naturally.

People in addiction-recovery programs have said that both drinking and drug usage, for addicts, are daring death -- each time you don't die you say "See? I can get away with it!". And the next time you try a little harder to die.

But this is all content and not form. It doesn't say anything about control theory. Seeking and avoiding danger are learned, because "danger" itself is a learned concept. If you don't learn it you will die sooner than if you do. If you learn it but don't connect it to your other goals in life, you will also die sooner. It's all a matter of how you reorganize and how you're set up to evaluate the results of reorganization. If you do things that hurt you, you will reorganize, but there's no guarantee that you will do so in a way that saves your life before a statistical bobble puts you over the edge into death. Nature doesn't care which happens. The reorganizing system isn't smart enough to care.

Bill Cunningham (910618) --

You seem to be saying "reference" where I would say "perception." Can you be more specific about your meaning for "reference?"

Also, the terms "encode" and "decode," while not strange to me, seem to imply a model of perception quite different from mine. Can you elucidate?

I can agree with the general outline of your picture of communication. But there are some mysterious aspects to it: for example, exactly how would "importing a reference" work? And how do encryption and decryption get into the act? Is there some reason to make the messages harder to understand by encrypting the codes?

When you say that "level identification must be part of the code," I think you're getting into an untenable model of how perception works. If perceptual systems are organized as I propose (which is pretty straight neurology), it isn't necessary for levels to be identified, nor is it possible for a neural signal to carry information denoting both how much of a given perception is present and what kind of perception it is -- not to mention what level of interpretation should be given to it. I think your proposition contains a lot that isn't necessary, and you're loading neural signals with a burden they are not designed to carry.

When you say
" .. a lot of our interpersonal behavior involves giving and receiving feedback to 'calibrate' the communication process" you're turning control theory into a metaphor. Beware of words that you have to put into quotes because they don't literally match what you mean. Feedback, technically, is the effect of a variable in a closed loop on itself. You don't "give feedback" to someone. Feedback is the result of the other person's actions on the perceptions in that person affected by those actions. No matter how you respond, or fail to respond, to someone else's attempt at communication, feedback exists. You have no control over that. When you ask a child "What time did you come home last night?" and the child maintains silence, that is feedback that relates to your perception of communication and of the child. If you have control over whether someone else gets feedback or not, the other person has no control and there is no feedback.

"Calibration" and "spinup" are not technical terms, are they? I don't think we need to avoid technical terms on this net, although non-technical metaphors may help in communicating with those unacquainted with control theory. I think it would be better to establish a vocabulary of exact meanings before we start using metaphorical shorthand for them. The meaning of the control-system model can too easily be lost if we don't keep the basic relationships of control clearly in mind and avoid images that contradict the basic logic of control. We must propose arrangements that at least in principle can be tested as generative models -- i.e., models that behave according to their own explicit rules.

Joel Judd (910618) --

>As adults, we like to analyze (well, at least most of the cultures with
>which most of us are familiar do) the things we do (although there is a
>current beer commercial that says, "Why ask why?"). But for the
>unquestioning, naive, (suckers?) and children among us, it would seem
>that principled action, generally directed by someone more mature, is
>one of, if not the way, to develop SCs.

This, too, is the way in which "someone more mature" gains converts and exercises power, regardless of the merits of that someone's system concepts. Someone has to take responsibility for what is taught.

I don't think that the development of one's own system concepts is optional. Without them, principles are chosen at random or at the whim of any persuasive person. Autonomy requires not only that you HAVE system concepts, but that you have the ability to modify them and acquire new ones that enhance your prospects for controlling what happens to you.

Nobody else knows how a given system concept will interact with your other system concepts.

The ultimate criterion for a "right" system concept is one that fits internally with all other system concepts, both directly and in terms of the required lower-level goals and actions. I believe that there are natural physical and logical constraints on what system concepts will prove best. In a society composed of autonomous control systems, only certain ways of living together will enable individuals to seek their own conceptions of the good without acting on other people in a way that frustrates that very seeking of the good. There's a lot of latitude -- it's probably easier to talk of ways that don't work and the reasons that they don't work.

Control theory gives us a pretty good idea of what those reasons are, particularly if we assume that people will normally try to reach agreement on system concepts (the most obvious way to avoid conflict). Lying, for example, gives other people an incorrect picture of the effects of their actions (when they must rely on communication). A society that accepts lying under its system concepts will weaken or destroy everyone's capacity to control cooperatively. All the deadly sins imply principles that, if allowed under a common system concept, destroy the organization that endorses them. The reasons are neither subtle nor complicated. All the commonly-recognized sins end up creating conflict with others, and others' attempts to prevail in their own processes of control will counteract one's attempts to reach the misguided goal. From the greedy, it will be taken away. Who lives by the sword will die by the sword. Give Caesar what he wants and he will stop bugging you. If someone compels you to walk one mile with him, go cheerfully and chattily for two miles, or however far it takes for him to be sick of your company and order you to go where you wanted to go in the first place. All good control-system advice, for someone who understands the concept of a control system.

System concepts can, of course, be proposed and taught. But someone has to accept the proposal and the teaching, convert them into a real internal way of perceiving and acting, and test the result against direct experience to see if it actually works as advertised. Unfortunately, we can't pass system concepts directly from one brain to another. What is understood is never, at first, what is meant. As adults, we always begin with an organization that works under DIFFERENT system concepts and controls DIFFERENT perceptions. The new always hooks up to something familiar at first. The greater the novelty of the new idea, the more unhooking has to be done; the more radically will the initial understanding change before the learner finally feels the lightning bolt and says "Oh, my God, is THAT what you meant?" (And answers, "Yes, of course it is.") At that point, of course, it doesn't matter any more how the system concept got in there. Or where you think it originated.

Joel Judd and Bruce Nevin (910618ff) --

Why do you have to put terms like "ego permeability" into quotes? Aside from the purpose of making me throw up by using them at all?

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Date: Wed, 19 Jun 91 07:57:32 -0600

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Addendum to Mark Olson

[From Bill Powers (910619.0745)]

Mark Olson (910618) --

A belated thought about your last post, where you said

>One of you (I forget who) likened my desire for intellectual error to
>skydiving. I found this to be an interesting observation. Another
>compared it to setting a goal to writing a book and creating temporary
>error in order to accomplish that goal. Another said that it was like
>not being bothered by the pain of putting out a candle with bare
>fingers--"you identify with the pain; it's not undesirable." These are
>all interesting and make sense, yet the "indentify" answer is completely
>different than the "writing a book" or "skydiving" answer.

.. and later followed with

>If we want to have fun, we will skydive, ride rollercoasters, climb
>mountains. I am no different before I do such things than before, yet
>for some reason I (human beings) enjoy the creation of that temporary
>error. Sure, we could ride in a balloon, enjoy the scenery and remain
>safe (no error)--that would be fun, too. But its a differnt fun. We're
>not skydiving exclusively for the view, but for the satisfaction of
>experiencing the diffusion of a huge error (at a very high level I might
>add). Why why why??? To feel immortal? What does that mean in CT
>terms?

These are not really control-theory questions. They are questions of fact. Does a person seek situations where symptoms of error are felt? Does the person perceive skydiving, rollercoastering, climbing mountains as "fun?" Is the purpose the satisfaction of feeling symptoms of error diffusing? Does the person wish to feel immortal? If the answers to these questions are yes for a given person, then they are yes. We don't answer the questions by saying that these are human traits (not in control theory) common to all people (which they're not). We approach such questions by asking "If it is so that these goals exist, how does the control-system model apply in this person's case?" The answer is the same in every case: seeking goals is a process involving perception, comparison, and action, organized in a negative feedback loop. What we want to do is lay out the structure of this person's organization, given these hints as to this one person's perceptions and goals.

Don't confuse control theory with other theories that try to classify human beings in terms of specific goals or perceptions that they share. Control theory is first of all a theory of the individual. The facts that are true of one individual are not necessarily true of another. What people share is not traits but the basic organization that can support perception and control.

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Date: Wed, 19 Jun 91 11:26:27 EDT

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Converted from PROFS to RFC822 format by PUMP V2.2X
From: "Bill CUNNINGHAM - ATCD-GI (804" <CUNNINGB%MON1@LEAV-EMH.ARMY.MIL>
Subject: Information transfer

Bill Powers(910618.1830)

I plead guilty to monumental muddling of mixed metaphors. It will take a while for me to digest and respond, but let me quickly clear up several points.

I am wrestling with how an individual, operating on his/her own hierarchy, receives and processes information from somebody else operating on their own heirarchy. Receipt of the information alters the perceptions of the recipient. I'm seeking to understand this within the rules of HCT. My current understanding of HCT implies (to me) that individual A's perceptions are independent of individual B's perceptions, with no mechanism for transfer (however imperfect). That contradicts what we know about communication and has to be resolved.

The communication model is used because the issue is information transfer. Any help in simplifying the description would be most gratefully received.

Encoding/decoding are not the same as encryption/decryption, although they have similar features. Encoding is a necessary transformation of information into a form suitable for transmission over a medium. Receipt requires sensing the presence of information in the medium and suitable decoding to retrieve the information. The communication model illustrates a requirement for the encoding and decoding processes to have a common reference. Language--spoken, written, nonverbal--is a coding system, or perhaps systems. Codes are essential for communication.

Encryption is a coding scheme where the intent is obfuscation--denial of information transfer to anybody who cannot employ the correct decryption process.

My muddled metaphors are examples of poor encoding because they prevented unambiguous decoding by the receiver, leading to problems with the receiver's perception of what was sent. The advice to use more precise language points this out.

My proposition that communication requires nested coding schemes, with level identification as part of the code may be utter nonsense. It is an attempt to explain how individual A with a perception at a given level might cause individual B to have a similar perception.

Bill Cunningham

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Date: Wed, 19 Jun 91 12:06:04 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: metaphors, quotes

As I understand him, Bill Cunningham is trying to apply control theory to the hierarchical communications problems of the U.S. military. Whether a human organization is a control system or not, to what extent saying so is a metaphor, and how reliable (useful) such a metaphor might be, are not clear to me, though it might seem that I thought so given previous words about family systems. (Will return to mulling the bit about chair backs and arms vs. chairs, parallel control systems on same level, etc. when I get time.)

Example of interaction ritual used (I think) for calibration: different voices have different normal pitch ranges. Semantically empty "hello, how are you" greeting rituals may enable conversation participants to tune in, determine where F0 (fundamental frequency, lowest formant) is located. This was a supposition of Leigh Lisker years ago.

The permeability quote was just that, a quote from Joel's message in my case and (I think) a quote by Joel from Guiora's writings. I don't know what the heck he meant by it, and that's why I asked. "I gotta use words when I talk to you." (Eliot--Burnt Norton I think)

Bruce Nevin
bn@ccb.bbn.com

Date: Wed, 19 Jun 91 12:46:30 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Guiora et al

[from Joel Judd]

Sorry I mixed up the source of comments. Here's some Guiora refs:

Guiora, A. et al. 1972. The effects of experimentally induced changes in ego states on pronunciation ability in a second language: an exploratory study. _Comprehensive Psychiatry_ 10:421-428.

Guiora, A. & Acton, W. 1979. Personality and language: a restatement. _Language Learning_ 29:193-204.

Guiora, A. et al. 1980. The effects of benzodiazepene (Valium) on permeability of language ego boundaries. _Language Learning_ 30:351-63.

Enjoy, but don't try these at home-- leave it up to the professionals.

Joel Judd

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Date: Wed, 19 Jun 91 12:46:40 -0500

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: Misc replies

[from Joel Judd]

Bill (910620),

>Someone has to take responsibility for what is taught.
>
>I don't think that the development of one's own system concepts is
>optional.

I didn't mean to imply SC development is optional. For the very reason it's NOT optional your first statement is important to me. I think that if nobody does anything, then there will develop something by DEFAULT.

Thank you for the timely comment on feedback. There have been some questions on a language network concerning it lately.

>Why do you have to put terms like "ego permeability" into quotes? Aside
>from the purpose of making me throw up by using them at all?

I'm laughing so hard I can hardly type. It's just that Bruce's comments reminded me of these studies. I'm not holding them up as paragons of SLA research. They're basically little more than justification for getting someone drunk or drugged and showing how their pronunciation improves up to a point (all scientific, of course). I haven't reread them since since becoming aware of CSG. I'll have to see how they read now.

Speaking of this, however, is there anyone on the net who could address pharmacology from a CT standpoint?

Joel Judd
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Date: Wed, 19 Jun 91 13:42:30 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: code, information, language

[From Bruce Nevin]

Bill Cunningham:

You are talking about communication of information from one person to another.

What do you mean by information? The information-theoretic term avowedly does not apply to information as we informally understand that term, it is simply a measure of the cost of transmitting a given sequence of distinguished tokens called a "message". The socalled "message" can even be meaningless in the usual sense so long as the tokens must be kept distinct and there can therefore be a definition of

transmission error.

Language is *not* a code. Formal relationships among parts of an utterance themselves constitute the information "in" or "carried by" that utterance. (Vessel metaphor or portage metaphor, you pick.)

References if you like. For starters:

Harris, Z. S. 1989. _Language and Information_. Columbia U. Press

Harris, Ryckman, Gottfried, et al. 1990. _The Form of Information in Science_. Boston Studies in the Philosophy of Science. Dordrecht: Reidel.

Ryckman, Thomas. 1986. _Grammar and Information: an investigation in linguistic metatheory. PhD. dissertation, Columbia University. (Unpublished because referees refuse even to read it.)

I believe that some basic formal relationships in language correspond to formal relationships between control systems in a human being (some variation from person to person, from family to family, from community to community, from culture to culture). Those that do not directly so correspond are understood by analogy to ones that do (ones that, in this sense of correspondence, the person does directly control). Analogy (metaphor) is fundamental to how language works and in particular to how people adapt it, and consequence how it grows and changes.

I believe this relation between structures in language and both (a) structures in the set of control systems in a human being and (b) social constructs are essential for understanding higher levels of the control hierarchy.

(My oh my do I wish I could just work on this! But back to work on T/300 traps. Lunch is long since over.)

Bruce Nevin
bn@ccb.bbn.com

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Date: Wed, 19 Jun 91 13:59:56 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Speech, levels

[From Rick Marken (910619)]

Well, I guess my question about how to determine hierarchical relationships between perceptual variables was too tough. Nobody gets the bonus points.

How about a little discussion about a behavior that people spend so much of their time doing -- speaking. I think this topic was broached briefly. Speaking words seems like an excellent example of controlling perceptions. I think that there is considerable evidence that speaking involves the control of auditory perceptions (in hearing speakers -- other perceptions in

deaf speakers). The evidence I am thinking of is research which involves distorting the auditory signal produced by the speaker. I know of studies where the signal is delayed, filtered and masked. In all cases there is "deterioration" of the speech "output". What is never done, as far as I know, is an analysis of the "distorted" output to see if it is what would be expected to preserve the perceptual input. I think it should be possible to add disturbances to the auditory output which would require predictable variations in the output in order to preserve some aspect of the input -- if the speaker is controlling that aspect of the input -- ie-if it is a controlled variable. One varable that might be controlled during speech is the signal to noise ratio of the input. There have been some studies on this -- and I think they do show that S/N ratio at the input does stay relatively constant with variations in N (the noise disturbance created by the experimenter). I would like to see studies where continuous, low frequency variations in noise and signal level are monitored (at the input to the ear) to see if the ratio is continuously controlled. More interesting studies would also be possible using digital signal manipulation to change the speech output signal before it enters the ear. For example, it might be possible to change the temporal structure of the auditory signal within a word in real time. So you "stretch out" the "r" part of "rick" and shorten the "i" part and make the "k" normal. This could involve nothing more than repeating or removing samples of the time waveform of the auditory aotput. I would bet that even these gross changes in the signal (if not too gross) would lead to compensatory articulatory efforts aimed (possibly somewhat successfully) at producing the intended input signal pattern -- and maybe not.

Any thoughts?

****

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

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Date: Wed, 19 Jun 91 16:01:46 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: manuscript available

David:

>About: Towards A Perceptual Control Theory Psychotherapy
>For those CSGnet people who are interested in psychotherapy, I
>wanted to let you know that the above manuscript is available
>upon request. Just send me a note indicating your interest and your
>mailing address and I will send you a copy.

I'd like a copy. I assume you mean that you will send it by email. Either way OK with me.

>I will not be attending the CSG meeting this summer and would have
>probably presented it there.

We'll miss you at Durango. Sorry you can't be there.--Gary

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Date: Wed, 19 Jun 91 17:52:19 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Linguistics

[From Bill Powers (910619.1730)]

Cunningham, Nevin, Judd, other linguists --

Rather than Big Daddy trying to solve the communication problem, I'd like to encourage you linguists to put together your own control-theoretic analysis of how communication works. I'll try to give you some hints, the first of which is that the *structure* of the solution should be very simple, even if its details aren't.

Forget about coding, encryption, information content, formal constructs, and "language." Just consider one person who has a meaning in mind: say a pattern of coins on the table in front of him/her (it). The object of communication in this case is to create an image in the imagination of the second person that is as close as possible to the same arrangement of the coins. Leave out markings, size, color, etc. We're just trying to communicate a spatial arrangement, at no more than the category level (i.e., you're allowed to name the coins to distinguish them).

Let's assume, too, that this is vocal communication. This tells us that the only medium for transmitting any meaning is sound waves coming out of one mouth and entering several ears (probably four). There is nothing in these sound waves but a small amount of physical energy.

Last hint: both the speaker and the hearer of these sound waves experiences them first as a string of varying intensity signals; speaker and listener have identical perceptual organizations (let's make this easy).

Now, just reasoning it out from scratch, what has to happen in speaker and listener in order for a reasonable facsimile of the intended meaning to appear in the experience of the listener?

David Goldstein --

I would like a copy of your paper. P.O. Box 2566, Durango, CO 81302-2566.

Rick Marken (910619) --

Excellent proposals for a basic experiment in communication -- and another hint for my suggestion to linguists, above. Given the fancy equipment that experimenters now have available, putting disturbances into the feedback path, at least the auditory path, should be easy. The correction of the heard result might be hard to measure, but if human listeners could listen to the direct vocal output, then the output plus disturbance that the speaker hears, they could probably judge which was least distorted, with reasonable reliability. I hope there is someone out there with the equipment and the desire to do this eminently doable experiment. It would settle a lot of questions.

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Date: Wed, 19 Jun 91 20:10:07 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Journal Suggestions

[From Rick Marken (910619.2000)]

I could use some help from CSGNet members. What do you think is a reasonable forum for publication of my paper on "Hierarchical behavior of perception". If you havn't read it, the paper is basically a look at "behavior" as the behavior of perception; it is a theoretical paper that presents some research that is consistent with the idea that "behavioral" limitations are often limitations on the actor's ability to perceive the intended results of actions. I though the paper would be perfect for the journal Psychological Review but it was rejected because the "ideas were not new", it was "not up to Psych Review standards (perhaps I did not refer to enough research) and it didn't seem to account for some phenomena that the reviewer was interested in (I bet few papers submitted in Psych Review are rejected because the theory described in the paper cannot account for control phenomena -- such are the benefits of being in the establishment). Rather than rewrite the thing (if I account for the reviewers results I will just get new ones -- believe me) I submitted it to Psych Bulletin. I just got a nice letter back from Robert Sternberg (the editor) saying the Psych Bulletin publishes literature reviews, not theory. He suggested sending it to Psych Review (it's nice to know that there's not collusion between journal editors). He said that he thought that I had interesting ideas and that I should submit to the Bulletin when I had a literature review -- definitly a nice guy. Anyway, where should it go next (besides the waste basket). Behavioral & Brain Sciences is out -- for me. I don't mind submitting when there is the possibility of rejection; but it's no fun when it's a certainty. If Harnad doesn't want to publish Powers, he certainly is not going to publish Marken.

I was thinking maybe Cognitive Psychology or Acta Psychologica.
A do want something that might reach a reasonably broad and
discerning audience. Any other ideas. If you have any good
ideas for possible venues for my paper, please post them to me
(personally or to csgnet) and, if you can, let me know
the name and address of who to send it to, the number of copies
to send and any other annoying rituals that musty be performed
in order to get it looked at.

Thanks so much

Rick M.
marken@aerospace.aero.org
==========
Date: Thu, 20 Jun 91 08:26:30 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Converted from PROFS to RFC822 format by PUMP V2.2X
From: "Bill CUNNINGHAM - ATCD-GI (804"
<CUNNINGB%MON1@LEAV-EMH.ARMY.MIL>
Subject: Coining phrases

Cunningham:

Bill Powers (910619.1730) Right on! That reduces the question to
its nub. A linguist I'm not. Information theory is closer to home,
but agree the approach here should not get bogged down in esoterica.

I have a shotgun pattern of office & travel obligations over next 7 days.
I'll be in and out of contact, but not far from the problem.

Bruce Nevin (910619.1342) "Formal relationships among parts of an utterance
constitute the information..." That defines a code very well. So we
obviously mean something different. Will work on this.

My visitors are here.
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Date: Thu, 20 Jun 91 10:58:42 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: language control

[From Bruce Nevin]

Rick Marken (910619)

Rick, I concur, yours is an excellent proposal.

First, you would have to set up the apparatus so that the participant
could "tune" the sound received in the headphones so that it sounds
normal--that is, so that it matches the sound transmitted most strongly
through the bone and tissue of her own skull and partly through
surrounding air (the latter attenuated roughly as is the sound from a
speaker facing away from you while seated on your lap, not, as usual,

facing toward you). Control here would involve mostly relative intensity at different frequencies, I think. I suppose you could compare listening while saying something with and without feedback through the headphones.

The alternative is to require the participant to get used to hearing his own voice more as others do, assuming this will not interfere with the control experiment.

Real-time distortion of length of a single segment requires some means of identifying the segment. I think it would be easier to lengthen all voiceless stops (p, t, k, and glottal stop in English), since these come out as an interval of silence--no sampling required, recognition relatively easy. Other factors, such as the miniscule transitions in vowel formant frequencies either side of silence that distinguish one voiceless stop from another, are tougher to recognize and manipulate.

Lengthening segment length creates a disparity between the perception of articulatory position and contact and the perception of acoustic features. I believe both kinds of perception are concurrently controlled during speech. The length of segments of silence for voiceless stops is already brief. Distorting acoustic feedback by lengthening these segments will induce further shortening, running into physical limits as to how quickly the tongue can change its shape. Compensating for shortened segments of silence won't run into this wall.

If the time variable t by which designated segments is increased gradually from zero, it is possible that progressive deviation from normal speech cadences would not be noticed, up to some discoverable (rubbery) value for t.

Some problems:

1. Speakers don't just get their perceptual feedback through the air to their ears, but also through the structures of the skull, as mentioned. The participant will feel her larynx start to vibrate a beat ahead of the resumption of the vowel in her ears, and a natural interpretation might be of delay in the auditory loop, as in speaking over a PA system where the speakers are some distance away (stadium, train station, etc.)

2. It may turn out that speakers control for syllable length, or for the relative length of syllable constituents. More sophisticated distortions of the acoustic signal feedback would be required to test this.

I will ask John Makhoul if there are means in his speech analysis lab here at BBN to do this, and if I can set up such an experiment sometime in my "spare time". More remote possibility is talking with Kenneth Stephens at MIT. Might be means to do some more sophisticated distortions. I picked what I think is the simplest to recognize and distort assuming I would have to find (somewhere) some software and I/O devices for my PC. ("Spare time" is a poor joke.)

>variable that might be controlled during speech is the signal to noise

>ratio of the input. There have been some studies on this -- and I think
>they do show that S/N ratio at the input does stay relatively constant
>with variations in N (the noise disturbance created by the experimenter).
>I would like to see studies where continuous, low frequency variations
>in noise and signal level are monitored (at the input to the ear)
>to see if the ratio is continuously controlled. More interesting

I don't understand this. Could you unpack it a bit?

Bill Powers (910619.1730)

>the *structure* of the solution should be very simple, even if its
>details aren't.

>consider one person who has a meaning in mind: say a pattern of coins on
>the table in front of him/her (it).

>The object of communication in this case is to create an image in the
>imagination of the second person that is as close as possible to the
>same arrangement of the coins.

>Leave out markings, size, color, etc. We're just trying to communicate a
>spatial arrangement, at no more than the category level (i.e., you're
>allowed to name the coins to distinguish them).

>this is vocal communication. the only medium for transmitting any
>meaning is sound waves coming out of one mouth and entering several ears
>(probably four).

>There is nothing in these sound waves but a small amount of physical
>energy. . . . both the speaker and the hearer of these sound waves
>experiences them first as a string of varying intensity signals;

>speaker and listener have identical perceptual organizations

>Now, just reasoning it out from scratch, what has to happen in speaker
>and listener in order for a reasonable facsimile of the intended meaning
>to appear in the experience of the listener?

Suppose the speaker says "four cents" (roughly [fo@s&n's], where @ is the low-centralized vowel called schwa and & represents a low-mid front vowel usually represented phonetically by epsilon, with peaks of intensity on [o] and [&]; ['] is glottal stop].

Speaker	Listener
(expelling air with diaphragm in pulses for two syllables concurrently with the following)	(Having previously heard enough of the speaker's voice to know the pitch ranges for vowel formants, possibly to calibrate for dialect differences too)
presses lower lip against upper teeth	hears disorganized hissing noise with most energy concentrated at lower frequencies for sh, which in turn does not go as high as s, but not restricted

	from higher frequences as the [x] of Bach.
arranges tongue so that it is "lumped" toward the back of the oral cavity, with the tip lowered, body raised higher than for "far" or "fought" but not so high as for "foe", "foot", or "food"	
projects lips forward in rounded position, not so much as "foe" etc. but more than for "far, fought"	hears F0 constant, F1 and F2 low, and rising slightly to positions relative to F0 just
concurrently increases tension on vocal bands so that vocal vibration begins	below that characteristic of some vowels, above that for others, converging toward intermediate positions characteristic of [@]
relaxes lips to slightly more spread position while	
lowering body of tongue to mid position in oral cavity	
spreads vocal bands apart while raises lamina of tongue to controlled proximity to alveolar ridge so that air forced through aperature produces [see under hearer]	hears disorganized hissing noise with most energy concentrated at higher frequencies than for sh, f, or x
lowers back of tongue and raises front (nearly equal) while spreads lips (horizontally) while increases tension on vocal bands to resume vocal vibration	hears energy at formant frequencies for [&] vowel
. . .	<I am not sure I got the relative positions of formants right, it's been a few years since I read Lehiste et al.>

What the next level up of organization in phonology should be is a matter of some contention. Certainly, speakers and hearers are controlling for differences that make a difference, and one way of thinking of these is as phonemic contrasts or (with some slippery reification) phonemes. Morphemes (minimal meaningful elements of a language, in one time-honored formula) contrast with one another and the minimal differences between them can be teased out by phonemic analysis (with more than one phonemic solution possible). I see a glimmer of

suggestion that the status of segments, features, and the like may be just artifacts of control processes (and of course convenient tokens for orthographies).

Skipping over that hoof-torn turf:

Speaker	Listener
controls articulation and for sound for the distinction of "four" from:	controls what? does not subvocalize so far as we can determine, and that would make no environmental difference to feed back
pour, tore, chore, core, soar, more, nor, lore, roar, . . . fear, fair, fer[ret], fir, far, . . .	
and controls for the distinction of "cents" from pence, tents, . . .	

At the next level, these are particular kinds of words. "Four" is a name reduced from some act of counting. "Cents" is a primitive argument: one must say/hear an operator word asserting something of it or else one must reconstruct the zeroed presence of such a word plus a plausible justification for it being so redundant in context as to be zeroable. Such reductions reduce redundancy in language, but the more redundant, unreduced paraphrases are generally available as alternatives. In context, the speaker might intend something like "there are four cents on the table" and the speaker might reconstruct something like "I have four cents here" as the speaker's intent.

Also here is the differences between "four" and all other words of its type, and the differences between "cents" and all other words of its type. Thus "cent" is acceptable as first argument (subject) of "fall" but not of "eat" except perhaps in a fairy tale, a dream, a joke, or the like, whereas "rabbit" is acceptable (expectable) as first argument of either in a broader range of contexts.

This matter of acceptability or expectability can be understood in terms of word-dependencies that have been experienced in actually-occurring discourses in the past (others' one's own). These dependencies and associated reductions (when something is highly expectable it is reducible to more concise form, including zero) cluster by subject matter (and perhaps other categorizations of discourse type). So speaker and hearer are controlling for expectability with respect to word dependencies remembered from prior discourses.

Memories of other controlled perceptions are associated with those word dependencies. In this way, formal relationships among language entities can come into loose correspondence with relationships among things that matter to speakers and hearers in the world.

Now gee whiz, let me get back to work, I have some deadlines to meet!

[Translation, as if you needed it: I'd much rather do this all day, but it's not what I get paid for.]

Bruce Nevin
bn@ccb.bbn.com

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Date: Thu, 20 Jun 91 12:32:29 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject: channelling; Harnad

[From Joe Lubin (910620.1200)]

Bruce Nevin (910618.0900) --

> I'm no basis for any ad verecundiam arguments. Let some reviewer turn
> up my 1982 book on astrology and whiff! there goes my scientific
> credibility. My wife is a channel, and has been teaching psychic
> development for 15 years. I have those experiences to account for in
> any comprehensive model of perception and cognition. I can't just
> make them go away by the usual non-arguments. I know my wife to be a
> sincere and honest person, I have seen the changes in her students and
> clients, I have had conversations with Emmanuel (whom she channels) and
> have heard him lecture. So be careful. You might not want the CSG to
> be associated with me.

My wife is also. I echo this paragraph.
Will you be going to Durango?

Rick Marken (910619.2000) --

> Behavioral & Brain Sciences is out -- for me. I don't
> mind submitting when there is the possibility of rejection; but it's
> no fun when it's a certainty. If Harnad doesn't want to publish
> Powers, he certainly is not going to publish Marken.

My good friend, Stevan Harnad, is taking a bad rap from this group. He is one of the most honest and straighforward humans I have met. Now, I'm not getting defensive -- I think the BBS furor was pretty funny. But realize that the furor was only a positive feedback loop in this network. Stevan had very little to do with the reactions that ensued.

He has said that he is indeed interested in CSG and PCT, and that I am to keep him informed as I see fit. He also said that the book review choice back when was not his alone to make. Another thing he said was that typically the BBS associates need strong positive responses from at least two disparate fields to warrant publication. CSG work should have no trouble being multidisciplinary. If submitting to BBS, one might keep in mind to try to be EXPLICITLY multidisciplinary.

I will ask him if he is interested in joining the conversation. He is prolific.

In fact, Rick, upon receiving your permission I will send him this note as an invitation.

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Date: Thu, 20 Jun 91 12:36:44 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Converted from PROFS to RFC822 format by PUMP V2.2X
From: "Bill CUNNINGHAM - ATCD-GI (804" <CUNNINGB%MON1@LEAV-EMH.ARMY.MIL>
Subject: Four cents worth

:from Bill Cunningham:

Bruce Nevin (910620.1030)

I think that's more complicated than intended. With less detail, try

```
Sender                                          Receiver
----------------
Perceive 4 pennies
Perceive need to communicate same
Translate "4" into symbol "four"
Control for correct translation
Initiate muscular action (as you describe)
Sense change in acoustic intensity         Sense change in acoustic intensity
Control for acoustic sensationControl for acoustic sensation
Control for configurationBegin control for configuration
  Adjust muscular action                          Continue
  Continue control loop to complete "four"        Continue
Complete execution of "four"                       Percieve end of "four"
                                                       via intensity & sensation
                                                   Complete control of "four"
                                                   Translate  symbol  "four"  to
"4"
                                                   Control  for translation

Control for object of "4"          Control for object of "4"
                                                   Anticipate further input
                                                     (adjusts references)
```

Repeat loop for pennies

That's quick and dirty, but I think more oriented to principle than the mechanics. The result should be in the simple terms of HCT rather than the details. We could just as easily be drawing pictures of 4 pennies or

sending icons on computers.

Yes, earning a living does interfere with this!! Hi Ho Hi Ho again.l
==========
Date: Thu, 20 Jun 91 13:38:38 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Linguistics

Bill Powers (910619.1730):

Rather than Big Daddy trying to solve the communication problem, I'd like to encourage you linguists to put together your own control-theoretic analysis of how communication works.
==========
I have been working on this problem for several years. A few weeks ago I attempted to put together a short-form version for this group's comments, but I was unable to compress it sufficiently to make it seem to make sense in the compass of one (or two) mailings. The theoretical structure is, as Bill Powers suggests, very simple and self-similar, although it can lead to very complex results. My colleagues and I are trying to draft a book on the theory and its applications. We have a partially drafted set of HyperCard stacks as a tutorial, as well. These do not focus on the control-theoretic aspects.

In contradiction to Powers, the theoretical foundation is equally in control theory and in information theory. It is the theoretical impossibility of getting an error-free signal across a noisy channel in finite time, coupled with the instability of feedback loops with long delays, that leads to the layered structure of communication.

Also in contradiction to Powers' simplifying assumption, the requirement for feedback comes from the fact that almost certainly the talker and hearer do not have the same perceptual organization, and if they do, they don't know that they do. Each must model the other's organization, and improving the model is one of the primary functions of the communication process, since improved models lessen the bandwidth requirements for feedback, improving the system stability and increasing the effective communication bandwidth.

A simulation experiment to study the stability criteria has been planned, but the colleague who was developing it (with a M.A.Sc. in control theory) has left the project, and I don't know when or whether it will be pursued. My own engineering background is long enough ago that my control theory is limited to linear deterministic systems, and I don't know how to deal with two coupled non-linear stochastic control systems. (I know that in principle they are close to being deterministic, but there are bound to be enough unmodelled effects that a stochastic system will provide a better match to reality in the simulation).

If anyone wants to know more, an introduction to the ideas is is "The Structure of Multimodal Dialogue" (Eds Taylor, Neel and Bouwhuis, North Holland, 1989) Chapter 11 (by me).

Perhaps in light of Bill Cunningham's notions and Marken's and Powers' comments, I may try again to distill the concepts in the CSG language.

Rick Marken (910619) --

What you are talking about (speakers compensating for environmental distortions of the voice such as noise) is well studied by speech researchers. One of the names is the "Lombard effect." The compensation occurs, but does not maintain the signal to noise ratio, and is different in different talkers. I don't have references at hand, so I can't give details, but one of the major effects is to tilt the voice spectrum to emphasize the higher frequencies. The increase in power is much smaller than the increase in noise power, so SNR is much lower in high noise levels than in low. Your idea that speakers maintain the same perceived signal might possibly be salvaged, if their perception of their own speech becomes more and more to depend on bone conduction (which emphasizes low frequencies) than on air conduction. I think that this will happen, but it is unlikely to provide an exact match to the observed effects of noise, because there should be a fairly sharp crossover from primarily air conduction to primarily bone conduction at some predictable noise level, and spectral changes should therefore occur only around that noise level rather than continuously as the noise level increases. (And for all I know, the critical noise level may be below 0 dB, so not in the realm that can be studied).

Martin Taylor

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Date: Thu, 20 Jun 91 10:51:24 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: aphasia (really alexia)

[Rick Marken (910620)]

Martin Taylor sent me an interesting post which he said I could take to the net -- so here it is.

Martin says:

>You may have encountered the periphery of a serious issue with the reviewer's
>question about aphasia, and then again the scent may be of a red herring. I
>do wonder how PCT can handle some of the dissociations that occur in brain
>damaged people. Do they have pure output control without feedback, or is
>the feedback there but somehow inaccessible. I think, for example of
>alexia without agraphia, a condition in which a person cannot read, but can
>write perfectly coherently and legibly. Immediately after writing, the
>person cannot read what he wrote, although he may remember what it should
>have been, and another reader will confirm that it is correctly written.
>There are a number of such dissociations that one would consider "logically"
>impossible, but that occur in real people. On the face of it, they present
>a problem for PCT, but that problem may be more apparent than real.

Problems are what control theorists like. I think the conditions that result from strokes and other unfortunate causes of brain/neural damage can

provide very interesting suggestions about the nature of control. I think people with detailed knowledge of these conditions could be quite helpful. The example you give above is very interesting -- a person cannot read but they can write coherently. This general description sounds like it could be a problem for control theory if you imagine that a person must be able to read in order to write. But this depends on what "read" and "write" mean -- from a control perspective we would ask what variables can and cannot be controlled. Writing involves the control of many, hierarchically related perceptual variables -- and it looks like the alexic above can control them -- the writing is not open loop or it would not be consistent. For example, can the person write on different surfaces with very different friction coefficients?-- I bet he/she can, indicating control of the forces involved in writing. The person can probably write the same word quickly or slowly, indicating the ability to control the transitions and configurations involved in writing. Can the person say what he is writing -- probably not (alexia). This may be because there is a problem perceiving the meaning represented by the words (transforming the lower level perceptions of configurations and sequences into the perception of a program, category or such that is the "meaning" of the words or it could be a on output
disconnect that prevents the "meaning" levels from sending references down to lower levels so that the person cannot tell YOU what is mean by the words (rather like the split brain patient who cannot say what they saw but can point to it , if asked -- this may be something to try with alexics).

I think it could be very informative to turn a control theorist loose on a stroke patient (actually, my dad is a stroke victim but he has no obvious aphasias -- but he might be a good start for me).

The aphasia described by the reviewer was similar to the one you describe -- the patient cannot make phonemic discriminations and categorical perception is "impaired" but "phonemic serialization in production" is not impaired. I think this means that the person cannot report the order of two phonemic sounds, nor can they say reliably whether the sound is "la" or "ra" but they can still talk -- ie say words. Again, this looks to me like a pretty clear case of a loss of connection between levels -- there is no open loop control (I presume the reviewer imagined that saying "dog" without being able to say whether "d" or "g" came first is evidence that "dog" is produced open loop). I can imagine a hierarchy of control systems
which can produce the event "dog" by adjusting the appropriate lower order transitions for configurations of sensations and intensities. This same system may have lost the ability to perceive (or report) sequences (which are computeed at a higher level in the hierarchy than events and the rest). Thus, when a person is asked to report on the sequence of configurations that make up an event, he cannot do so because the sequence is not perceived; but the sequence systems can still send references to the event control systems, on of which is a reference for a certain level of "dog" event; and the system produces it.(By the way, the reviewers did not seem to understand that an event, which consists of a sequence of lower order perceptions over time. can be produced by a system that cannot pereive sequence. But this occurs all the time -- we can crete an event (like a word) without any idea of the order of the component sounds, other than that there is a first and last.)

Now that I think of it, these peculiar aphasias, where the ability to control certain variables remains intact while others are lost, are extremly strong evidence of HIERARCHICAL control and extremely poor evidence for programmed output type hierarchies. By looking at aphasic abilities properly -- ie. testing for control by introducing appropriate disturbances to hypothetical controlled variables -- we might be able to use the disconnects introduced by the stroke or whatever to map hierarchical relations between controlled variables.

Hasta Luego

****

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

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Date: Thu, 20 Jun 91 13:13:56 cdt
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "McClelland,Kent" <MCCLEL@GRIN1.BITNET>
Subject: Goldstein ms/thanks

[from Kent McClelland]

David Goldstein: I'd like a copy of your manuscript on PCT Psychotherapy.

Rick Marken, Bill Powers: Thanks for sending the spreadsheet models and Demo1 and Demo2. Very interesting stuff, though more than I've been able to assimilate yet. I think some of this may be useful in a course next year, as well as just helping me get a better feel for how control systems work.

By the way, I recently got ahold of a copy of Philip Runkel's book, Casting Nets and Testing Specimens (NY: Praeger, 1990), and found I couldn't put it down. Excellent book!

(my apologies if this duplicates an earlier message--had transmission trouble)

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Date: Thu, 20 Jun 91 14:27:56 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: alternate channel

[From Bruce Nevin]

Joe Lubin, I see we could have some interesting conversations indeed, but alas I see no way of getting to Durango with mutually conflicting family, work, and academic commitments all firmly alleging themselves full time.

I was quite astonished as were many by Harnad's prolificness some might say prolixity or even profligacy but I think the first is right on the AIList last year re "symbolic grounding". I printed all that and have yet to wade through it all, doubt I will now. Glad to hear your report, my reaction to Rick's lament was what evidence of rejection being a sure thing have you other than the "let's flood him with email" fiasco not so long ago, hence my earlier question what did we think he would be controlling for in all that, after all?

Be well,
Bruce
bn@ccb.bbn.com

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Date: Thu, 20 Jun 91 14:38:05 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: 4 pennies "4" and "four"

How do you get from the perception of 4 pennies to "4", what is the status of this "4", how do you translate it into `the symbol "four"' that is among other things in what sense is it a translation and does that imply that "4" is in one sort of language and "four" in another? What does `object of "4"' mean and in what way is the speaker going to control for it i.e. presumably for perception of it?

Bruce
bn@ccb.bbn.com

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Date: Thu, 20 Jun 91 14:12:52 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Expectability and Competition

[from Joel Judd]

Bruce (910619) said

>This matter of acceptability or expectability can be understood in terms
>of word-dependencies that have been experienced in actually-occurring
>discourses in the past (others' one's own). These dependencies and
>associated reductions (when something is highly expectable it is
>reducible to more concise form, including zero) cluster by subject
>matter (and perhaps other categorizations of discourse type). So
>speaker and hearer are controlling for expectability with respect to
>word dependencies remembered from prior discourses.

Have you read any of Brian MacWhinney's/Elizabeth Bates work with the Competition Model? The above paragraph sounds like competition. A good summary is in _Mechanisms of Language Acquisition_ (1987?).

If you want, when I get back from El Paso I'll dredge up the language hierarchy outline I quickly drew up last fall as a heuristic when I was

trying to visualize a CSH for language, and we can pass it among interested folk. It's part of what I would like to present at Durango and is also part of my dissertation (...awaited with baited breath...).

Joel Judd

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Date: Thu, 20 Jun 91 13:55:08 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Misc replies

[From Rick Marken (910620B)]

Wow, lots of comments on various topics. There is a plausible lull at work so I'll try to handle them quickly:

Joe Lubin (910620.1200) and Bruce Nevin (910620) both commented on my snide remark about B&BS rejecting my paper.

Joe says:

>My good friend, Stevan Harnad, is taking a bad rap from this group.
>He is one of the most honest and straighforward humans I have met. Now, I'm
>not getting defensive -- I think the BBS furor was pretty funny. But realize
>that the furor was only a positive feedback loop in this network. Stevan had
>very little to do with the reactions that ensued.

>He has said that he is indeed interested in CSG and PCT, and that I am to keep
>him informed as I see fit. He also said that the book review choice back when
>was not his alone to make. Another thing he said was that typically the BBS
>associates need strong positive responses from at least two disparate fields
>to warrant publication. CSG work should have no trouble being
>multidisciplinary. If submitting to BBS, one might keep in mind to try to
>be EXPLICITLY multidisciplinary.

Ah, now I remember. The BBS "furor" was the result of my suggestion that CSGNet people send email to Harnad indicating their interest in having Bill's book reviewed. Harnad thought it was some kind of ballot box stuffing. I didn't know I was doing anything wrong but I certainly could understand how Harnad might feel about this. I'm not very politically astute -- I just thought it might help if the BBS editors knew there was some interest in having the book reviewed. I guess Harnad got upset because getting a book chosen for review could produce increased sales so maybe he thought it was an unfair marketing type scheme. Such a thought never entered my mind since I'm quite sure that Bill's works are not going to be topping the best seller lists soon. I just wanted to see what others would say about the book. So, if you see Harnad, tell him I don't feel bad about his concern about the ballot stuff nor about the fact that "living control systems" was not chosen for BBS review. I never felt bad because of what Harnad said -- I thought of it as an informative diagnostic. No furor intended.

The reason for my comments about "certain rejection" by BBS stems from my experience with BBS's rejection of an invited paper by Bill Powers and of an uninvited paper by me. Harnad may be honest and nice and interested in control theory but he certainly is more interested in seeing his perception of it appear in BBS than in having Bill's or mine. I personally watched, about 6 years ago, as Bill wrote and rewrote an article for BBS that was eventually rejected by Harnad (personally, I believe) because Bill said things in the paper about how control systems work that Harnad felt were not true. So we had the student telling the teacher what to teach. I found it particularly disappointing because I really wanted to see what the behavioral science community would have to say about Bill's work (with Bill's counter-replies) and also because I read the papers Bill wrote and kept perceiving this enormous QUALITY signal and then seeing stuff in BBS that produced no quality signal at all.

I could care less about BBS rejecting my paper (they completely missed the point but that's no news). But I did get some enormous quality perceptions from that paper as well. If Harnad is really interested in publishing a control
theory target paper in BBS he is obviously waiting for someone who can do it better than Bill or I. So who is it going to be. My guess is someone who understands control theory as Harnad does. And why not -- he's just controlling his perceptions of what should go into his journal (and what others might want to see) -- and control theory (at least the annoying version of it that Bill describes) is a damned disturbance; not in the sense that it threatens any of his preconceptions, but in the sense that it must seem so obviously wrong. I know that my "Hierarchical behavior of perception" paper will be rejected, because the more I make it sound RIGHT to me, the more I know it will sound WRONG to Harnad.

You can't force people to reorganize (and remain a nice person yourself). It looks to me that readers of BBS are quite content to carry on their "debates" in the context of the prevailing zeitgeist; and why not? BBS itself is not going to change -- it's a hell of a successful journal, as far as I can see, so, obviously, their editorial policy is "right". Publishing Bill's or my paper would very likely lead to the journal being perceived as going "down hill"; and that's loss of subscribers. So if I were Harnad, I wouldn't publish me either.

>In fact, Rick, upon receiving your permission I will send him this note as an >invitation.

Feel free to send him your notes and mine. If you do, I want to make it clear that I don't have any bad feelings toward Harnad -- I think he might, indeed, be a mensch. I just think his system concepts conflict with ours; that's not the sign of a bad person (on either side); just two control systems maintaining different system concepts.

Martin Taylor(910620) says:

>What you are talking about (speakers compensating for environmental >distortions of the voice such as noise) is well studied by speech >researchers. One of the names is the "Lombard effect." The compensation >occurs, but does not maintain the signal to noise ratio, and is

>different in different talkers.

I know about this research -- I used S/N ratio because it is simple. The "Lombard" effect suggests that S/N may not be a controlled variable -- or that they are not getting a proper measure of the actual controlled variable
which, as you (or Nevin?) note is influenced by bone conduction, among probably other external disturbances -- remember, the controlled variable is really an afferent neural signal. We try to get a handle on it by measuring physical variables that might be correlates of that signal.

>I don't have references at hand, so
>I can't give details, but one of the major effects is to tilt the
>voice spectrum to emphasize the higher frequencies. The increase in
>power is much smaller than the increase in noise power, so SNR is
>much lower in high noise levels than in low.

Good, getting closer to an idea of what variable(s) might actually be controlled. Obviously, S/N is too simple. We know that our initial guesses are likely to be wrong. We hypothesize, test, and alter the hypothesis bases on the test -- trying to hone in on the controlled variables. A good example of the process is described in Bill's 1971 Behavioral Science "Rat" paper -- also in his book and in my "Behavior in the first degree" paper (last demo).

>   Your idea that speakers
>maintain the same perceived signal might possibly be salvaged,

Control theorists are happy to "scap" hypotheses about controlled variables. We just know that we are looking for controlled variables -- we don't know what they are. I don't need the S/N theory salvaged; I need suggestions about what variable to test next -- and how. Conventional psychologists are looking for the stimuli that cause behavior (Noise causes shouting, for example) -- this simple model, I suppose, makes it more crucial for the researcher to find THE stimuli that actually cause THE behavior. They are just look under a whole differnt lamp post.

> if their
>perception of their own speech becomes more and more to depend on bone
>conduction (which emphasizes low frequencies) than on air conduction.
>I think that this will happen, but it is unlikely to provide an exact
>match to the observed effects of noise, because there should be a fairly
>sharp crossover from primarily air conduction to primarily bone
>conduction at some predictable noise level, and spectral changes should
>therefore occur only around that noise level rather than continuously
>as the noise level increases. (And for all I know, the critical noise
>level may be below 0 dB, so not in the realm that can be studied).

Good research suggestions. I just want to make it clear that I have NO IDEA what variables people are controlling when they speak. I am sure it is many (simulataneously) and that it is not easy to find out what they are. Please don't imagine that I have any committment to finding that people control a PARTICULAR variable (like S/N ratio). Finding out what the variables are is what control theory research is all about.

Regards

Rick

****

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

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Date: Thu, 20 Jun 91 16:49:19 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Converted from PROFS to RFC822 format by PUMP V2.2X
From: "Bill CUNNINGHAM - ATCD-GI (804" <CUNNINGB%MON1@LEAV-EMH.ARMY.MIL>
Subject: more on 4

:Bill Cunningham:

Bruce Neven(910620.1438)

As I said, "quick and dirty".

There are two ideas to transmit: quantity and what. I chose to transmit in that order--quantity then what. That was certainly arbitray and maybe short sighted, as I will explain subsequently.

I perceive 4 pennies, but that does not mean I have labels and numbers pinned down. In fact, I may not until I have a reason to be precise. My perception may be "a bunch of copper disks, between 3 and 5". This gets into the "can you think without words" question which I'm trying to acknowledge without getting mired in. Anyway, I've got to transmit a recognizable symbol, so I translate the "4" into "four". I might have chosen "vier", but our problem is stated in terms of common organization, presumably in English.

Some bunches of things have collective names. "Copperglut" (one word) might be a single symbol meaning "exactly 4 pennies", or it might mean "something over 3 pennies".After all, two bits is a quarter.

On translation and symbols:
I have to choose a symbol because I can't tranmit the hunks of copper for the other person to look at. Presuably the symbol is chosen from an available reference set. Since this is a control problem, I choose the symbol with the least error between what I perceive and the symbol I've chosen. To me, that implies a control loop prior to initiating the muscular activity. Hence, control for translation. Receiver has same problem.

On the object of "4":
I borrowed this term from you, perhaps incorrectly. Somehow I have to

convey "4 somethings". By object of "4", I meant the pennies. I avoided the homophones (right word?) "for" and "fore", but each of these begs a question that the sender has to answer. Consider the receiver anticipating the second word which could be "cents", "ever" or "play". That implies some sort of control loop within the sender to make sure the two words, originally to be sent separately, convey the right meaning when taken together.That's a complication that I'm deliberately avoiding.

Martin Taylor(010620.1338):

Tell us more. Please!! If we can't arrive at answer consistent with both control theory and information theory, we will have major explaining to do.

It would be incredible to have two identically organized individuals. My original comments about interpersonal feedback (calibration, "getting to know you", and eventhe business of establishing a common level) were really based on the point that individuals are not identically organized and would have to find/create organizational subsets common to both. For now, I'm happy to assume they're idendical. If we can figure out how to deal with $.04 (electronic fund transfer?), we can build up the rest. On reflection, we have already lied. The sender perceives the 4 pennies and the receiver does not (yet). If the sender and receiver are successful, the perceptionwill be transferred and they will become identical again. Before somebody asks, by identical I mean common reference sets and weighting. Right now the sender is 4 pennies ahead and we haven't talked about geometric pattern or anything else.

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Date: Thu, 20 Jun 91 17:37:44 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: more on 4

Bill Cunningham asks me to expand on my brief reference to my ideas on interpersonal communication. I'll try, but as I said in my original note, I did try earlier, and couldn't reduce it to a reasonable size. Perhaps I will do as some others have done, and send something to Gary for redistribution to interested people. But don't hold your breath, because doing this will have to compete with many other things I must do in this area. Meanwhile, if people want reprints of the chapter I mentioned, I do have a few copies, so I would respond to a few requests that provide Smail addresses.

Martin Taylor

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Date: Thu, 20 Jun 91 23:43:21 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Communication

[From Bill Powers (910620.2300)]

Try it this way. Examine what you imagine as this communication unfolds in slow motion, line by line, remembering that the meaning of words and

word-structures is (according to my hypothesis) an imagined perceptual experience that is not a word or word-structure.

In front

of me

on the table

are three

coins.

Call them Large, Small, and Medium.

Large is above Medium

and a little to the left of it.

Small is to the right of Medium

and a little above it.

What am I seeing?

Answer: coins arranged in an approximate right triangle tilted a little to the left, with the largest coin at the top, the medium-sized one at the right angle and the smallest one, to the right, at the remaining vertex.

(This is a second description, purportedly a paraphrase, of what the listener assumes to be the same image).

The first description relies primarily on relationship-terms with categories used mainly to provide naming-distinctions. The second introduces a category statement (right triangle) which helps to organize the relationship-statements in a form more easily grasped (the specific relationship statements are required to resolve ambiguities included in the category "right triangle," although not all ambiguities and unwarranted assumptions are removed -- actually the coins are all the same size, but they are called "Small," "Medium," and "Large" under advice from Lewis Carrol, which is of course not his name but only what he is called).

In this case, the listener can draw a picture of the transmitted image and show it to the speaker, so the speaker can say "Yes, that is what I meant," or "No, that is not what I meant." The relationship between the picture and the speaker's image is different from the relationship between the speaker's words and the image (on either end).

Pretty Please, discuss without using the words "semantics," "formal", or "information." Use the HCT model, if possible, or invent ad-hoc levels, if necessary.

Leading question: how does the SPEAKER know that these words describe what he is seeing? This is the same as asking how the LISTENER can draw a picture of the meaning as "understood."

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Date: Fri, 21 Jun 91 11:07:14 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: 3 cents

----BEGINNING OF FORWARDED MESSAGES----
Received: from BBN.COM by CCB.BBN.COM ; 21 Jun 91 10:39:00 EDT
Received: from uxc.cso.uiuc.edu by BBN.COM id aa09750; 21 Jun 91 10:35 EDT
Received: by uxc.cso.uiuc.edu id AA16816
(5.65c/IDA-1.4.4 for <bnevin@ccb.bbn.com>); Fri, 21 Jun 1991 09:34:21 -0500
Date: Fri, 21 Jun 1991 09:34:21 -0500
From: Mail Delivery Subsystem <MAILER-DAEMON@uxc.cso.uiuc.edu>
Message-Id: <199106211434.AA16816@uxc.cso.uiuc.edu>
To: bnevin@ccb.bbn.com
Subject: Returned mail: Host unknown

----- Transcript of session follows -----
550 vmd.csu.uiuc.edu (TCP)... 550 Host unknown
554 <csg-l@vmd.csu.uiuc.edu>... 550 Host unknown (Authoritative answer from name
server)

----- Unsent message follows -----
Received: from ux1.cso.uiuc.edu by uxc.cso.uiuc.edu with SMTP id AA16814
(5.65c/IDA-1.4.4 for <csg-l@vmd.csu.uiuc.edu>); Fri, 21 Jun 1991 09:34:21 -0500
Received: from BBN.COM by ux1.cso.uiuc.edu with SMTP id AA11846
(5.65c/IDA-1.4.4 for <@uiuc.edu:csg-l@vmd.csu.uiuc.edu>); Fri, 21 Jun 1991 09:34:15 -0500
Message-Id: <199106211434.AA11846@ux1.cso.uiuc.edu>
Received: from CCB.BBN.COM by BBN.COM id aa09397; 21 Jun 91 10:30 EDT
Date: Fri, 21 Jun 91 10:26:06 EDT
From: "Bruce E. Nevin" <bnevin@ccb.bbn.com>
Subject: OK, 3 cents
To: csg-l@vmd.csu.uiuc.edu
Cc: bn@ccb.bbn.com

As we sit, Bill, you in front of your terminal and I in front of mine, in the contratemporal space of email, we replicate your proposed communication scenario. You have your arrangment of coins called "how communication works in HCT" and you are asking me (among others) to describe what I think that arrangement is.

To be sure, you are asking me to describe how communication using language works in a simplified version of our situation, but you are

controlling for a perceived match between what I say (in the role of the drawing passed over the barrier) and your already held understanding of the process (in the role of the arrangment of coins).

I have no objection to your removing the one occurance of the word "formal" from my first cut at this. I did not use "semantics" or "information" and my use of the word "formal" is only by way of emphasizing that the latter two words are not needed.

The rest of what I wrote is at a level much lower than your proposed line-by-line scenario, which begs many questions about the relationship between space-separated strings of ASCII characters printed on our respective screens and acoustic vibrations in a complex wave which fourier analysis with a sound spectrograph shows to have different intensity at different frequencies, although the latter is the point at which you asked us to start.

Your description of that scenario refers to "relationship-terms with categories used mainly to provide naming-distinctions." In this, you are relying on our shared understandings with language to avoid talking about what relationship-terms, categories, and naming-distinctions might be and how they mignt be established and maintained in a control system. I sketched exactly that missing link. I ask you to please examine what I wrote again with that in mind.

My knowledge of HCT is imperfect and crucially uninformed by modelling experience or even experience of demonstration models, and I am here to learn. However I do believe what I wrote constitutes a satisfactory if sketchy indication of how perceptual control on very low levels may be built up through successive levels of control to account for language and communication (two different though related things).

I will not have time to tackle your communication scenario as seriously as it needs to be until this weekend, if then. I will try, I promise.

Talking about what I submitted previously: there is a constitutive relationship between entities in language, some of which involve changes of level of control and some of which are "horizontal" in the manner of chair arms, legs, seat vs the chair as a whole. I tried to show how with minimal means things like relationship terms, categories, and naming distinctions can come to be in language, where at the level of controlling direct sensory perception there are only complex sound waves with continuously varying intensities at continuously varying frequencies, plus (for the speaker) some control of tactile and kinesthetic perceptions perhaps, when there are acoustic limitations.

With written text, letters constitute a word and except for construal of misspellings and disambiguation of homographs that's that. With spoken language there is sometimes astonishing range and flexibility in the precise sound features available for a given word, and in the range of articulations used to produce them of course. The reason is that speakers and hearers are not controlling for approximation to a target sound (a discrimination task) but for contrast with other possible sounds within the language. The elements are not sound features or articulatory gestures or target positions, but rather contrasts. You

can analytically represent the contrasts by letters, by names of acoustic and/or articulatory features (defined with suitable flexibility), by time-flow diagrams showing relative timing of articulatory gestures and/or acoustic features, by the plurally hierarchical diagrams of autosegmental phonology, etc. I have this hunch that all of that has only to do with the representational convenience of linguists trying to describe behavior rather than control. Speakers control for the distinctness of words and parts of words (stems, affixes) from one another. Hence my skipping over the hoof-torn turf of phonological representations.

With the caveat that alphabetic representations of written language presuppose a resolution of these issues that is surely wrong, let's start from written words, then, assuming that word and parts of words are distinct from one another in precisely the same ways as in speech (a false assumption).

The simplest account, I believe, is that speakers and listeners control for expectability of dependencies among these elements (words and parts of words). This is the account that I began to sketch. Speakers and listeners control for a match between one of several alternative possible dependency relations in the recognized sequence and a dependency relationship found in (usually many) previously understood discourses. Establishing such a match for some sentences of the current discourse reduces the search space (I here risk introducing the much abused computational metaphor) for subsequent ones, unless the subject is changed. To say "kick" you must also say two other words like "boy" "dog" or "stone" or else your saying it must be so expectable in context that you needn't say it ("What did he do?" "Kicked the dog.") or may say some other form instead ("he" in the example).

The precise form of such reductions and to some extent the occasion for them are matters of social convention: arbitrary, subject to change, subject to variation that may mark the speaker in terms of provenience, social class, personality traits, etc. Sometimes the speaker and hearer control consciously for these perceptions, usually, the control is unconscious (especially in the famous first 60 seconds or whatever of first impressions).

In looking for a match of dependencies, we may understand by analogy. If a particular A-B word dependency is not remembered for the type of discourse (subject matter, usually), we look for a match in other adjacent types of discourse (adjacency defined in terms of vocabulary and intersecting sets of word dependencies). Alternatively, we may use classifier words, the most well-understood of which are predicates of what is commonly called an ISA relationship in AI parlance (a collie is a dog, a dog is an animal). These two are related: each domain (discourse type, subject matter) has classifier vocabulary that is characteristic for it, and which is used to characterize the domain in its metalanguage. (The metalanguage, crucially, is necessarily included in the language.) Adjacency of domains can be understood in terms of shared classifiers, or subsumption of certain classifier words in each under higher-level classifier vocabulary. These classifier words correspond to perceptions of relationships that speakers of the language control.

Looking at your configuration of coins again, I mentioned that numeral names are associated with remembered acts of counting. For the smaller numbers are remembered also common geometrical configurations, such that a number is recognized without the act of counting (which had been done previously for the shape). But in general I believe there are words in our language for perceptions that we ordinarily can control consciously, and conversely it is difficult to bring to consciousness control of perceptions for which we have no words, though we can and do make up and adapt vocabulary as we do so.

I've done it again. We've got to stop meeting this way. My boss will get jealous.

Bruce Nevin
bn@ccb.bbn.com

PS:

I would like to get some demos to run on my PC, but I don't have:

joy-stick or mouse
spreadsheet program (well, a friend has a pirated copy of Lucid and I might be able to borrow 123 from work)

----END OF FORWARDED MESSAGES----

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Date: Fri, 21 Jun 91 11:40:08 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: up a level on a parallel track

On a level more usually addressed by the word "communication," we should note that the same words with the same physical referents may have quite different referents (may be associated with quite different controlled perceptions) concerning social relationships. An excellent example is in the following excerpted from some of this morning's mail from another source, quoting Deborah Tannen's bestselling _You Just Don't Understand_, which discusses gender differences in expectations for communication.

>It's more complicated than "he's logical" or "she's emotional"; in
>many cases, the two sides are using the conversation for different
>purposes. For instance, the author points out that males often
>enjoy challenging one another, and that challenging another speaker's
>facts is taken as a normal part of the conversation. By contrast,
>females often treat challenges as a personal attack. What winds up
>happening is that both partners wind up angry and confused.
>("Why's she getting so upset about a simple debate?" "Why can't
>he ever believe what I say?")

>The most widely-quoted example in the book is the following:

>[man is driving car; woman is passenger. An ice cream stand looms up.]

>She: "Would you like to stop for some ice cream?"
>He: "No, thanks." [continues driving.]

>[One hour later:]
>He: "Why are you so angry?"
>She: "Why didn't you stop for ice cream when I asked you to?"
>He: "But you DIDN'T ask me to; you asked if I wanted ice cream, and I don't."
>She: "You could have asked if I wanted any."

>And the fight is under way.

>What's going on here is that females often phrase requests as questions
>about the other's needs. She was expecting an answer like "No, but would
>you like to stop?", to which she could reply "Yes, please." He thought
>the question was whether *he* wanted ice cream; he assumed that a request
>to stop would sound like "Could you stop at this ice-cream stand?"

        Bruce Nevin
        bn@ccb.bbn.com
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Date: Fri, 21 Jun 91 12:02:16 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Communication

Bill Powers (910620.2300)--
Leading question: how does the SPEAKER know that these words describe
what he is seeing? This is the same as asking how the LISTENER can draw a
picture of the meaning as "understood."
==========
I agree that the leading question is interesting and important, but it is
not the same as the gloss. For communication, as opposed to talking to
oneself, the Speaker must in some way model what the Listener will do with
the speech, and this is not necessarily (nor even usually) what the
Speaker would do with the same words were they spoken TO him. The question
Bill poses has a presupposition that "words describe what he is seeing."
In themselves they don't. They do only insofar as they can evoke an
intended effect in the hearer (who may be the Speaker) in the situational
context that the Speaker believes the Hearer to experience. Bill and others
have correctly identified that there are different levels of abstraction
in communication, and it has been mentioned that the reconstruction of
intention of a communication depends on what is already known to the
hearer. Words, therefore, cannot "mean" anything out of the context in
which they are used, and furthermore, someone who is not party to a
conversation cannot be sure what the words "mean" to those who are
participating in the dialogue.

Bill asks the listener to show the speaker a drawing of the coin
configuration as proof of understanding. I think this is the same as
Gordon Pask's "agreement over an understanding," which is demonstrated by
giving feedback that is not an echo of the original message, nor a paraphrase
(level 2) but something that both agree represents the original intent

but is derived through a model different from that used for the original message (level 3). According to Pask, it is the most reliable way (he says the only reliable way, but I would dispute "reliable") for the parties to be sure that the concept has been correctly transmitted.

I realize that this is not cast in control-theoretic terms, but I don't know how to do that when we are dealing with coupled control systems; and it is especially difficult in light of the recent discussion on the role of imagination, which figures prominently in any discussion of communication between intelligent partners.

Martin Taylor

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Date: Fri, 21 Jun 91 12:32:07 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: 3 cents

Bruce Nevin (910621 and an earlier message) points out:

> Speakers control for the distinctness of words and parts of words (stems, affixes) from one another.

==========

This is a crucial point about communication, and a primary reason why I don't like Bill Powers' wish to keep "information" out of the discussion. Not only do speakers control for the distinctness of words, they control for distinctness at all levels. For the most part, communication is the attempt to select in the mind of the partner a satisfactory configuration out of all the configurations that the partner's mind can produce. The control is of the partner's behaviour in response to the communication. Selection is a matter of probability. If the partner strongly expects a particular configuration to be selected, a hint is all that is required. If the partner has a low probability for the configuration you want, you must supply a lot of information (in the Shannon sense as well as the everyday sense). That's what this list is about. If everyone had Powers' deep intuitive feeling for how CT works, he wouldn't need to write so much, but since we don't, he has to enable us to converge slowly on his understanding
(and perhaps modify his understanding in the process). If a talker provides excessively detailed specification, the hearer may misinterpret. If he provides too little specification, the hearer may be unable to interpret.

People really do take this into account in describing situations such as Powers' coin example (see work by David Olsen). They usually wouldn't say something like "The top coin" if all the objects are coins and the listener knows it. They would say "the top one" instead. At a lower level, the first time a new content word is introduced in a description, it tends to be articulated more clearly than on subsequent uses. At a higher level, Grice's maxim of quantity comes into play (something like "say no more than you need.").

What is being controlled for is the listener's discrimination among possibilities perceived by the talker as available. That goes at all levels of abstraction.

Martin Taylor
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Date: Fri, 21 Jun 91 13:02:52 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: reply to Bill

[From Bruce Nevin]

Bill Cunningham, you wrote:

>There are two ideas to transmit: quantity and what.

>On the object of "4":
>I borrowed this term from you, perhaps incorrectly. Somehow I have to
>convey "4 somethings". By object of "4", I meant the pennies. . . .
> Anyway, I've got to transmit a recognizable symbol, so
>I translate the "4" into "four".

>I have to choose a symbol because I can't tranmit the hunks of copper for
>the other person to look at. Presuably the symbol is chosen from an
>available reference set. Since this is a control problem, I choose the
>symbol with the least error between what I perceive and the symbol I've
>chosen. To me, that implies a control loop prior to initiating the
>muscular activity. Hence, control for translation. Receiver has same
>problem.

Bill, I don't think the following theoretical entities are needed for this task:

idea, quantity, what, symbol, somethings (as in "4 somethings"), translation, transmit

More exactly, the way that I'm trying to describe use of language in HCT terms doesn't require them. I can't relate them to the task or to what I'm doing unless you define them in HCT terms. If there are any primitive terms in my account for which the HCT definitions are not apparent, please tell me and I will try to meet the same standard.

> After all, two bits is a quarter.

This raises another issue about language. "Two bits" used to mean two coins each worth 1/8 dollar, hence the terms two bits for a quarter and four bits for a half dollar. The phrase now cannot be understood analytically since it does not refer to two of anything but rather one coin, a quarter. Every language is filled with frozen expressions that used to mean the literal, analytical meaning of the words in construction but now mean something different. "Take the bull by the horns" is a frozen expression that is apparently of great antiquity, since it is found in word-for-word correspondence in nine or ten different indo-european languages (intimations of oath rites in Mithraism, or visions of the bull-dancers of Minoan Crete?).

This has bearing on the issue of sublanguages, the subject-matter

specializations that together make up a language. Words that constitute a phrase with analytical meaning in one sublanguage may be a frozen expression functioning as a single word in another. In the sublanguage of pharamacology the phrase "the beating of the heart" is a single "word" of the symptom class, as in the sentence "digitalis affects the beating of the heart." But in the sublanguage of physiology (which is in fact the structurally superordinate sublanguage of a logically prior science) this phrase is analyzed into several words that belong to several word classes (each associated with a separate classifier word in the sublanguage). It would seem that a phrase may be borrowed from one sublanguage into another, where it is cut off from the detailed control of perceptions with which it was associated, and constitutes an unanalyzable frozen expression associated with control of different perceptions in the new domain. The basis is either inclusion of controlled perceptions in both domains, as in the pharmacology example, or analogical extension of control from one domain to another.

Turning back to the more familiar case of expressions like "kick the bucket," or "three sheets to the wind" (meaning "drunk") it appears that these are like borrowings where the original context and sublanguage analyticity are forgotten. The original context for "kick the bucket" is unknown, but "three sheets to the wind" was borrowed from nautical jargon in the days of sailing ships, where the analogy to a staggering vessel was plain.

Thus the borrowing may be for explication where two sublanguage domains intersect, as in the pharmacology example, or it may be for analogic comparison as for drunkenness, or it may be to extend words and a relationship from a familiar domain to a less familiar one, as I suggested in a previous post. This is an important way for us to apply our control of perceptions in some situations to perceptions in new situations that are not so familiar.

I'm not doing well at shutting up and getting back to work, am I?

Lunch is over!

Bruce Nevin
ccb.bbn.com

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Date: Fri, 21 Jun 91 13:08:41 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: reply to Martin Taylor

What do you mean by "information," since you want to include it?
If you just mean probability, or expectability, why not use those terms? I can see how an expectation could be modelled in HCT terms. I can agree that probability is a way of talking about the basis a machine might have for setting expectations. (I don't think people calculate probabilities and from them set expectations.) But I can't see any way to model information. Other than Shannon's probability-based measure, I can't guess what you might mean.

Bruce Nevin
bn@ccb.bbn.com

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Date: Fri, 21 Jun 91 14:33:04 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: reply to Martin Taylor

Bruce Nevin (910621)--

>What do you mean by "information," since you want to include it?
>If you just mean probability, or expectability, why not use those
>terms? I can see how an expectation could be modelled in HCT terms.
>I can agree that probability is a way of talking about the basis
>a machine might have for setting expectations. (I don't think
>people calculate probabilities and from them set expectations.)
>But I can't see any way to model information. Other than Shannon's
>probability-based measure, I can't guess what you might mean.
>

I do mean Shannon information, but people often make the claim that Shannon information cannot be reconciled with the everyday use of the term "information". I think it can, through the use of the layered passing of messages at different levels of abstraction, since the messages can be viewed as modifying prior probability distributions in the recipient of the message. (Note for Bill Powers: I do not argue that the mechanism for determining or for changing the probability distributions is any kind of mathematical computation; your neural frequency averaging works quite well as a recency-weighted averaging computer, and analogous methods presumably perform the needed shifts of probability distributions).

Shannon information is the basic measure, but it is only a unidimensional measure, and cannot distinguish among different probability distributions that have the same entropy. It is not the same as probability, and definitely not the same as "expectability", whatever that might be. It is a structural measure, and we are talking here about the structure of messages and models.

Martin Taylor

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Date: Fri, 21 Jun 91 13:55:08 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: query repeated

What the heck, it's Friday. And I have got a lot done for BBN today despite all.

One of Kafka's parables has our protagonist walking toward a gateway flanked by two huge guards.

"May I pass?"

Silence.
"Uh, excuse me--may I pass through here?"
Silence.
"Does your silence indicate permission to pass?"

A while back I proposed sending a diskette of email and a descriptive letter to Brain/Mind Bulletin in response to a query from them about exciting research and clinical trends that are particularly significant in terms of social or research breakthroughs. I asked if there were any objections to my doing this. I will do so if I see no objections in my mail on Monday morning when I come in to work again.

Bruce

(PS--personal favorite from Kafka:

> If it had been possible to build the Tower of Babel without having to climb it, the work would have been allowed.

This is partly in acknowledgement of the Charles Dodgson reference reference reference, Bill.
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Date: Fri, 21 Jun 91 22:44:32 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Linguistics

[From Bill Powers (910621.2300)]

Bruce Nevin, Martin Taylor, Joel Judd, Bill Cunningham, have I left anyone out? --

I'd better try to explain what I'm trying to do, or get done, concerning my responses to all the posts on linguistics (most of which are beyond my ken). First, of course, is an attempt to propose some CT-type hypotheses about communication. Second, and maybe more important, is to find a level of discourse at which we beg the right questions.

If we propose that people "just talk," that begs all the interesting questions and is at too general a level. If we describe in detail what each muscle is doing during articulation, we still beg the question of just how those particular muscle tensions are created, but it's clear that we are now trying too hard to analyze the details. Somewhere between these extremes is a level of description at which we can get a picture of the organization of the system without either making the picture too fuzzy or overwhelming ourselves with details by using too high a power in our microscope.

Bruce, the analysis you presented on 910620 got somewhere near the level I was talking about, although it tended toward excess detail. Because of the detail (which I'm sure was correct), this picture did not show the dynamic relationships of control clearly. I think we want to widen the

field of view somewhat and let a few of the details disappear; if the picture becomes more organized when we do that, we can always go back to the more detailed level to see HOW the broader conceptions of function work.

For example in describing what follows the "f" of "four", you say

>arranges tongue so that it is "lumped" toward the back of the oral
>cavity, with the tip lowered, body raised higher than for "far" or
>"fought" but not so high as for "foe", "foot", or "food"

This is the output part of the loop, but we can convert it to a description of controlled input rather easily, if less certainly:

"creates the combination of sensations that is the felt transition from 'f' to the 'ou' configuration" (pardon me if I don't use proper notation here -- I don't know it.)

This skips over the exact articulatory configurations and movements involved and focusses on the perception that is created. If a properly-working control system is present, it will VARY the articulations so as to create exactly that sound that the speaker intends to hear. We don't need to worry too much about what those articulations are -- particularly if we contemplate doing experiments in which we interfere with the mouth cavity so that different articulations are needed to produce the SAME sound.

We could now pause and sketch in the control system(s) responsible. A set of reference signals for the feel of "f" is received by a comparator (but this is really a set of control systems). Instantly the muscles alter the configuration of the mouth so that this feel is present to the intended degree. The sensations are brought to the reference level in a matter of 100 milliseconds or less (the neural paths are short here; it could be under 50 milliseconds). Then the set of reference signals changes so that this "f" feel changes and becomes the "ou" feel. The physical properties of the mouth and its muscles make this a continuous change rather than an instantaneous jump. A similar consideration is involved when changing from the feel of one vowel to another.

Note that this system does not hear anything: it feels.

With the air supply suitably being varied and the vocal cords turned on at the right moment (by other feel-controlling systems we aren't considering right now), control of these feels results in hearing sounds: first the fricative, then the vowel sound. As these sounds begin to form, the next level of system, which is receiving a reference signal for each intended sound in turn, detects the error between the sound as heard (initially no sound) and the intended sound, and the error drives very rapid adjustments of the reference signals for the feel of making the sounds. The sounds are made to follow the varying reference signals with a slightly longer lag than there is in the feel-control systems. The means of adjusting the heard sounds as they are occurring is a variation in the reference signals supplied to the feel systems. The minimum lag of this control process is set in part by mechanical limitations of the feel systems, and in part by stability requirements.

The changing reference signals for the sounds are generated by a system which is monitoring a sound-event. This system receives copies of the sound-signals being controlled by the next lower level of system, and reports them as a signal representing a specific event in progress, the event we hear as the pronunciation of "four" or "fore" or "phore." This system responds to ALL ongoing sound-events in terms of the degree to which they sound like "four." A different system would respond to the sound-event "soar," and still a different one to "more." The magnitude of the perceptual signal would depend on how well the heard sound-event matches the parameters of the input function.

The "four"-event controlling system detects the difference between the reference signal it is receiving and the perceptual signal. This difference is converted by this system's output function into a series of changing reference signals for the next-lower system (actually the transition level would probably get into this, but ..). The changes in the lower-level reference signals are those that bring the ongoing event as heard into a temporal shape that is perceived as closer to the higher-level reference signal. This control process occurs somewhat more slowly than the lower-level processes that create each phoneme, but fast enough to maintain the ongoing event in the form matching the specification given by the reference signal. The speed with which the event-forming control process can produce a given event depends on the speed of all the control systems involved. The speaker can now create an uttered word in his own perceptions.

That's a good place to stop.

I have certainly begged a lot of questions here. The biggest one is the question of how an event-error can be turned into just those adjustments of the sound-forming control systems that will make the event-error smaller. This implies a rather complex output function. Also I have only an elementary notion of how an event-perceiver might work. But we don't need to solve these problems right now: we can simply draw boxes in our diagram and label them by stating what they have to accomplish. They goes on the list of needed research.

The point I am trying to make is that by matching the amount of detail to a block-diagram description of a control organization, we can begin to see how the process of speech -- of controlling heard sound-events -- can be put into the control-theoretic framework. We can get the idea of many levels working simultaneously, each being the means for a higher level kind of control of a new *perceived* aspect of the process. An enormous number of questions is raised by doing this, but they're the right kind of questions: *how does this work*? Insofar as some of the details are already understood, we can at least partially answer some of the begged questions.

Of course as we try to answer these questions we will probably find that the initial guess about the control organization causes problems, so we have to alter the model's organization. In the example above, I have simply used my own conjectures about what the levels of control are -- that is, what classes of variables they are concerned with. I am not at

all happy about the degree of obfuscation that is necessary when talking about how the "event" level really works. I hope that real linguists will examine such problems and make their own revisions of the levels -- at the same time creating a descriptive mode that is suited to the system-diagram kind of representation, neither too detailed nor not detailed enough.

At the point where I cut off the development above, we had a system that could receive a reference signal specifying a sound-event, and immediately, concurrently, make its perception match that sound-event. In the process of doing this, sound-waves audible to someone else were generated. Most important, the speaker experiences these sound waves; experiencing them is the essential part of controlling them. At the sound-event-level, we first have something that could qualify as a "word."

The word, however, still has no meaning. It's just a sound-event, a perception like any other auditory perception. In my hypothetical hierarchy, the next level up is "relationships." One possible relationship between two perceptions, A and B, is "A means B". Given A, the relationship is brought into being by bringing B into perception. Given B, it is completed by producing A. The meaning-relationship is not necessary commutative; that is, B means A may have to be learned separately from A means B. But again, that's a question we can beg for the time being. If the relationship is "A means B," we can call the A-perception the "symbol" and the B-perception the "meaning of the symbol."

The point is to represent giving meaning to a word (or translating a word into a meaning) as a process of controlling a relationship. The basic reference signal is simply "find a meaning." When the missing element of the relationship is produced through lower-level control processes, the error is corrected: a meaning is requested, and a meaning is perceived. The perception that constitutes the missing element can then be passed on to higher systems, which can work with the meaning as well as with the symbol. I am aware, by the way, that this same function can be accomplished by content-addressed memory.

According to this view of meaning, any perception can mean any other perception. I think this is basically the case. But for communciation and for many higher-level processes, it is advantageous to have one member of this meaning-relationship be a very easily manipulable kind of perception -- i.e., words. Also, the speed of finding meanings suggests that memory is brought into the picture, and the fact that finding meanings does not have to involve any overt action implies that the lower systems supplying the meanings are operating in the imagination mode: the meaning control loop is closed internally.

Finally (there's no point is going even further beyond my ability to guess what a competent model will look like), the little trial communication I presented day before yesterday:

My basic hypothesis was that the speaker begins with a non-verbal experience, a visual image, which is to become the communicated meaning of a spoken series of sentences. The first goal must be to hear oneself producing a spoken description which, when interpreted by all levels from

intensities to the highest, evokes a non-verbal meaning which matches the image one is looking at. Many levels of control are involved, and each one must be actively varying the reference levels of those below so as to match its own perception with the reference signals it is receiving from above. The highest level of control compares the images evoked by one's own words with the intended image; when there are errors, the developing sentences are modified as they are being produced to make the perceived error smaller and smaller.

The first phrase was "In front of me." What image does this produce? In me, only a vague sense of the space before my eyes. To make this image more exact, I then add "on the table,". This alters the meaning and restricts it to only part of that space: I am now imagining a tabletop, but the tabletop is devoid of coins. Then follows the phrase "are three", which by itself simply evokes the sense of threeness or three blobby areas without further identification -- perhaps, as suggested, an imagined counting process. Finally, the last word, "coins.", pops coins of some sort into the three blobby areas.

There is still no sense of position, however, unless I imagine a default position for each coin. The remainder of the communication evokes in my mind specific positions on the table. Quite probably I supply meanings that the words do not actually suggest to me, manipulating my mental image directly to make it match what I am seeing without giving enough consideration to whether these final manipulations came from the words I am hearing myself say. This is how I can feel that I have adequately described the pattern when in fact I haven't said how big it is, or what the relative distances between the coins are. And a few other possible failures of communication which I noted in passing.

Note that exactly the same perceptual processes occur in the listener, although as mentioned the listener could well end up with a different picture owing to differences in perceptual organization. By drawing a picture, the listener can supply a real image to match the imagined one he has been building up from hearing the words.

Please understand that I'm going through all this not in order to impress anyone with this pseudo-model of communication, but only to show a general approach that would be consistent with the control-theoretic point of view. I'm just cranking the engine; I don't know where you will end up driving this car. In the above scenario I haven't considered any complications, such as the speaker's having a model of the listener's likely interpretation, and so on. All that can come later. If we can begin to get a glimmer of a control hierarchy behind the manifestations of speech, even though we can't account for all the known complications, we will have at least made a start toward "thinking in other categories."

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Date: Sat, 22 Jun 91 12:58:05 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Distinctness vs information

[From Bill Powers (910622.1200)]

Martin Taylor (910621) --

> ... I don't like Bill Powers' wish to keep "information" out of the
>discussion. Not only do speakers control for the distinctness of words,
>they control for distinctness at all levels. For the most
>part,communication is the attempt to select in the mind of the partner a
>satisfactory configuration out of all the configurations that the
>partner's mind can produce.

I don't necessarily want to keep "information" out of the discussion -- only to discourage its use as an explanatory term. It may well be that speakers control for distinctness, and that in doing so they increase the measure of information in the communication. But they don't increase distinctness BY MEANS OF increasing the measure of information -- just the opposite. They do it at lower levels by separating words, exaggerating sounds likely to be misheard (s,f,th), and so on. They do it at higher levels by adding modifying phrases, using redundancy, selecting words they have reason to think the speaker knows versus strange words, and so on. These methods of increasing distinctness do not require perceiving and controlling Shannon information content. They may or may not increase Shannon information content as a consequence of their use. But the speaker does not ever have to know the information content in technical terms. Neither would knowledge of the Shannon information content be of any help in indicating just how to alter one's speech to increase that content.

Also, these methods may or may not improve distinctness as experienced by the listener. All that the speaker can do during any communication is improve the speaker's experience of distinctness (as sensed or as judged in terms of higher-order perceptual interpretations). The listener's requirements for experiencing distinctness are appreciated by the speaker only over the course of one or more complete conversations. Within any one utterance, the listener's experience of distinctness can't be sensed by the speaker and thus can't be controlled. The only distinctness over which the speaker has any immediate control is his/her own experience of it. The speaker may IMAGINE that speaking in a certain way will improve the distinctness that the listener experiences, but this is really an open-loop process; the speaker is only guessing. The listener may have forgot to turn on his hearing aid, so emphasizing the "SH" in "shift" may only make the result sound more like "fifth."

I used to live on "Whitfield" Road. I have received letters addressed to Woodfield, Witfield, and Watfeld Road (in each case, having given the address over the telephone). I gradually learned to say (almost) HA -WIT-FEELD in the attempt to get the "WH" sound over, and eventually ended up spelling it out every time. Some people heard it correctly the first time with no trouble. I never did run into anyone who asked whether I was saying WHIT or WIT. The problem wasn't one of making a distinction. The same people who misaddressed my letters despite my strenuous mouthings probably pronounce whale as "wale" and "which" as "witch" (and maybe "weapon" as "whepon." But I never did consider the information content. I don't think that doing so would have done any good.

In terms of a "pandemonium" model, distinctness is the contrast between

different perceptual signals generated simultaneously by the same input set. If there is no clear winner, the result is experienced as indistinct, implying that more than one perception has a significant magnitude so that more than one higher-order interpretation becomes valid and subject to (possibly conflicting) control. This is what makes (some) puns funny.

I'm trying to say that while "information" may be a valid and convenient measure of distinctness, it can't be part of a model that explains how distinctness is sensed and controlled. It's a perception that belongs in the observer, not in the observed system.

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Date: Sun, 23 Jun 91 12:06:08 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: saturn.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: Experiential Psychotherapy 2

From: David Goldstein
Subject: Experiential Psychotherapy

This is a letter I wrote to Alvin Maher. I thought that CSGnet people who were interested in therapy might be interested in how I analyzed the practice of Experiential Psychotherapy.

June 21, 1991

Alvin R. Maher, Ph.D.
University of Ottawa
Center for Psychological Services
11 Marie Curie
Ottawa, Canada K1N 6N5

Dear Dr. Maher:

I have had the opportunity to listen to all three session tapes along with the transcripts. I think they are great and hope you find a good publisher for them.

I mentioned in my last letter, that it might be fun for us to look at the four steps of Experiential Psychotherapy from the view of Perceptual Control Theory (PCT). I assume that you have received the manuscript and may have had a chance to give it at least a brief reading.

So here goes. As I see it from the perspective of PCTP, step 1 and step 2 serve the purpose of identifying an internal conflict. Step 3 and step 4 function as the conflict resolving steps.

(a) Step 1--Attaining the level of strong feeling. The topic which the patient winds up discussing will be one which, in PCT terms, has very little or lots of error signals associated with it. The experience described will be one which is very well controlled or one which is not well controlled. It makes good

sense from PCT to select topics in this way.

I assume that if you gave your patients the Life Perception Survey that the topics chosen would be ones with very high or very low satisfaction ratings. I might mention that the way that I developed the Life Perception Survey was to think about the different presenting problems which people raised in the first session.

(b) Step 2--Appreciating the experiencing. I see step 2 as one interation in the method of levels. The "inner experience" is the same as "going up a level" in PCT. I can see how attaining a strong level of feeling in step 1 will increase the chances of being able to successfully go up a level and to feel confident that the inner experience you obtain is genuine and worthy of therapy attention.

When you focus on the initial experience in step 1, the experience is at about the program level of perception in PCT terms. There is some specific action which the patient takes in the scene. Carol does not follow the car or does not allow the person to get close in the parking lot. Joseph does not order a worker around who does a sloppy job. Dora does not talk about her pains with people and continues to go to work.

The inner experience which is identified in step 2 is at about the principle level of perception in PCT terms and seems to be one side of the internal conflict. Carol is being defiant. Joseph is being bossy. Dora is being bad.

The self-image experience is at the system level of perception in PCT terms and is at the level above the principle level of perceptions. It is from the level of the self-image that the conflicting reference signals come. The desired self-image has to be changed. This seems to be what you do in steps 3 and 4.

(c) Step 3 and Step 4--Being the experiencing in earlier scenes and Being-behaving in prospective scenes. I see these steps as neat ways of resolving unspecified internal conflicts by changing the self-image from which the conflicting reference signals come. Let me explain what I think the conflicts might be in each of the tapes.

In the Carol tape, Carol wanted to be defiant but was afraid to be defiant; she wanted to be safe. She didn't want people to get mad at her because she was so weak that she would't be able to handle it. She wound up thinking of herself as stronger.

In the Joseph tape, Joseph wanted to be in charge but was afraid to be in charge; he wanted to avoid making mistakes. He might make a dumb, impulsive mistake like he did in the past. The person in charge is the responsible one. He wound up thinking of himself as more capable of being a leader.

I am a little foggy on the Dora tape. The obvious conflict was for Dora to be bad versus be good (and endure the pain). However, I also think that Dora was in conflict about being dependent or not. She didn't like being dependent on her second husband, perhaps because of her experience with her first husband. She didn't like using her neck pains as an excuse because she was not completely incapacitated by them and she wanted to be tough and self-reliant. Dora wound up feeling comfortable about being dependent and relying on her second husband.

I am fascinated by the fact that you are able to resolve internal conflicts in such an indirect way. As far as I can tell, you do not think about what you are doing as resolving internal conflict. Is this correct?

As I mentioned over the phone, the way you work with the inner experience reminds me of the way people with Multiple Personality Disorder force the therapist to work. Each alter is treated as a separate person. You treat each one side of each conflict as if were the only one. With MPD patients, the goal of therapy is to help the person become one integrated person. I see steps 3 and 4 as resulting in a changed self-image.

Both the steps take place in what PCT calls the imagination mode. See page 8 of the manuscript I sent you for a brief explanation of the imagination mode (video tape versus live broadcast). Step 3 is remembering. Step 4 is imagining. If you were the new you then, how would have things been different? If you are the new you in your curent life situations, how will things be different?

It seems to me that you give permission, encourage the person to take one side of the conflict, the one you think they really want. They do this in the safety of the session. They simulate what it would be like if that side of the conflict won out. The other side of the conflict is temporarily held in suspension.

They start to experience a new self-image without the conflicting principle level perceptions. They like what they experience and become committed to it. The new self-image results in the conflict being resolved. In step 4, they start to control the new self-image.

As mentioned in the manuscript, the heart of helping a person according to PCTP is to identify and resolve internal conflicts. Then the persons own Reorganization System will take over from there. Your method of doing this is perfectly consistent with PCTP. As I mentioned over the telephone, I think that you have saved me years of trial and error in practice development. Perhaps PCT will lead to some refinements in the practice you have outlined. We will see.

When I briefly explained the approach you take to Bill Powers, he

had a positive reaction and made an interesting observation. Bill and I have been having an ongoing conversation about how to model things which are more qualitative at the higher levels of perception. [If you ever get around to reading Bill's stuff, you will see that he is a strong believer in the modeling approach. Bill has created models for people performing in tracking tasks which almost predict perfectly, moment by moment, what the person's actions will be. Psychology has never seen anything like this before.] He made the interesting point that the therapist seems to be selfless in your approach. The therapist uses all his/her own existing control systems to simulate the patient. In that way, the therapist can have similar experiences within the therapist which, perhaps, the patient is having. Do you agree with the idea that your own self-image is being suspended during the therapy session? Does it feel this way to you?

I am enclosing a manuscript which I wrote which goes over a self-image exercise based on PCT. The three levels of perception mentioned, namely, program, principle and system, becomes clearer from this self-image exercise. I am thinking of asking Cathy who is described in the paper if she wants to be my first Experiential Psychotherapy patient. In terms of the Perceptual Control Theory Diagnostic Survey, which is in the manuscript I sent you, items 8, 9 and 10 have a high need for change within Cathy. Perhaps by using your four steps, I can make some progress in these areas.

My next step in learning more about your approach is to order and read the book you wrote about dream work. And after that, probably your theoretical book.

Time to draw this overly long letter to a close.

Best regards,

_____
David M. Goldstein, Ph.D.
801 Edgemoor Road
Cherry Hill, NJ 08034
(609) 667-0166

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Date: Mon, 24 Jun 91 07:48:09 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Metasystem transitions and control theory

[From Bill Powers (910624.0600)] --

Cliff Joslyn (9106xx) --

Thanks to Don Campbell, I have a copy of the talk that Cliff Joslyn will present on July 4 at the "First workshop of the Principia Cybernetica Project," Free University of Brussels. This paper presents HCP (or PCT) in a way that should go far toward establishing a link between cyberneticists and control theorists. I thank you, Cliff, for your gratifying and generous treatment of my contributions, as well as for your willingness to take on the role of ambassador and synthesist.

As this paper has not yet been presented, I won't review it but will only put down some thoughts that come to mind from reading it. Probably the most clarifying aspect of the paper comes in references to Turchin's "evolutionary control hierarchy" [Turchin, Valentin (1977) *The Phenomenon of Science*; New York: Columbia University Press]. Turchin's thesis, I gather, is given by the title of a cited paper: "Metasystem Transition as the Quantum of Evolution."

A metasystem is, in HCT terms, a higher level of control system. A metasystem transition is then the evolutionary process that adds a new level of control (or perhaps I should say it is the process of adding a new level of control that creates the appearance of a major evolutionary increment in complexity). This is a nice distinction between evolutionary changes that simply shuffle the genetic details around without altering the hierarchical complexity of organisms, and those that result in adding a new layer of organization. The latter would seem far more important than the former, although gene-shuffling can't be dismissed as trivial.

What this idea has clarified in my mind is a reason for the existence of levels as discrete functional units. I have had vague notions along these lines from the beginning, but forgot about them and never treated them systematically because, as Cliff has noticed, my treatment of evolution has been pretty disjointed. But evolution, and particularly the concept of metasystem transitions, brings in a new reason for supposing that the levels have some internal coherence and that a given level will have much the same character no matter what sensory modality is involved.

In trying to characterize levels of organization, one exercise I have found useful is to try to imagine how the world would seem if a particular level were the highest level in the organism. It's clear now that this is the same as trying to imagine the organism at a much earlier stage of its evolution. I see now that an adult creature with configuration as its highest level of systematic control (all other control occurring via reorganization) could not be human. Such an organism could control postures, but not movements or anything more complex. It could extend or retract a syphon or a tentacle; it could open and close an aperture; it could anchor itself against a current (but could not swim against it). The transitions between postures or configurations would not themselves be controlled -- they would take place at whatever rate was dictated by properties of muscle-like machinery and the viscosity of the medium. Resistance to establishing a given configuration could be overcome through varying the motor forces, but obstacles could not be avoided, nor could a particular path be followed repeatedly from starting configuration to ending configuration.

Such an organism would have to be small because it would have to live in

a world dominated by viscosity, there being no other limitation on speed of changes. Being small and subject to viscosity, it would not have to be concerned with the dynamics of behavior: inertia would be negligible. It would have to live, most likely, in a liquid medium, because it has no way either to propel food toward itself or to propel itself toward food -- not in a systematic or disturbance-resistant way. Anchoring itself against a moving current would be its only equivalent of mobility. It might wave flagellae, but the waving would be simply evidence of a natural open-loop oscillator (or, as in the case of E. coli, a rotor that can only be turned on and off regardless of what the flagellae are doing). The movements themselves could not be controlled relative to an adjustable reference level for speed or frequency, nor would any adjustment be made if external disturbances either slowed or speeded up the movements.

The upshot is that there is no point in trying to imagine how the world would seem to a human being deprived of all levels above configuration control. An adult human being so cut off would die almost instantly. Even walking or chewing and swallowing would be impossible. The fewer the levels, the simpler must the organism be and the more restricted must be its niche.

This leads to the question of the nature of the next level that will be added at any stage. We judge levels, or at least I have been doing so, on the basis of what exerience as a human being tells us. If we were able to unravel the stages of metasystem transition that have occurred along the human track, we might well find that at each stage a more or less familiar level of control was the highest existing one. It might well be that human development from egg to adult recapitulates the sequence in which human levels of control were added. But if we were able to trace the sequence for another organism like a elephant or a cockroach, we might find a different story.

A "level" of control is defined by the perceptual computations typical of that level, because input, not output, is controlled. Those computations are applied to perceptions that already exist at the next lower level. In order to propose that levels exist at all, we have to imagine that when a new level of perception is invented, a new type of computation is invented, which them proliferates "sideways" and results in adding many more control systems that use the same kind of perceptual computation. Because all neural signals are alike, there are no boundaries between "sensory modalities" in a nervous system. The newly-developed perceptual computation does not know the meaning of the inputs it receives. If all these reasonable supposition happen to be true, then we would expect all instances of control systems at the new level to sense and control variables of the same logical type.

But the nature of a new type of control system would depend in a more global sense on the nature of those already existing at lower levels. I see the impetus behind starting a new level as being a consequence of proliferating control systems at the previously-highest level. The more systems that are acting in parallel, the more likely it is that the control actions will run into conflict situations. Organisms that can move themselves through a medium in a controlled way, for example, might develop motion-control to some considerable degree before running into a

conflict situation that is more than a transient problem. But when motion control elaborates enough, the organism will begin to run into obstacles, blind alleys, other organisms, or unfavorable environments that it would not otherwise have encountered. If these situations arise with significant frequency, the organism MUST develop a new level of control or die from the dangers to which it is now exposed. It must learn, at least, to back up and try another direction. Eventually this would become an elaborate series of moves -- events. And inevitably, these moves would generate new types of conflict because of their implications in the environment. The organism would have to learn how to steer away from dangerous obstacles instead of just going through its pattern of movements and trusting that their blind execution will again take care of the problem. Relationship control would appear.

Suppose, however, that the proliferation of control systems takes a different course at the level of configuration control, anchoring being an example. Suppose the anchoring systems elaborate to the point where the anchors become roots. This is a committment to a way of life that utterly eliminates some problems of motility, but introduces other kinds of problems. Now the organism may find that it often anchors itself in such an orientation that it doesn't receive enough sunlight. There may be a conflict between WHERE it anchors itself and the consequent availability of nutrients. One solution to this problem might be to attain the capacity to control the height to which it grows, according to season and circumstance. Another might be to acquire control systems that maintain a constant orientation to the sun, or the wind, or the rain. So a new level of control would develop that might be interpretable by human beings as something roughly familiar, but which would in fact be the plant's next level of control founded on a different style of interaction with the environment, and an earlier committment to a particular pattern of control systems at the previous level.

At every step of this continuing process of metasystem transitions, there arises the possibility of different elaborations of the new level of control -- committments to radically different solutions of the previous problems -- that lead to different problems. Thus at every step we could expect to find different "new levels" being added. The tree of life, as evolutionists have long known, branches again and again. Hierarchical control theory, coupled with the concept of metasystem transitions, gives us a new basis for identifying the branches -- a basis that looks at control processes rather than chemistry, at problem-solving rather than at a wholly blind process that implies infinite and random gradations of change.

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Date: Mon, 24 Jun 91 14:30:05 -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: ref for error again

Bill,
You wrote on june 19 that "control theory is a threory of the individual, not to be confused with theories which classify people in terms of specific goals or perceptions." I understand that this is the case. However, it does seem to me that despite this fact, CT offers the best opportunity for

classifying individuals because we know what the important factors are--goals. It seems to me that many theories of personality center around what people DO, not the purposes for why they do that which they do.

You just can't go up to a person ans ask "Do you climb mountains and ride rollercoasters to feel immortal?" They aren't going to know that consciously. Whereas I agree that the similarity among individuals is the control Structure and not the Goals, I think at the very deepest levels there are very few goals to choose from, if there are even two. One is that struggle for immortality. We may all be referenced for that but at the next level lower we diverge into a score of goals and hence are extremely different. All the same, but different--no contradiction.

My point is not what that goal may be but that we all may have the same ultimate one.

you also said that we don't seek out that which creates the most error--"we dont' use defective parachutes, or ropes that are fraying." So are you saying that as long as one believes that the rope will hold or the parachute will open, the error is small. It seems like the error would still be large (larger than walking through a park) but I see how it may not be. Can we experience error at a low level but cancel it at a higher one ("Yes, I realize that you are falling and that the earth is approaching rapidly, but its OK because the parachute will open").

So, yes, we do as you say avoid the largest errors but I still want to know why (or how) some of us desire that which we call "thrilling." Is this for pleasant sensations or for feeling immortal? I think this is a valid question that can't just be answered by asking the person in question.

By the way, I read "Feedback Thought in Social Systems Science" by Richardson. Maybe I already wrote this but I can't remember. It was strange reading it having started with Powers cause this theory fits in between the two threads. Once ya start with an integration of two threads, its hard to see the threads separately. Anyway, it was good to get a taste of what the arguements and issues are, and more interesting to note that those seem to be the issues which keep coming up on this net.

Sorry it takes me so long to respond.

Carpe' Diem

Mark Olson

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Date: Mon, 24 Jun 91 12:53:05 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Linguistics/Events

[From Rick Marken (919624)]

Bill Powers (910621.2300) says:

>   If a properly-
>working control system is present, it will VARY the articulations so as
>to create exactly that sound that the speaker intends to hear. We don't
>need to worry too much about what those articulations are -- particularly
>if we contemplate doing experiments in which we interfere with the mouth
>cavity so that different articulations are needed to produce the SAME
>sound.

>The "four"-event controlling system detects the difference between the
>reference signal it is receiving and the perceptual signal. This
>difference is converted by this system's output function into a series of
>changing reference signals for the next-lower system (actually the
>transition level would probably get into this, but ..). The changes in
>the lower-level reference signals are those that bring the ongoing event
>as heard into a temporal shape that is perceived as closer to the higher-
>level reference signal. This control process occurs somewhat more slowly
>than the lower-level processes that create each phoneme, but fast enough
>to maintain the ongoing event in the form matching the specification
>given by the reference signal. The speed with which the event-forming
>control process can produce a given event depends on the speed of all the
>control systems involved. The speaker can now create an uttered word in
>his own perceptions.

I think I'm close to a reasonable non-verbal demo of this "event control"
process. Instead of speaking a word, the subject produces a visual event;
an "expanding square" for example. The "articulator" is the mouse, which
controls the height and width of the square. Right now I have it so that
horizontal movement of the mouse influences the width of the square,
vertical movement influences the height. The height and width of the square
are
also influenced by different, variable disturbances. The event "starts" when
the subject moves the mouse in either the x or y direction or both. In order
to create the event, the mouse movements must vary appropriately during the
event to compensate for the disturbances and preserve the shape of the
square during the event (as the size of the sqaure is changed by the subject--
at his/her own rate). I have done this task
with events lasting as little as 1.5 seconds (during which time the square
expands and then decreases back to original size). Looking at the traces of
mouse movements and lengths of the square sides during the event it is clear
that I am compensating for disturbances which vary slowly during the event.
There is still much to do but I think this is a start at showing control of
an event, not just control of the static length. Of course, one the demo
works well, I'll make a model that can control events; this might not be
trivial. The problem will be getting the mode to perceive the event; ie.
I must figure out away to transform the degree to which changes in the
shape of a quadrangle resembles expansion and contraction of a square over
time. Any suggestions?

Regards

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Date: Tue, 25 Jun 91 00:17:28 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Re: $ 0.04

Whatever happened to the attempt at modeling the 4-cent problem with HCT-PCT? Things were just heating up.

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

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Date: Tue, 25 Jun 91 11:10:00 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Hugh Petrie <PROHUGH%UBVMS.bitnet@VMD.CSO.UIUC.EDU>
Subject: DEMOS

From Hugh Petrie

Bill, I tried to send this personally, but with no reply yet. Then I thought that others, especially some of the new folks, might like to hear about it as well. I have been meaning to purchase your demos, 1 and 2, and any other more recent ones to use in a class this fall which I have been teaching. It turns out the university has a little money left in this fiscal year's budget which I would like to use for this purpose. Could you tell me how much they are and where to send the check. I'll send a personal check and then use your paid invoice sent back to me, dated before July 1 (end of fiscal year) to get reimbursed. Thanks.

Hugh

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Date: Tue, 25 Jun 91 12:13:29 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Harman thesis

From Rick Marken

Tom Bourbon

I'm sending this via the net because I tried sending to your Bitnet address but it bounced.

I just wanted to acknowledge receipt of Wade Harman's thesis proposal. I have read most of it -- I think it looks great. You are apparently doing a wonderful job of teaching your students about control theory. I hope to have some more detailed comments about Wade's thesis later. My only suggestion is to shorten the intro section and play up the multiple degrees of freedom aspect. This is a really fascinating study -- and it relates to many of the more interesting things that people do -- like speaking -- where many degrees of freedom must be varied to produce a particular result. I think the fact that a person can manipulate all these degrees of freedom to control a particular result presents quite a challenge to the theories of behavior that Wade discusses. I would also suggest emohasizing the importance of the modeling you are doing as an approach to figuring out what the subject is actually controlling. I think trying alternative models of how control is achieved in this task is one good way to approach that apparent "attention switching" phenomenon with size and angle control.

Anyway, it is an excellent thesis; and a strong and fascinating demonstration of the power of controlling input rather than output.

I'll be VERY busy for the next two weeks -- preparing for (and then going on) vacation -- so I don't know if I'll be able to contribute anything sensible to CSGNet in the near future (but then, do I ever?). But give Wade a thumbs up from me on the paper. It's alwasy nice to know that there is someone out there who is doing the research that needs to be done.

Rick

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Date: Tue, 25 Jun 91 15:19:37 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: quick reply on the run

[From Bruce Nevin (910625.0942)]

Bill Powers (910621.2300) et al.

I have been remiss and will continue to be for a couple of weeks. Yesterday I took a vacation day unexpectedly because my now 4-year-old's birthday party was scheduled for her birthday instead of Sunday

as anticipated. We (family) are driving to Chicago Friday for my wife's HS reunion. I will fly thence to SF Saturday. Will give a paper Tuesday at the Hokan/Penutian Workshop scheduled for Monday and Tuesday at the LSA Summer Institute (linguists' organization). Achumawi, the native Californian language I researched in 1970-74 and whose phonology I will my writing up as a dissertation over the next couple of years, is a Hokan language.

But I blather.

Thanks, Bill. I will study your suggestions and see what I can come up with. I do wish I had a laptop to carry on the train with me! But since I completed my conference paper this weekend maybe I'll have more time for this on the PC at home.

Reason for all the low-level detail was your request to start with physical intensities of sound energy. That necessarily involves one in a lot of detail.

We need to be clear that communication and use of language are distinct. A great deal of communication is about relationships between oneself and others. As Bateson's writings occasionally note, mammals place a great deal of importance on relationship, and for reasons that he discusses and that I have mentioned previously (regarding things like authenticity and sincerity) most of this communication is *necessarily* nonverbal, including nonverbal aspects of the use of language--how you say it as distinct from what the words literally mean, including things like vocabulary choice as well as obviously gestural things like tone of voice. (I suspect that most of what is of interest for the clinicians among us concerns the management of communication in this sense, including self-image, self-esteem, etc.)

Your proposed communication scenario about an arrangement of coins attempts to exclude this sort of communication by making the relationship between the participants an institutionally controlled variable that is not of concern to them individually, but the two participants are likely still each unconsciously and continually to construe available perceptions so as to support an ongoing process of imagining the other and the relationship.

But what you are concerned with is more narrowly what we may call the transmission of "verbal" information. This is a reflex of the fact that not all combinations of language elements actually occur in the language. The first order of redundancy is in the phonemic contrasts. If for every string of n phonemes heard in the present utterance you refer to a count of the number of n+1th phonemes that have followed those n phonemes in prior utterances, you find that the number of possible next-successors drops as n gets larger, and then suddenly rises to a peak at or near the full inventory of phonemes. These peaks concide well with morpheme boundaries (with some stickiness about infixes, etc., and a better correspondence if you calculate the relationship as a percentage). Thus, from expectation based on prior experience you can determine which strings (simple configurations) form elements on the next-higher level of construction. Dependencies among morphemes are not so simple as among phonemes, but in those dependencies

resides the "verbal information" that we are concerned with. I sketched how that works over-briefly and will try to return for a better shot at it later.

One event-control system per word (that is, morpheme)? Not sure:

Acoustic feedback is too late for controlling pronunciation of the current word. It could affect pronunciation of a subsequent repetition of the same word if it were repeated for correction. Seems more likely that acoustic feedback would influence management of implicated articulator(s) for *all* sounds involving it or them (and this would subsume non-immediate repetitions of the same word). For example, acoustic feedback for "f" would influence management of lips as articulators in all sounds involving labial articulations, supposing, for example, the lips were swelling with an infection.

I imagine cutting off acoustic feedback by playing white noise over headphones, and recording the reading of a text. Speaking in a very noisy environment, one may not even be aware of intra-cranial vibrations reaching the eardrum (though it is difficult for me to say whether lack of awareness implies lack of perception sufficient for control). Would pronunciation "drift" over time? Would it become over-precise sounding? I think of a deaf friend whose pronunciation is quite wide of our shared acoustic targets, but whose articulations are more available for "lip-reading" than those of ordinary speakers.

I imagine introducing a disturbance to perception of articulators by use of a topical anesthetic or something like Novocaine. My remembered experience with the latter is that I thought I was slurring my speech when I had the Novocaine, but did not detect any slurring when listening to others complaining of the same experience (on different occasions).

>The point is to represent giving meaning to a word (or translating a word
>into a meaning) as a process of controlling a relationship. The basic
>reference signal is simply "find a meaning." When the missing element of
>the relationship is produced through lower-level control processes, the
>error is corrected: a meaning is requested, and a meaning is perceived.
>The perception that constitutes the missing element can then be passed on
>to higher systems, which can work with the meaning as well as with the
>symbol. I am aware, by the way, that this same function can be
>accomplished by content-addressed memory.

You do not know what "a meaning" is. If you use the word "translate" you metaphorically at least presume that meanings subsist in a form something like that of language itself, a metalanguage for representing meanings. But a natural language necessarily contains its own metalanguage (one can talk about the language as well as about any topic, etc--arguments in Harris _Mathematical Structures of Language_ Wiley 1968), and conversely there is no need to use anything other than the language itself to represent its meanings, and finally if one were to devise a separate metalanguage for representing meanings that metalanguage would in turn require a semantic interpretation so where would its meanings reside? Meaning resides in the redundancies of the language itself. This is exactly analogous to Bateson's claim that mind is not transcendant but rather immanent in nature.

>According to this view of meaning, any perception can mean any other
>perception. I think this is basically the case. But for communciation and
>for many higher-level processes, it is advantageous to have one member of
>this meaning-relationship be a very easily manipulable kind of perception
>-- i.e., words. Also, the speed of finding meanings suggests that memory
>is brought into the picture, and the fact that finding meanings does not
>have to involve any overt action implies that the lower systems supplying
>the meanings are operating in the imagination mode: the meaning control
>loop is closed internally.

If on the other hand you mean association with memories of controlled peceptions that were concurrent when remembered word-dependencies occurred in discourses, I think we're in agreement, just fumbling around for ways to talk about it.

Got to run. It's 3:20 and I have to leave at 3:00 to get to my train without running.

Bruce

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Date: Tue, 25 Jun 91 18:19:26 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Speech Feedback, Aphasia References

[From Rick Marken (910625)]

Just a couple quick notes. First, to Bruce Nevin (910625) who says that acoustic feedback comes to late to correct pronunciation.

I think this is arguable. In the visual analog of speech that I just described, the subject can correct for disturbances that occur within a 1 second time window. In speech, the articulators are continuously correcting for the effects of others articulators on the sounds being produced. Thus, disturbances to the speech created by one articulator are corrected (to some extent) by another. The corrections are not necessarily perfect but they reflect the operation of the feedback control loop. You can prononce words while you eat (though you shouldn't talk with you mouth full -- but that's a higher level variable) and the changes in the shape and size of the mass in your mouth is continuously changing. If there were no feedback control (ie - if the same muscle forces were used to say a word with the mouth full as with the mouth empty) I doubt that sounds would be pronounced that would count as having been pronounced. Remember, feedback loops operate continuously. There can be lags and delays but the variables vary continuously. Thus, the control system doesn't really have to wait for the feedback about the state of a variable to arrive. The feedback is always there -- but if there are significant lags or delays it can produce instabilities -- and what looks like poorer control of the input variable.

On a related note, I got some references to the Blumstein aphasia study that were mentioned by one reviewer of the paper I submitted

to Psych Review. The number of references convinces me that the reviewer was almost certainly Blumstein himself. These references look great -- one is called "Acoustic invariance in speech production" -- wow, just what we wanted to know about. Is anyone out there familiar with Blumstein's work? If so, how about a description before I go out and look for the articles themselves?

Thanks

Rick M.
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Date: Wed, 26 Jun 91 04:59:17 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: linguistics; thrills

[From Bill Powers (910625.0800)]

I'm trying out a new schedule: send in the morning, then get new mail, before rates change. This will give me a whole day to think about mail. If this doesn't improve the quality of my responses, there's no hope for me. It's interesting how making a simple change like this has ramifying effects. I used to wait for 17:00, then read the mail at the evening connect rates, 0.10/min. Of course I'd then get engrossed in it, switch to the editor, and start concocting replies, while Mary ended up making dinner again even though it was my turn, and we'd end up returning the video without seeing it. Everything is connected to everything. Now I will send and receive at 4 cents per minute (I take it that everyone by now knows what 4 cents means) in the morning when my head is working better, have a leisurely day to think about replies, and take my turn cooking dinner (I'm a great cook as long as I can scrape, chop, fry, bake, broil, or boil what comes out of the bag from the grocery store) and watch the movie. Marital harmony, fairness, better output, entertainment, and cheaper communication. All by changing WHEN something happens. Wow.

Bill Cunningham (910624) et. al. --

>The choice to communicate by spoken word is purely arbitrary. We could
>just as easily have chosen handwritten or typewritten words with their
>attendant muscular control systems. We could also have selected several
>nonverbal media or a hybrid. Any principles uncovered should apply to
>other media.

Right. I picked verbal communication arbitrarily. In written communication, we perceive configurations directly. At first we string the individual letter-configuations into word-events (using a left-to-right scanning transition, for English), much in the way we hear words. Later we learn to see whole words as configurations, so a word that is read is a configuration-level perception instead of an event-level perception. (This is why we can read so much faster than we can talk or

understand spoken words. Handwriting is slower because the mechanics of writing involves an inherently slower set of procedures: saying "ah" can happen a lot quicker than writing all the loops of "a". An expert typist can just about keep up with slow speech. Shorthand ..)

I presume that the visual configuration-signals then skip the transition and event levels and go right to the relationship level where (very tentatively) meaning is assigned. Or maybe meanings get assigned at every level -- I don't want to make an issue out of that. Once you get to the relationship level it doesn't matter where the signals came from (although in reading you're missing some channels of communication such as inflection, tone of voice, facial expressions, and so on). Words, as words, probably don't exist above the event level.

>I had hoped to avoid issue of whether a human can think without words,
>but find that may be an essential part of the problem.

This issue may be easier to handle if we downplay the "thinking" part. Can we imagine experiences that are not words? Yes. Can we imagine moving objects? Yes. Can we see someone spill a salt-shaker into a bowl of soup and imagine how it would taste? Yes. None of this requires words or language. Is running mental models in imagination, complete with sensations, objects, movements, events, and relationships -- but without symbols -- "thinking?" If you like. Or you might like to reserve "thinking" for the category level and up, so that symbol-manipulation predominates. It's still manipulation of perceptions, but the perceptions are of higher levels. We say we think in symbols if we happen to be referring to the levels at which symbols are manipulated and controlled. These levels may be far from scientifically justified, but I think they do put some order into subjects like "thinking.

>What I sense strongly is control for the choice of the right word.
>Vocabulary comes in increments (words). If I control for the the right
>word, there is almost certainly error.

I suggest that the picture is clearer if we see it as control for the right MEANING. We VARY the word-selection, which produces varying meanings, until the evoked meaning matches the intended meaning. Isn't that what we mean by the "right" word? It's the word that produces the right sense of meaning. So word-selection is part of the output process, while meaning-perception is part of the input.

> Now, does my perception of what I see become the reference, against
>which the word selection must adjust?

For the moment, let's not worry about how the thing you're looking at becomes a reference signal. We have a more serious modeling problem here. Let's just imagine a mode of control in which the reference signal is selected somehow, and the error signal works through word-selection to evoke the nonverbal perceptual signal. The error signal clearly has to select a word -- but if it's the wrong word, how does the error signal point us closer to the right word -- the word whose evoked meaning will come closer to a match with the reference-meaning?

One possible answer is that the error signal drives a scanning process.

Unfortunately, error signals are one-dimensional, and they could drive only a serial search (if there's an error, keep scanning). I'm sure that this can't be fast enough, by orders of magnitude, to account for real-time speech or reading. Even if you substitute silicon for protoplasm. What we need is a parallel process, not a serial one.

Here's one way we can get it, a solution that's unique to control theory. There's a basic principle that says a control system controls ONLY what it senses. This means that if an output signal from such a system has multiple effects, the only effects that matter are those that are sensed by the system. All the others are side-effects and are not controlled BY THAT SYSTEM (they may be controlled by other systems). Thus in a system that controls arm position, the output signals may activate several muscles, and none of the muscles may align exactly with the direction in which this one system is sensing arm position. So in bringing the arm to a position where the perceptual signal representing this one dimension of position matches its reference signal, the arm may actually be rotated and deviated to one side. That doesn't matter, because this control system's reference condition is satisfied. If you want to prevent rotation, you need to add a rotation-sensing system; if you want to prevent sideward deviation, you need to add a system to sense and control that.

OK, in the present case, we don't need a single system that will find "the" right word. What we need is just a way to select ANY word whose perceptual meaning matches the reference level. Furthermore, every word evokes a number of perceptual meanings -- the more so if we entertain the idea that the link is through memory associations. It doesn't matter how many unwanted perceptions are evoked IF THE WANTED ONE IS AMONG THEM. The control-system's input function will detect the perception that is relevant, if the raw material is there, and ignore those that are irrelevant. This one control system will therefore find the perceptual meaning it wants.

This greatly reduces the job that has to be done under direction of the error signal. I can imagine a limited sort of mapping function, that organizes word-space into a few dimensions of variation, or even only one dimension, so that a positive error signal says "look that way" and a negative one says "look the other way." The function receiving the error signal would not have to settle on any specific word: it would just have to grab a bunch of words, each of which would evoke a bunch of meanings, so that the needed meaning is among the results.

This means, of course, that a single isolated control system can't really match meaning to intention. Many control systems, each concerned with a different dimension of the overall meaning, would have to act at the same time. Each would be grabbing a different collection of words to evoke a different subset of meanings, with a different intended meaning to be found among them. For any one system, the selection/perception problem is relatively simple and crude. Refinement comes only from the concerted action of many control systems each concerned with a different dimension of meaning and all acting simultaneously.

Implied in this model is the idea that the word-selection process itself is of a lower level, so that the systems controlling for meaning act on a

shared mechanism. We can imagine that each higher-level system activates a broad group of these lower mechanisms, a different group for each higher-level system. The NET evoked meaning is then the one that is the intersection or resultant of all these activated subgroups (or, also likely, the net activation is the resultant of all the activating signals).

I think this is beginning to smell like a neural network of semi-familiar design. It's probably equivalent (at least in some respects) to a model in which a perception is represented by one complex composite signal, and in which reference signals and error signals are also of that nature. But I think that when it comes down to implementing this sort of system in wetware, the simple-signal-multiple-system arrangement will turn out to be closer to how it actually works. In engineering control theory, signals are often represented as vectors or matrices, with matrix addition and subtraction and so on filling in the function-boxes. But when you BUILD such a thing, each signal has to be handled individually; in hardware, the matrix has to be expanded into its component operations. The matrix notation is only a computational convenience. If you've ever written a program that will DO matrix operations, you already know how true this is. We're just starting with the expanded system.

As I've mentioned before, the REAL system design probably lies between these extremes. A perceptual function is probably just one path through a complex sensory nucleus, in which there are intereactions among paths that we would have to represent over and over in a model that uses parallel separated input functions. But I don't know how to handle that.

Anyway, are we getting any closer to a realizable model of meaning?

This is already too long, but --

Mark Olson (910624) --

>It seems to me that many theories of personality center around
>what people DO, not the purposes for why they do that which they do.

Yes, that's true, and that's what is wrong with such theories. They're really not theories: they're attempts to describe facts that are generally observable in human beings. But the worst aspect of them is that they're NOT true. There are no specific behaviors or motivations that are actually common to human beings, not at any interesting level of generality. What you're describing is the old way of understanding human behavior -- looking for generalizations that apply to homo sapiens, and forcing what is true of some (to varying degrees) onto all. Control theory is about properties and organizations that are actually to be found in every single human being under all circumstances with no time-outs and no excuses.

>You just can't go up to a person ans ask "Do you climb mountains and
>ride rollercoasters to feel immortal?" They aren't going to know that
>consciously.

Who says so? I wouldn't hesitate to ask a person why he or she climbs mountains or rides rollercoasters, if there was any interest in answering such questions. If the person didn't say something about wanting immortality, then I wouldn't assume that the person really did want immortality but just didn't know it consciously. I'd rather explore the reasons that the person gives, to see how they fit together and make sense of the behaviors, than ignore what they say and substitute my own guesses. Guessing at people's unconscious motivations is really just a form of one-upsmanship (I know something about you that you don't know).

>Whereas I agree that the similarity among individuals is the control
>Structure and not the Goals, I think at the very deepest levels there
>are very few goals to choose from, if there are even two. One is that
>struggle for immortality. We may all be referenced for that but at the
>next level lower we diverge into a score of goals and hence are >
>extremely different.   All the same, but different--no contradiction.

At the VERY DEEPEST LEVEL, I can agree, we may share similar goals, but these goals aren't things like immortality. That's a learned goal -- somebody told us that immortality was possible, and we had to decide whether to believe it or not. Even survival is a learned goal. I can see that our very deepest goals -- our intrinsic reference signals -- concern the control of variables that must be controlled if we are to survive (or, given the chance, become immortal). But no one of those goals is "survival" or "immortality." You might say, for example, that in working my head off on control theory I am seeking immortality. Maybe so, but I won't get it. I'm going to die, and to me that means that the idea of going on living forever (actually living, so I can go on doing things I love to do, be with real people as they are right now, see what's around the corner a few centuries from now -- and still care about it) is a fraud. People may be reading some of my stuff a hundred years from now, but I won't get much satisfaction out of that. The IDEA of it may give me satisfaction right now, but that's basically all I'll get out of it -- whatever it means to me RIGHT NOW. Before I kick off.

As to "error as a goal," I just don't think that this makes sense in control-theory terms. Error signals drive lower-level reference signals, but they don't serve as their own reference signals. I wouldn't even know how to model that. As far as I can tell, they aren't even sensed. What you sense is the effort to correct errors. You ALWAYS try to correct errors if the control system is active; if it isn't active you don't try to do anything about them.

Error is simply a natural part of acting. Without error you don't lift a finger. When you select a goal that requires efforts near the limits of your capacity to produce them, the error signals HAVE to be larger than usual to produce that much effort. If you succeed at such difficult tasks, I wouldn't be surprised if you enjoyed that: the experience of success at controlling is probably one of those deep goals you mention as common to human beings. Good thing, too. But why interpret this as enjoying error? If what you enjoy is error, why try to correct it? Just go halfway up the rock face and relax everything. That could be a lot of fun, except for the very last bit.

>So, yes, we do as you say avoid the largest errors but I still want to

>know why (or how) some of us desire that which we call "thrilling." Is
>this for pleasant sensations or for feeling immortal? I think this is a
>valid question that can't just be answered by asking the person in
>question.

So, I'm asking. Do you want thrills in order to feel immortal? How does feeling a thrill imply feeling immortal? Are you talking about escaping death? If so, does a thrilling escape from death imply that you can do it again, any time you please? Does immortal mean invulnerable? Would it be desirable or advantageous for human beings to believe that they are magically protected from consequences? You see, even to consider your question, much less try to answer it, I have to guess about what you mean and raise the issues that my guesses imply. Maybe you'd prefer to do your own guessing.

You're in your early 20s. I'm soon to be 65. Isn't it odd that you should be concerned with immortality as a goal while I, to whom it should matter more, am not?

Rick Marken: No suggestions. Keep on keeping' on.

Tom Bourbon: Patience is a virtue.

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Date: Wed, 26 Jun 91 06:58:25 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: acoustic feedback

[Friom ?Bruce Nevin (910626.0700)]

Rick Marken (910625):

It seems that you are right, and that I have not been impressed enough by the quickness of response of the muscles and the short neural pathways involved, to which Bill alluded.

The "white noise" sort of test, depriving the participant of acoustic feedback, might indicate that we can use kinesthetic and tactile perceptions as a backup, but that control with them is less precise and liable to "drift" if we go too long without hearing ourselves speak. Might have to set up some vibration in the skull to mask intracranial feedback, including that in soft tissue and eustachian tubes. I can imagine setting up the microphone inside a kind of mask that fits over the nose and mouth, and having headphones on for the masking sound. Other participant can hear the first through headphones (while both are being recorded), but can only communicate nonverbally or in writing. Or else just have one reading solo, though reading pronunciation is already more self-consciously controlled than conversational speech.

I'll keep this in mind and maybe the resources to do it will turn up.

Bruce Nevin
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Date: Wed, 26 Jun 91 16:09:42 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: Speech Feedback, Aphasia References

[from Gary Cziko 910626.1600]

I've been following the language discussion with much interest. I am hoping to do some PCT language research with second language learners next year, but I don't want to get into that now.

Rick Marken (910625) says:

>You can prononce words while you eat
>(though you shouldn't talk with you mouth full -- but that's a higher
>level variable) and the changes in the shape and size of the mass in
>your mouth is continuously changing. If there were no feedback control
>(ie - if the same muscle forces were used to say a word with the
>mouth full as with the mouth empty) I doubt that sounds would be
>pronounced that would count as having been pronounced.

This and other recent discussion about feedback in language production found me doing some strange things which I thought would be fun to share. You don't have to put food in your mouth and talk to see how disturbances are corrected. You can play with your tongue instead. For example, keep the tip of your tongue against the inside of your bottom teeth and talk. I found this very easy to do with almost no sound distortion. Even sounds which normally require the tip of the tongue to move to the top of the mouth (/t/, /l/, /n/) are no problem--the middle of the tongue just comes up instead. For some reason "gluing" the tip of the tongue against the bottom teeth is much harder, but still intelligible after a little practice. But watch yourself in the mirror if you want some laughs. The facial compensations that I use make me look like I'm snarling. Vowels are quite easy either way. Mustn't there be real-time perceptual control for this to work? Seems so to me.--Gary

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Date: Thu, 27 Jun 91 10:07:47 +0200
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Oded Maler <Oded.Maler@IRISA.FR>
Subject: Re: Speech Feedback, Aphasia References

[From Oded Maler 910627]

(In reply to Gary (26) and Rick (25))

Incindently, I was reading last night some commentaries on the paper "What muscle variable(s) does the nervous system control in limb movements?" (R.B. Stein, BBS, 5, 535-577, 1982) and one of them, J.H. Abbs, "A speech-motor-system perspective on nervous-system-control varibles", 541-542, talk about similar experiments where, for example, some loads were applied to the jaw, and instead of compensation in order to keep the jaw position as needed for pronounciation, the compensation took place in the adjusted of the movements of the lips.

--Oded

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Date: Thu, 27 Jun 91 06:12:46 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: LInguistics etc.

[From Bill Powers (910626.0800)]

Bruce Nevin --

I should have said this before, but I am grateful to you for devoting so much of your attention to CT subjects, and for being patient with those of us who are unranked amateurs in linguistics. When I make proposals about how language works, I'm strictly speaking control theory, not linguistics -- I'm saying "Judging from my understanding of control theory, here's how it might work." I'm not trying to persuade you that the CT version is right. I hope that one day, when you have assimilated the basic relationships of control theory and we have learned enough from you to enable useful communication, you will start offering some detailed corrections of the CT model as it applies to linguistics.

The discussions of linguistic problems that have appeared on this net in the last few months represent the first time that people with real knowledge in this area have also committed themselves to becoming knowledgeable in control theory. Linguists have hooked up with control theory in the past, but aside from one person in Bangkok who's not on the net, have veered off again. It's very hard to get people who are proficient in any behavioral/cognitive discipline to LEARN control theory well enough to contribute to the model; usually they either ignore it or adapt it to fit the style and beliefs of their discipline -- and of course miss the main points as a result.

> As Bateson's writings occasionally note, mammals place a great
>deal of importance on relationship, and for reasons that he discusses
>and that I have mentioned previously (regarding things like authenticity
>and sincerity) most of this communication is *necessarily* nonverbal,
>including nonverbal aspects of the use of language--how you say it as
>distinct from what the words literally mean, including things like
>vocabulary choice as well as obviously gestural things like tone of
>voice. (I suspect that most of what is of interest for the clinicians

>among us concerns the management of communication in this sense,
>including self-image, self-esteem, etc.)

>Your proposed communication scenario about an arrangement of coins
>attempts to exclude this sort of communication by making the
>relationship between the participants an institutionally controlled
>variable that is not of concern to them individually, but the two
>participants are likely still each unconsciously and continually to
>construe available perceptions so as to support an ongoing process of
>imagining the other and the relationship.

As a modeler, I try to start with simple problems before tackling the hard ones. The principles you learn from solving a simple problem may require revision when you enlarge the scope of the model, but they're not likely to be negated. They might not be discovered at all if you start with too complex a problem. Description of a visual pattern is a relatively simple problem precisely because it doesn't introduce the full complexity of real communication. In fact, so far I haven't even tried to complete the picture by analyzing the recipient of the message -- everything in yesterday's post is still concerned with how the speaker can control the production of sounds that do in fact mean TO THE SPEAKER what is intended.

The CT version of language *production* (or production of any behavior) does not depend on a tree that elaborates in the downward direction, but on *perception* that builds from the bottom up. From what little I know of linguistics, it's assumed by most linguists that the higher-level stuff comes first, and is then fanned out level by level into more detailed aspects of language until you finally come to articulation, which is the output. The HCT model shows how the same result can be achieved through a perceptual tree that converges in the upward direction, with the downgoing commands being determined mostly by errors.

Similarly, I study tracking behavior because the principles of control can be seen very clearly there -- even though the complexities of arm dynamics are ignored, and the model would be of little use in explaining the strategies of an Olympic slalom racer. My scenario certainly isn't intended to "explain communication." It's intended only to develop some notion of the way in which a simple experience can be transmitted from one brain to another using verbal communication. I never supposed that we would stop at this level of detail -- only that we would pause here long enough to establish a few basic ideas.

I certainly appreciate the points you raise above in citing Bateson. Those are aspects of communication we will have to handle when we are ready. But I am also aware that there is a tendency among Batesons to avoid getting down into the machinery to ask how all these high-level phenomena can possibly occur. The high-level picture you get of what is going on may be more dependent on the assumed detailed model than is apparent -- an example being the difference between a conventional top-down conception of language production, based on the standard neurological model, and the control-theoretic conception of control of input at every level, a new conception of how the nervous system works.

>If for every string of n phonemes heard in the present utterance you
>refer to a count of the number of n+1th phonemes that have followed
>those n phonemes in prior utterances, you find that the number of
>possible next-successors drops as n gets larger, and then suddenly rises
>to a peak at or near the full inventory of phonemes. These peaks
>concide well with morpheme boundaries ...

This is hot stuff and I definitely want the details. I've heard of "transition probabilities" before, but only in contexts where it seemed that they were being used in an attempt to explain language without reference to meaning. This looks like a viable way to distinguish levels of perception -- maybe, once you tell us more of the data, we can come up with some control-system experiments that will poke into these phenomena a little further.

>Thus, from expectation based on prior experience you can determine which
>strings (simple configurations) form elements on the next-higher level
>of construction. Dependencies among morphemes are not so simple as
>among phonemes, but in those dependencies resides the "verbal
>information" that we are concerned with.

We may get into a theoretical controversy over this proposal (good, not bad). I get the sense of an attempt to understand the organization strictly from analyzing the words and their relations to each other. I'm going to argue that the perceptions meant by the words would show relations EVEN WITHOUT THE WORDS. That is, I'm going to propose strongly that the structure of the word-relationships FOLLOWS the structure of the nonverbal perceptual relationships rather than creating it. My proposition implies that a person can control the nonverbal relationships without the use of language (at least some of them -- the ones that aren't themselves language phenomena).

Fortunately, at the level of phonemes and morphemes we're talking about the lower levels of control, where we know how to make models and do experiments pretty well. What we need is an experiment that involves controlled perceptual variables that we know how to measure, yet relates to language at this level. We may have to scratch around a little to find something suitable, but maybe we'll get lucky.

First, though, let's see if there is actually any divergence of opinion here. When you say "verbal information," maybe you're not talking about meaning, but just information about the words themselves. There would only be a controversy here if you're saying that words HAVE meanings, or that meanings are somehow "in" the words. (see later -- I'm commenting piecemeal here).

>Acoustic feedback is too late for controlling pronunciation of the
>current word.

The famous delayed auditory feedback experiments show that pronunciation can be severely affected by 300 milliseconds of delay or less. This is about the delay-time (250 millisconds) that I associate with configuration control. This level, I would guess, corresponds to the level where the units of language perception are phonemes.

On the other hand, the kinds of impairment I know about have to do with getting stuck on a phoneme (dragging it out or perseverating) or just halting. This suggests that the transition level is the one involved -- it works somewhat slower than the configuration level, but I don't know how much slower (Rick?). I think we can be pretty sure that auditory control at the transition-level and above is real-time.

I think that phoneme control is also real-time, but it may depend more on kinesthetic/tactile feedback than on auditory feedback. This is what we've taken lately to calling "instrumental control;" control of a variable in one hierarchical tree by use of control systems in a different tree. Kinesthetic control of configuration is definitely fast enough for real-time control: the bandwidth of continuous control is around 3 Hz for a low-mass system like a finger, maybe higher for a tongue. The second-order (sensation-controlling; formants?) systems are even faster -- even using a whole arm as the actuator you can get response-times of 150 milliseconds (to a touch), implying bandwidths of at least 5 or 6 Hz. And of course by exaggerating the swings of the reference signals you can drive these systems a lot faster (at the expense of the ability to resist sudden disturbances -- control of the fingers during very fast piano playing is pretty poor).

Don't believe everything you read about "feedback is too slow." Most of those statements are qualitative and aren't based on any real knowledge of how fast control systems can actually work. A properly-designed control system is ALWAYS faster than an open-loop system built of the same components. The reason is that you can provide high amplification factors that would make the open-loop system grossly overreact, and throw away the excess gain with negative feedback. The result is a frequency response that is flat over a much wider bandwidth than is inherent in the "forward" part of the loop. H. S. Black discovered this in 1927. For an open-loop system to provide the same amount of response at high frequencies, it would have to have far too much gain at low frequencies or steady-state; if the steady-state response is correct, the high-frequency response will be very poor. Open-loop systems depend on calibration. The only way around this problem would be to insert a filter that reduces the input drive at low frequencies -- and the best you could achieve by this ad-hoc method would be to match the performance of the closed-loop system.

>I imagine cutting off acoustic feedback by playing white noise over
>headphones, and recording the reading of a text. Speaking in a very
>noisy environment, one may not even be aware of intra-cranial vibrations
>reaching the eardrum (though it is difficult for me to say whether lack
>of awareness implies lack of perception sufficient for control). Would
>pronunciation "drift" over time? Would it become over-precise sounding?
>I think of a deaf friend whose pronunciation is quite wide of our shared
>acoustic targets, but whose articulations are more available for "lip-
>reading" than those of ordinary speakers.

Don't imagine it; do it. This kind of question is easy to answer experimentally. Anybody have the equipment and some time? The point about deaf people is highly pertinent -- although it may relate more to the initial learning than to execution. If the auditory-control systems aren't working, there's nothing to send appropriate reference signals to

the kinesthetic articulation-control systems as a way of controlling the sounds, is there? So the articulation-control systems wouldn't have a chance to be used even though they could work perfectly well. Congenitally-deaf people can't really talk at all (the best they achieve is intelligibility, if that). But people who become deaf later in life CAN speak almost normally. The kinesthetic control systems became organized appropriately, so speech can be based on kinesthetic control run open-loop as the output function of a higher system. I would suppose that with time, the quality of speech would deteriorate, but I don't know.

>You do not know what "a meaning" is. If you use the word "translate"
>you metaphorically at least presume that meanings subsist in a form
>something like that of language itself, a metalanguage for representing
>meanings.

Yes I do. I do not presume that meanings subsist in a form something like that of language itself. I presume that meanings are just ordinary perceptions, the kind that are there whether we refer to them with words or not. No special metalanguage is necessary to handle them: they are simply the world we experience. Words are pointers to experiences (some of which, of course, are other words). Have I used the word "translate" in this context? I don't think so.

>... there is no need to use anything other than the language itself to
>represent its meanings...

Oh, yeah? What is the meaning of "beside?" How about "purple?" What about "after?" There are words, of course, whose meaning is entirely given by definitions in other words, but these are words that have no experiential meaning, and whose significance is at least debatable. All really useful words point to some direct experience at some level of perception. When you define a word using other words, eventually you have to arrive at terms that rely on experience -- or you just have an empty trip around the dictionary. When you control the meaning of "cursor position" relative to the meaning of "target position" you don't need any words to do this. You control the meanings -- the perceptions -- directly.

Even a word like "and" is meaningless if you don't have the ability to perceive the relationship of logical conjunction (and this is also true of "logical conjunction."

>Meaning resides in the redundancies of the language itself.

Balderdash. There, I said it. Do your worst.

Happy Birthday to your little girl. I hope that my understanding of "little girl" is not based just on the redundancies of language itself.

Oded Maler --

>... as someone who is critically engaged in standard science (hoping to
>get a tenure position sometime, somwhere), I can see all the
>defficiencies in the system, but I can see as well the provinciality,
>the lack of broader perspective, awarenwss to current research etc. that

>might characterize some members of commmunities like yours.

It's not as bad as it looks. Most of us are, in fact, participating in "standard science," and have degrees and all that (except me). Control theory is "standard science." We're not a bunch of hippies who march around with signs saying "Down with Science." Don't conclude that there is a "lack of broader perspective" just because there is strong disagreement with some of the aspects of standard science, and failure to cite current research. Very often the current research that isn't cited has actually been looked at and found uninteresting. Of course I am guilty as accused, but others in the CSG know what's going on. They just don't believe most of it.

>As for your theory, I admit that I have not yet understood it deeply,
>and I don't have access to your `73 book (I convinced our local lib to
>order your other book ["the cheaper.."] but it takes time).

It would be a good idea to understand the theory before you decide that it's part of a guerilla movement searching for "some meaning in life."

But we love you anyway. Peace.

P.S. I would like to get your paper on the worm. My address is
   Bill Powers
   P.O. Box 2566
   Durango, CO 81302-2566
   USA

Best regards to all,

Bill

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Date: Thu, 27 Jun 91 09:08:03 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: LInguistics etc.

Bill Powers (910627)--

>The famous delayed auditory feedback experiments show that pronunciation
>can be severely affected by 300 milliseconds of delay or less. This is
>about the delay-time (250 millisconds) that I associate with
>configuration control. This level, I would guess, corresponds to the
>level where the units of language perception are phonemes.

Actually, that's nearer the syllable rate, or about 1/3 to 1/4 the phoneme rate.

>
>On the other hand, the kinds of impairment I know about have to do with
>getting stuck on a phoneme (dragging it out or perseverating) or just
>halting. This suggests that the transition level is the one involved --
>it works somewhat slower than the configuration level, but I don't know
>how much slower (Rick?). I think we can be pretty sure that auditory

>control at the transition-level and above is real-time.

It's not clear what corresponds to configuration or transition here. If the configuration is articulator configuration, then the transitions have a time-scale of about 10 msec, which just happens to be a rate of change to shich the auditory system is sensitive. We can detect reversals of sequence of sounds if they are separated by 10 msec or less much better than we can if they are separated by 30 msec to (?)80 msec. I do not think the phoneme transitions are a controlled perception, but as Bill mentions in his 73 book, they are the results of primary (first or perhaps second-order) motion sensors.
In production, I think it likely that the behaviour is very similar to that of the finger movements of a skilled pianist playing a well-learned piece--open-loop and controlled largely by the mass-viscosity-force relationships in the articulators. The parameters of that system are what I think are controlled, and the control is not fast, as Gary's demonstrations of constraining his articulators show.
>
>I think that phoneme control is also real-time, but it may depend more on
>kinesthetic/tactile feedback than on auditory feedback. This is what
>we've taken lately to calling "instrumental control;" control of a
>variable in one hierarchical tree by use of control systems in a
>different tree. Kinesthetic control of configuration is definitely fast
>enough for real-time control: the bandwidth of continuous control is
>around 3 Hz for a low-mass system like a finger, maybe higher for a
>tongue. The second-order (sensation-controlling; formants?) systems are
>even faster -- even using a whole arm as the actuator you can get
>response-times of 150 milliseconds (to a touch), implying bandwidths of
>at least 5 or 6 Hz. And of course by exaggerating the swings of the
>reference signals you can drive these systems a lot faster (at the
>expense of the ability to resist sudden disturbances -- control of the
>fingers during very fast piano playing is pretty poor).

I agree that control can be fast, but I do not agree that it can be fast enough to control phonemes in real time. The rates suggested here argue that one might be able to control syllables in real time, but the destabilizing
effects of delay make even this dubious. But, as I will ask in a separate posting, are the rates mentioned here really limiting rates?

Martin Taylor
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Date: Thu, 27 Jun 91 09:33:44 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Speed of skilled behaviour

Highly skilled behaviour seems to have different characteristics from simply trained behaviour, and it has different perceptual characteristics as well. I was going to ask this question earlier, but Bill Powers' comments on feedback speeds and the effective bandwidth of feedback systems prompts me to ask it now.

Some thirty years ago, I was a highly skilled cricketer, who fielded in a

position called "gully". As there is no equivalent on a baseball field, I will set the stage. Cricket (to a zero-order approximation) may be thought of as baseball played in a 360 degree field instead of a 90 degree arc, and in which the batter runs to the pitcher's mound rather than around four bases. The fielding position called "gully" is at an angle of about 120 degrees from the pitcher, on the "first-base" side for a right-handed batter. The bat is flat-faced, unlike the round baseball bat, and a good part of the skill of batting is to hit the ball hard in directions that would be foul balls in baseball. Hence, the ball is often hit fast through the gully position, and I have computed that the time from hit until the ball passes the fielder is often under 300 msec, sometimes under 200 msec. It may also be much longer, if the ball arrives from a mis-hit from the edge of the bat. The fielder therefore has to judge not only the direction but the timing, in order to be able to dive and catch the ball and not overshoot it. There is some possibility of predicting the general area the ball is likely to go, by watching the batsman's movements in the last 500 msec before the hit, but that prediction is unlikely to get you closer than one metre to the ball's actual position when it passes the fielder. And the fielders in cricket have no gloves and the ball is harder though the same size as a baseball. So a catch is a pretty precise act. An error of two cm leads to a missed catch and probably a bruised hand.

It is indisputable that people can perform this task, which sounds impossible on the face of it. Within the 300 msec from the hit, a highly skilled fielder can make a step and dive full length to catch the ball, and be most annoyed with himself if he fails to catch it. I used to ba able to do this myself, and I want here to report the perceptual characteristics of the performance.

There are two completely distinct perceptual modes: one I would call "fast mode", the other "slow mode." In both, there is the anticipation of what the batsman will do, and if the ball is going to come off the edge of the bat rather than from a deliberate hit in that direction, it is possible to see how much of the ball is obscured by the bat at the moment of contact, which allows (consciously) the prediction of where it will go, in a general way. Thereafter the two modes differ drastically. In fast mode, there is NO further conscious perception intil one finds oneself on the ground, perhaps a couple of metres away, with the ball in hand. One says to oneself "I must have caught it, because how else could it have got into my hand?" But it is the other players who see the catch, not the fielder who makes the catch. In slow mode, on the other hand, the ball slowly drifts from the bat, and one easily and with premeditation goes to where it is, and catches it (slowly is, of course, a purely perceptual term, because the ball is physically travelling very fast.) This effect seems to be observable from outside, as on one occasion
after I made such a catch, the wicket-keeper (analogue of baseball catcher) came up and said to me "That came very slowly, didn't it." I answered that it had, and truly I thought that it had not been a quick catch, but he said "No it didn't. It was BLOODY quick."

I'm sure there must be a PCT explanation for these differing perceptual modes, and I'd like to hear it because I can't think of one for myself. But also to the point, it is clear that these large-scale but precise movements are controlled, and controlled at a MUCH higher rate than any laboratory studies of reaction or tracking behaviour would suggest to be possible. This kind

of performance, which is replicated in skilled sports of many kinds, seems to tap different kinds of control, in which some of the functions given to higher systems in the early stages are passed down to lower systems when the skill level is high. And one can lose the skill by failure to practice it.

So, two questions: (1) A PCT explanation for the two perceptual modes, and (2) PCT theory as discussed here has a pretty fixed set of control levels, with names; can that be reconciled with the kinds of changes in control rates that come with great skill?

Martin Taylor

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Date: Thu, 27 Jun 91 09:37:55 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Converted from PROFS to RFC822 format by PUMP V2.2X
From: "Bill CUNNINGHAM - ATCD-GI (804" <CUNNINGB%MON1@LEAV-EMH.ARMY.MIL>
Subject: Sunday Sampling -- The tyranny of words

Bill Powers' (910625.0800) includes response to my 910624, which did not get relayed on the net--due to temporary insanity by my mailer. My original post forwarded next under. Sorry for hassle. Am digesting backlog of e-mail.

Bill Cunningham

*** Forwarding note from CUNNINGB--MON1 06/23/91 16:10 ***
To: CSG-L --CMSNAMES

FROM: Bill CUNNINGHAM - ATCD-GI (804) 727-3441
SUBJECT: Sunday Sampling -- The tyranny of words

Bill Cunningham (910624.1500)

Bill Powers--The check's in the mail.

My enforced absence has an advantage--considering several comments in one batch.

The 3 coin scenario is very helpful in conjuring up the image seen by the sender. The descriptions of control for word execution (all the components of physically speaking) are okay, but I think they take us away from the core of problem. The choice to communicate by spoken word is purely arbitrary. We could just as easily have chosen handwritten or typewritten words with their attendant muscular control systems. We could also have selected several nonverbal media or a hybrid. Any principles uncovered should apply to other media.

I'm still stuck with the sender deciding what to say. Martin Taylor's comment that the sender must control for a model (or image) of the receiver makes tremendous sense, but I also agree we can set that aside for the moment. I had hoped to avoid issue of whether a human can think without words, but find
that may be an essential part of the problem. As I imagine the scene with 3 coins, I do go through a sequence much like that described. What I sense

strongly is control for the choice of the right word. Vocabulary comes in increments (words). If I control for the the right word, there is almost certainly error. Now, does my perception of what I see become the reference, against which the word selection must adjust? If so, the discrete nature of individual words requires: adding more words to modify the original choice without changing its original meaning taken individually; or, modifying the meaning of the words to fit the reference. A hybrid is possible. I call the first technique the Martin Taylor solution and the second the Charles Dodson solution. By the way, the earlier admonition for me to stay away from terms in
quotation marks is another way of saying the Taylor solution is preferred.
There is another possibility. If I regard the vocabulary set as the reference,
then the image has to adjust to meet the choices available (or already made).
We can certainly point to some people who believe their own rhetoric.
Thus the tyranny of words.

The words themselves are symbols, sometimes for simple objects (configuration) and sometimes for complex principles. Talking to nobody but myself (I win 50% of these arguments), I am conscious of the dialogic process where both the image and the description of it both seem to adjust. At least they converge and my comfort level is a function their difference. If the sender in our problem has gone through this process to resolve what he sees and how he says it to himself, his next step would be open mouth and start talking--using the control loops discussed by Bruce Nevin and Bill Powers. Continuing with the scenario, the sender now has the problem of fitting his words to his perception of the receiver. The discussion in this paragraph is behind my original proposal that part of the vocabulary used by the sender would serve to assist the receiver to conduct a mirror-image word-to-image control process.
My original point was that specifying identical levels for both parties was an essential part of the process.

Any more, and I'll enter into the issue of coupled control systems. Other than
to say that has to be a voluntary process subject to influence (not control), I
had better quit.

The original question dealt with how the receiver perceives
something without personal experience. Right now, we're still
working on the sender. What little we've said about the receiver
has assumed (near) identical organization. We will still have to
deal with how the receiver creates a new image of something never before
perceived. Importing reference may be the wrong term, but the receiver's
reference set will have changed.

On the road again.

Bill Cunningham

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Date: Thu, 27 Jun 91 07:51:59 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>

Subject: Linguistics: demos

[From Bill Powers]

Bruce Nevin (910626) --

>The "white noise" sort of test, depriving the participant of acoustic
>feedback, might indicate that we can use kinesthetic and tactile
>perceptions as a backup, but that control with them is less precise
>and liable to "drift" if we go too long without hearing ourselves
>speak.

Slight refinement: the kinesthetic systems aren't "backup;" they are used to control articulation even WITH auditory feedback. Hierarchical control. But the auditory control system can IMAGINE the auditory words at the configuration level, thus satisfying all higher systems, sending its output to the kinesthetic control system reference inputs as usual but providing its own internal feedback. It's the perception of the reference signals (via the imagination connection) that slowly drifts. As a result of compensating for the drifing imagined sound, the outputs become different too, creating different articulations without the person knowing it. The person still imagines that the right sound is being produced.

I get a very strong sense of the imagined auditory feedback by just mouthing "hello" without any sound (not breathing in or out). I don't actually hear sounds (no intensity or sensation) but the mouthed "hello" is still very plain to me as an imagined auditory experience. Does this work for anyone else? (Of course any other words will do -- that's just the one I tried a moment ago). It's the same imagined auditory experience I get from READING "hello." (Come to think of it, there's also an imagined kinesthetic experience in reading "hello" or "hello?" Even more so with "rouge" (in French). Next thing, I'll be moving my lips when I read).

Gary Cziko (910626) --

>For example, keep the tip of your tongue against the inside of your
>bottom teeth and talk. I found this very easy to do with almost no sound
>distortion.

Brilliant!

Yes, it's easy! There is some distortion of the final result, but I'll bet that if you used Crazy Glue to keep the tip of your tongue fastened to your bottom teeth for a month, you'd be talking essentially normally at the end. What you would be saying is another matter. Any volunteers?

>For some reason "gluing" the tip of the tongue against the bottom teeth
>is much harder, but still intelligible after a little practice.

I presume you meant "upper teeth." Yes, it's harder -- you have to use the lateral margins of the tongue to make a "t" and the vowels get distorted and sound (in my mouth anyway) juicy. But it's still quite intelligible.

I just love this kind of simple portable demonstration. It's a complete refutation of the idea that articulation consists of producing a preset pattern of motor outputs, and anyone can do it in two seconds. Absolutely ingenious, Gary.

Oded Maler (910627) --

>... J.H. Abbs, "A speech-motor-system perspective on nervous-system-
>control varibles", 541-542, talk about similar experiments where, for
>example, some loads were applied to the jaw, and instead of compensation
>in order to keep the jaw position as needed for pronounciation, the
>compensation took place in the adjusted of the movements of the lips.

Another great example! I read that article in BBS, and my concluding thought was that if Stein had understood hierarchical control the article would have been much less confused. But I missed that comment. Stein gave a beautiful example of how to screw up control theory by looking for control of "muscle variables" -- output.

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Date: Thu, 27 Jun 91 12:57:10 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: O tell me where is meaning bred?

[From Bruce Nevin Thu Jun 27 09:17:32 EDT 1991]

Let me register my gratitude for having connected with the CSG.

Let me also comment on possible problems of tone. Like a guy out in the woods chopping down trees, wearing headphones and listening by walkie-talkie to conversation going on in a nearby house. Occasionally throws down the axe, runs over to the house and shouts a breathless contribution at about 3x the volume of everybody else, chopped & elliptical diction, then races back. I suspect I may sound brusque or pontifical, I know much of what I have contributed was not as well thought out as I would like, and there is so much in the replies and in all the threads of conversation that call for much more reflection than I have given. But it is incredibly nourishing, and if you can put up with my dashing back and forth and shouting through the window, I am extremely grateful.

Bill Powers (910626.0800)

Starting at the end

>>... there is no need to use anything other than the language itself to
>>represent its meanings...

>Oh, yeah? What is the meaning of "beside?" How about "purple?" . . .
>When you control the meaning of "cursor position"
>relative to the meaning of "target position" you don't need any words to
>do this. You control the meanings -- the perceptions -- directly.

I said "represent". Does that help?

You are using "meaning" as synonymous with perceptions. The perceptions are just that, perceptions. If you want to call them meanings, then (with a reservation reverted to presently) I think we are in violent agreement. But the perceptions are not representations of the meanings in language. I'm not even sure you would say that perceptions are representations of the things perceived, it seems to me that from an HCT perspective the things in the world do not themselves exist for us, only our perceptions of them exist for us (maybe only the perceptions of them that we have under control exist for us). Ding an sich and all that. But crank me back in if I'm going too far with this.

>Words are pointers to experiences

We are bumping against a traditional problem in the philosophical discussion of meaning: not all meanings are denotative or referential (no unicorns, no present king of France, no round square "out there"), but HCT may just get by all that by referring to experience (perceptions) rather than "objective" things and events apart from a perceiver, and by the fact that these perceptions include memory and imagination.

>Words are pointers to experiences (some of which, of course, are other >words).

Words that refer to other words are in the metalanguage. Many reductions depend upon a metalinguistic identification of one word occurrence with another (definite article, pronouns, reductions to zero). I don't think this is what you had in mind, so if you meant something more and I missed it and it matters please clarify.

>Words are pointers to experiences

I would say rather: words are experiences or perceptions and the dependencies among them are in some degree of correspondence with the dependencies among other experiences or perceptions that are not words. By dependency, I mean: if A occurs then B must also occur (or have occurred), or if a perception of class {A} occurs a perception of class {B} must also occur (or have occurred). I therefore also mean expectation. The dependency or expectation of cooccurrence is a perception on the level of relationships.

There is a further question whether having things (words and perceptions) in correspondence necessarily always means that the former points to the latter. In the case of memory and imagination, the mapping may well often go the other way--we stop attending to lower-level perceptions and run on automatic, that is, on expectations arising in part from words. Rationalization, as we all know, is one of the principle uses to which we put our capacity for reasoning.

>>Meaning resides in the redundancies of the language itself.

>Balderdash. There, I said it. Do your worst.

We are at cross-purposes (a lovely HCT concept, no?). I had thought you were talking about translation from language to some semantic representation, which would be a separate metalanguage whose ostensive purpose was representing meanings. It would not have been surprising if you were, since this is the standard approach in computational linguistics (typically, the query language of a relational database is called a "semantic representation" for a natural-language front-end), and the history of generative linguistics is littered with machinery of this sort.

>Have I used the word "translate"
>in this context? I don't think so.

Here is where you did in the prior message to which I was responding:

>Bill Powers (910621.2300)

>If the relationship is "A means B," we can call the A-
>perception the "symbol" and the B-perception the "meaning of the symbol."

>The point is to represent giving meaning to a word (or translating a word
>into a meaning) as a process of controlling a relationship. The basic
>reference signal is simply "find a meaning." When the missing element of
>the relationship is produced through lower-level control processes, the
>error is corrected: a meaning is requested, and a meaning is perceived.
>The perception that constitutes the missing element can then be passed on
>to higher systems, which can work with the meaning as well as with the
>symbol. I am aware, by the way, that this same function can be
>accomplished by content-addressed memory.

I know now that I should substitute "nonverbal perception" for "meaning" in the above passage, and I have less trouble with it. The remaining difficulty is because this account ignores the constitutive role of language in social reality and psychological reality, some would even claim in aspects of physical reality though I remain an agnostic among the true disbelievers there. I made the point above that the dependencies found among the elements of language are in loose correspondence with the dependencies found among other perceptions, and this correspondence is the basis for referential or denotative meaning. But perceptions (experiences) have meaning for us quite apart from our ability to name them and talk about them with words. You said as much, too:

>bad). I get the sense of an attempt to understand the organization
>strictly from analyzing the words and their relations to each other. I'm
>going to argue that the perceptions meant by the words would show
>relations EVEN WITHOUT THE WORDS. That is, I'm going to propose strongly
>that the structure of the word-relationships FOLLOWS the structure of the
>nonverbal perceptual relationships rather than creating it. My
>proposition implies that a person can control the nonverbal relationships
>without the use of language (at least some of them -- the ones that
>aren't themselves language phenomena).

The meaning of perceptions, I assert, resides in the dependencies that hold between them. In part depencencies on one level may be

constitutive of a single perception on another, and the higher-level perception may even then be taken as the "meaning" of the lower-level perceptions-in-relationship -- and (importantly) vice versa. Denotative meaning (in which words point to perceptions) are a special case of dependency among perceptions, the case in which elements of language are the perceptions on one side of the dependency and nonverbal perceptions are on the other.

We can always find words to talk about any perceptions to which we can bring our awareness, doing so can help us to retain and teach control of those perceptions and beyond that to establish some identified social significance for them, which in turn is perceptible through the many ways in which people may align themselves with or against a norm. All of this is apart from the nonverbal meaning we started with, and is itself partly nonverbal.

Now, again:

>>Meaning resides in the redundancies of the language itself.

You took this to mean that meaning *only* resides in the redundancies in language. Mea culpa, I was neither clear nor explicit, so focussed was I on rejecting the notion of a "semantic representation" apart from language on the one hand and nonverbal experience on the other.

Meaning also resides in the redundancies (dependencies) among nonverbal perceptions. Furthermore, the verbal or linguistic meanings of language would be empty if they did not correspond to corresponding nonverbal meanings (dependencies, redundancies, and associated expectations). In fact, they are empty when the correspondence lapses--as you are well aware, people often engage in verbalisms whose correspondence to nonverbal perceptions is tenuous at best. And communication fails when I cannot bring the dependencies among the words you say into correspondence with the dependencies among my remembered and present perceptions.

> I get the sense of an attempt to understand the organization
>strictly from analyzing the words and their relations to each other.

Historically, this is because linguistics didn't have HCT. Other available modes of psychological explanation were rejected in the 1930s, '40s, and '50s because they were inadequate. Moreover, there is validity in this precisely insofar as the dependencies in language are in correspondence to dependencies among other perceptions. (Whereas one could scarcely say that they corresponded to dependencies actually in the world, that claim when attempted ran into troubles real fast.)

> I'm
>going to argue that the perceptions meant by the words would show
>relations EVEN WITHOUT THE WORDS. That is, I'm going to propose strongly
>that the structure of the word-relationships FOLLOWS the structure of the
>nonverbal perceptual relationships rather than creating it. My
>proposition implies that a person can control the nonverbal relationships
>without the use of language (at least some of them -- the ones that

>aren't themselves language phenomena).

I agree except for my conviction that language does have a constitutive role in some cases, as suggested above. I have to believe that Whorf was on to something, that a person who uses Achumawi or Navajo all the time slices and dices the world of perception differently from an English or German speaker. Different things are obligatorily specified the way we specify gender with our pronouns, different things must be thought out and verbalized with awkward circumlocution the way we would have to explain a Navajo joke (losing the funniness of it). I can go into this more if it's terra incognita.

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>As a modeler, I try to start with simple problems before tackling the

Of course. I just wanted to make this explicit. And I was having trouble staying in focus (lots of dashing away to the woodpile and back to the window that day) so the intended development didn't happen or maybe I went too far with it because a more proper development was quite out of my reach. I know we have to simplify when we are designing models and experiments. But when we are evaluating results we must re-invoke the richer context in which communication actually and ineluctably takes place because there are just so many places for an experiment to leak, so many many redundancies that language users fall back on precisely when a situation presents a disturbance in some aspect of their use of language.

You're right about the top-down orientation of most of linguistics. But I think most of linguistics has had its head in a bag for the past 35 or 40 years, ever since Noam started marketing Zellig's ideas transmogrified by his own Rationalist commitments. For Chomsky and the Generativists (gee, sounds like a band), language is just too complicated for any child to learn it from experience, so there must be some biologically innate language- acquisition thingie in the brain that imposes much of the structure. But of course it's not language that's so complicated, only their description of it.

>This is hot stuff and I definitely want the details. I've heard of

Harris, Z. S. 1955. From phoneme to morpheme. _Language_ 31.2:190-222 [Was also in a Prentice-Hall student reprint.]

_____. 1967. Morpheme boundaries within words: report on a computer test. University of Pennsylvania Linguistics Department, _Transformations and Discourse Analysis Papers_ (TDAP) 73. [Out of print, doubtless.]

_____. 1968. _Mathematical Structures of Language_. New York: Wiley/Interscience. [Summarizes methods and results.]

_____. 1970. _Papers in structural and transformational linguistics_. H. Hiz, ed. Dordrecht: D. Reidel. [Includes reprints of the first two papers. Out of print, but may be in library. 1976 reissue less than

half the size omits the structural lx. papers, including the above.]

_____. 1990. _Language and Information_. New York: Columbia U. Press. [Based on Bampton Lectures for 1986, surveys full range of his theory of language in accessible form.]

I recently got John Wimbish at the Summer Institute of Linguistics (a missionary organization) interested in hooking a grad student up to this as a thesis topic for a program to do this on a PC. He's the author of a package called shoebox for managing linguistic databases, which I've begun using with my Achumawi data.

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I will be gone starting this afternoon until Monday the 8th. However, I'll be saving up the mail, and hopefully I'll catch up before too many more weeks go by. I don't think I'll be able to keep up this volume, at least not right away.

Thanks again,

Bruce Nevin
bn@ccb.bbn.com

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Date: Thu, 27 Jun 91 13:31:17 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: speed

Same thing happens in emergencies. Once hitchhiking I was riding inside a homemade camper on the back of a pickup (very homey, woodstove, stores of canned goods, etc.). Driver upset at a bee lost control so that the truck nosed into a bank on the right and overturned end for end. The swaying as the truck swerved was normally perceived. As soon as it started flipping I was disoriented but supporting myself against the roof as it turned, then crawling toward the door (now upside down) to open it. As I opened the door, I suddenly switched and was aware of moving frantically fast, whereas all during the flip itself and the successful landing in a crawling position on the inside of the roof was in seeming slow motion.

No specialized skill involved, here.

This is an experience known to many people. Movie directors sometimes simulate it. Compare bionic man on TV I think (I had no TV).

Bruce Nevin
bn@ccb.bbn.com

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Date: Thu, 27 Jun 91 14:13:49 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Haley <HALEY@SDNET.BITNET>
Subject: Linguisttics?

What about communication that depends on the absence of things?
Re: The following

o o o o o o o o

Can you arrange these eight circles to mean four?

See next page.

o o o
o o o
o o

Curt Lewin would love this.
Bill.

==========

Date: Thu, 27 Jun 91 13:37:59 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Articulaaation

Bill Powers (910627) says --

>I think that phoneme control is also real-time, but it may depend more on
>kinesthetic/tactile feedback than on auditory feedback. This is what
>we've taken lately to calling "instrumental control;" control of a
>variable in one hierarchical tree by use of control systems in a
>different tree. Kinesthetic control of configuration is definitely fast
>enough for real-time control: the bandwidth of continuous control is
>around 3 Hz for a low-mass system like a finger, maybe higher for a
>tongue. The second-order (sensation-controlling; formants?) systems are
>even faster -- even using a whole arm as the actuator you can get
>response-times of 150 milliseconds (to a touch), implying bandwidths of
>at least 5 or 6 Hz. And of course by exaggerating the swings of the
>reference signals you can drive these systems a lot faster (at the
>expense of the ability to resist sudden disturbances -- control of the
>fingers during very fast piano playing is pretty poor).

and Martin Taylor (910627) responds:

>>I agree that control can be fast, but I do not agree that it can be fast
>>enough to control phonemes in real time. The rates suggested here argue
>>that one might be able to control syllables in real time, but the destabil-
>>izing effects of delay make even this dubious. But, as I will ask in a
>>separate posting, are the rates mentioned here really limiting rates?

Note that Bill was talking about control of the acoustic variable as a side-effect of kinesthetic/tactile control. It may be true that a disturbance to the acoustic signal could not be corrected in real time. But a disturbance to an articulator (that WOULD produce a change in the acoustic signal) can be corrected fast enough to PREVENT degrdation of the acoustic input; control of the acoustic variable is a SIDE EFFECT of control of the articulation. Evidence for the ability of articulators to compensate for disturbance during speech was given in the Abbs study (mentioned by Oded Maler) and in the Gary demo. Bill's augmentation of the Gary demo (don't make sound while you articulate) shows how well you can control articulation without any auditory feedback. I have combined the Gary and the Bill demo so that I talk silently while placing my tongue in various odd positions in my mouth. Not only can I imagine what "would" be heard if I voiced these words, I can tell what my "compensated" version of a word would sound like; I think I can tell how well, for example, I am pronouncing "hello" with my tongue stuck against the back of my top front teeth -- even though I can hear nothing.

I agree that the ability to control variable aspects of the acoustic signal itself may not be possible below the "syllable" (event?) level. This could be tested by introducing disturbances directly to the acoustic signal and seeing if the subject can compensate for them by varying his/her own acoustic output. I think that the "distortion of feedback" studies should give some hint about what aspects of the acoustic signal can be controlled. I just think the studies of this sort that have been done are simple minded; they have not taken the perspective of looking for a controlled acoustic variable. Instead, they have just looked for gross effects of some acoustic stimulus (like noise level) on behavior (like speech level). If the relationship found in the latter case is not strong, they look for another stimulus (rather than trying to hone in on a controlled variable).

In a separate post Martin Taylor (910627) describes the difficulty of catching a criket ball and asks:

>So, two questions: (1) A PCT explanation for the two perceptual modes, and
>(2) PCT theory as discussed here has a pretty fixed set of control levels,
>with names; can that be reconciled with the kinds of changes in control rates
>that come with great skill?

I'll give the short answer -- 1) your perceptual modes sound like two different variables being controlled -- mode 1 sounds like an event, mode 2 sounds like a relationship 2) the levels are hypothetical and are fixed only until tested and rejected. Rejection hasn't happen yet -- but it could. However, changes in control rates with "skill" are easily handled in the context of the existing model; you control faster when you control at a different (higher or lower) level. See the study by Robertson and Glines

(referred to in the paper I sent) for an example of "faster" control that results from learning to control a variable as a higher level perception. Your own example of learning to control the "catching" event shows that learning to perceive the appropriate lower level variable can also lead to apparent "faster" behavior. (Incidentally, the same kind of perceptual control that you used for criket is also used in baseball -- which also happens to be a REAL sport (just kidding)).

Hasta Luego

****

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Date: Thu, 27 Jun 91 19:59:56 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: speed

Bruce Nevin (910627)--

Same thing happens in emergencies. Once hitchhiking I was riding inside a homemade camper on the back of a pickup (very homey, woodstove, stores of canned goods, etc.). Driver upset at a bee lost control so that the truck nosed into a bank on the right and overturned end for end. The swaying as the truck swerved was normally perceived. As soon as it started flipping I was disoriented but supporting myself against the roof as it turned, then crawling toward the door (now upside down) to open it. As I opened the door, I suddenly switched and was aware of moving frantically fast, whereas all during the flip itself and the successful landing in a crawling position on the inside of the roof was in seeming slow motion.

No specialized skill involved, here.

This is an experience known to many people. Movie directors sometimes simulate it. Compare bionic man on TV I think (I had no TV).

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Yes, I meant to mention that, but forgot. I think it is the same phenomenon as the "slow mode" I mentioned. I experienced exactly the same kind of thing when a car failed to stop at a stop sign and hit my bike flush, knocking me into a four-lane road. It seemed to take about 5 minutes of careful thought and motion before I hit the road in such a way as to take me away from the car's path, but not into the way of any other traffic.

The problem remains more or less as I posed it, however. How does PCT account for this order-of-magnitude change in the speed of perception under these special conditions.

Martin Taylor
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Date: Thu, 27 Jun 91 21:20:48 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: belated reply to Martin Taylor

[From Bruce Nevin Thu Jun 27 20:49:48 EDT 1991]

Since I'm still waiting for Sarah and the kids to arrive here in Cambridge from Gloucester so we can begin the 16-hour drive to Chicago tonight (ouch!), a few more words of catch-up to prior mail.

Martin Taylor wrote (June 21):

>For communication . . . the Speaker must in some way model what the
>Listener will do with the speech.

I would say also "has done with prior speech".

>. . . "words describe what he is seeing" . . . only insofar as they can
>evoke an intended effect in the hearer (who may be the Speaker) in the
>situational context that the Speaker believes the Hearer to experience.
>. . . the reconstruction of intention of a communication depends on what
>is already known to the hearer. Words, therefore, cannot "mean"
>anything out of the context in which they are used, and furthermore,
>someone who is not party to a conversation cannot be sure what the words
>"mean" to those who are participating in the dialog.

Typically, the speaker is more intent on her purposes than on the hearer's. Words do "mean" something outside of the immediate context of current use, and that has everything to do with remembered prior contexts of use (or generalizations thereof) by which the hearer construes what the speaker probably means. "Cannot be sure"? Even for participants in the dialog there is no certainty (though one or both may feel sure), and allowing a margin of indeterminacy and even ambiguity someone who is not party to a conversation can know something of what is going on and comes to know increasingly more as she listens longer. That is an important part of what it is to be a participant even if only a silently listening participant. The other part of course is making a counter-contribution so that some reciprocal correction of perceptions and understandings can take place.

Martin again says (same date, later message):

>Not only do speakers control for the distinctness of words, they
>control for distinctness at all levels. for the most part,
>communication is the attempt to select in the mind of the partner a
>satisfactory configuration out of all the configurations that the
>partner's mind can produce.

Neither listener nor speaker is typically aware of ambiguity. Ambiguity results almost entirely from the system of reductions (pronouns,

zeroings, other reduced forms of morphemes) that make speech less redundant. Attention is focused on the word dependencies, with the missing or reduced material construed as present, and in that form ambiguities of this most common type do not appear. The reductions actually lessen the contrastiveness in overt speech, that is, the contrastiveness that the speaker (perhaps wrongly) perceives as redundant.

>The control is of the partner's behaviour
>in response to the communication.
. . .
>What is being controlled for is the listener's discrimination among
>possibilities perceived by the talker as available.

It is not until the listener and speaker reverse roles that the first speaker has something to control, and that is the relation between what she said and what the erstwhile listener now appears to be saying. The listener's discrimination is not controllable by the speaker, and as I have said I don't believe the speaker is even aware of the range of possibilities (the ambiguities) in what she is saying.

Reaction to misunderstanding reflects a disturbance. One may control for congruity of the response to one's intention, inclusion of partial paraphrase explicitly or by presupposition and the like.

I think they're here now.

Be well,

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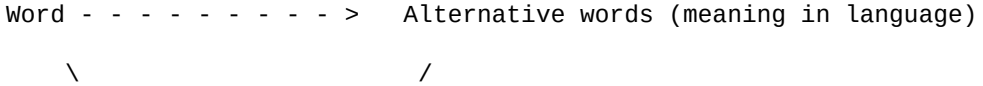
Date: Sat, 29 Jun 91 08:24:54 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Linguistics; misc

[From Bill Powers (910628.0800)]

Bruce Nevin (910627) --

>But the perceptions are not representations of the meanings in language.

If you mean



nonlinquistic perception

... then we have meshed.

This is similar to paraphrasing. In order to know that one word-structure is a paraphrase of another, you have to go through the above linkage -- i.e., the original wording and the new wording must both point to the same perception.

>...it seems to me that from an HCT perspective the things in the world >do not themselves exist for us, only our perceptions of them exist for >us (maybe only the perceptions of them that we have under control exist >for us).

It (first part) seems that way to me, too. But I think we experience perceptions of many things we don't control (moonrise, etc.). It's possible that there is control associated with perceptions that aren't controlled, in the sense that we (maybe) select reference signals for them that match them, so the way the uncontrolled world IS becomes the way it SHOULD BE (in perception, of course). I don't know where to go with that.

>We are bumping against a traditional problem in the philosophical >discussion of meaning: not all meanings are denotative or referential >(no unicorns, no present king of France, no round square "out there"), >but HCT may just get by all that by referring to experience >(perceptions) rather than "objective" things and events apart from a >perceiver, and by the fact that these perceptions include memory and >imagination.

Pretty soon you're not going to need me at all.

>Words that refer to other words are in the metalanguage. Many >reductions depend upon a metalinguistic identification of one word >occurrence with another (definite article, pronouns, reductions to >zero).

"Metalanguage" is a word. "In the metalanguage" is a phrase denoting a relationship. "... a metalinguistic identification of one word occurrence with another" is a (?) categorizing process (this is a that). I don't know if this suggests anything useful.

>>Words are pointers to experiences

>I would say rather: words are experiences or perceptions and the >dependencies among them are in some degree of correspondence with the <dependencies among other experiences or perceptions that are not words. <By dependency, I mean: if A occurs then B must also occur (or have >occurred), or if a perception of class {A} occurs a perception of class >{B} must also occur (or have occurred).

This (and ff in the post) is a big aha for me at two levels.

The first level, which has occurred to me before, concerns perceptions just as perceptions. Perceptual signals, being trains of impulses, are

all alike (to a first approximation, anyway). This leads to a problem of perceived "quality" (by which I don't mean sensations). Two perceptions of the same level are clearly different from each other -- "on" is clearly different from "beside" -- yet when you examine them closely you can't find what that difference is. They're just different. I guess this is called the "place" idea of perception. The conclusion is that perceptions have separate significance only in the context of other perceptions (Paul Churchland's "network theory of knowledge.") Perhaps this means that perceptions of any level take on these experiential qualities only when a higher level is receiving them (else how could their mere simultaneous existence have any significance?).

The second level is the one you have just supplied. Language is just a subset of all perceptions. Words, too, take on significance only in the context of other perceptions, some of which are words. This completely erases the artifical boundary between perceptions classifiable as words and those we call "nonverbal" (except for the fact that we can't speak or write nonverbal perceptions). The same hierarchy of perception and control, with the same general structure, is working in any case. "... dependencies among [words] are in some degree of correspondence with the dependencies among other experiences or perceptions that are not words" because the dependencies in both cases are the SAME and are imposed by the SAME perceptual apparatus. I can't take this any further, but I suspect that you can.

>There is a further question whether having things (words and
>perceptions) in correspondence necessarily always means that the former
>points to the latter.

I think it works both ways: given the object, the word is its meaning; given the word, the object is its meaning. But these connections may have to be learned separately (recognition vs. production vocabulary).

>>>Meaning resides in the redundancies of the language itself.

>>Balderdash. There, I said it. Do your worst.

>I had thought you were talking about translation from language to some
>semantic representation, which would be a separate metalanguage whose
>ostensive purpose was representing meanings.

I get it. "Redundancy" in this case means that there is more than one verbal way to indicate the same meaning (not, as I took it, that there are parts of words -- as spelled, for instance -- and phrases that are not necessary to resolve ambiguities). I guess I can take back the balderdash.

Concerning "translation," I said, as you say,

>>The point is to represent giving meaning to a word (or translating a
>>word into a meaning)

I was talking about "translating," which means substituting for a symbol the quantity that it represents. You, on the other hand, used a different word, spelled "translating," which means substituting for a symbol a

different symbol or set of symbols without reference to the represented quantity. See? Two completely different words, that happen to be spelled the same. Whew. Thought I was going to have to apologize or something.

>The remaining difficulty is because this account ignores the
>constitutive role of language in social reality and psychological
>reality, some would even claim in aspects of physical reality though I
>remain an agnostic among the true disbelievers there.

I'm not clear on what "constitutive" is intended to mean here (seriously). My dictionary says "(1) Constituent; making a thing what it is; essential. (2) having the power to establish or enact." I've heard the word used a lot in these contexts, but if I take it literally I have to reject it as meaningless (in CT). Words, being perceptions, don't have the power to do anything. The brain, the hierarchy of control theory, has the power to do things, meaning to make them occur in perception by means of action. Something has to be unpacked further here. Isn't it what people use words FOR (controlling their own perceptions) that relates to social, psychological, and physical reality? Isn't this something of an epistemological tangle?

I indulge in this sort of tangle all the time -- in tracking experiments, I certainly accept the joystick, cursor, and target as really being there, and the data as indicating what really happened. But I try to keep in mind the underlying concept that I am really comparing two models: a physical model and a neurological model. It seems to me that in order to relate language to interpersonal relationships etc. we must try to do the same thing: use the metaphor for convenience, but know that we are taking something for granted in doing so.

>The meaning of perceptions, I assert, resides in the dependencies that
>hold between them. In part dependencies on one level may be
>constitutive of a single perception on another, and the higher-level
>perception may even then be taken as the "meaning" of the lower-level
>perceptions-in-relationship -- and (importantly) vice versa.

I think we're coming to the end of the usefulness of the term "meaning." When we use the same term to indicate the evocation of one perception by another at the same level (perhaps through memory association) and the relationship between perceptions of one level and perceptions of a higher level, we're trying to pack completely different ideas into a single term. This means we always have to tack on an adjective to "meaning" in order to be sure the listener hears the right meaning: association-meaning, or level-meaning. This is made even worse by the fact that the level-meaning may or may not consist of the lower-level perceptions IN RELATIONSHIP. Transition-level perceptions arise from derivatives of lower-level perceptions, and that isn't a relationship but a function. So do we have to add "first-derivative-meaning?" And sensations are weighted sums of intensities (I think). Another kind of meaning? I think this all gets too confusing. HCT shows us distinctions that conventional terms don't distinguish. That is, the conventional terms indicate experiences that to the control theorist have important internal structure and in fact include mutually-exclusive categories. Like the term "learning," which refers to phenomena that in control theory can be identified as reorganizing, stabilizing, memorizing, or even just controlling (with a

novel disturbance). Those simply aren't subdivisions of a single topic.

As to the rest of this long rich post of yours, I'll just let it pass. We're both working on the same problems, obviously, and our understanding is converging. Nice. I'll look up the book references. Farewell to Noam Chomsky's Ragtime Band.

Bill Haley (910627) --

That's one way. Here is another way to make four out of the eight evenly spaced things: it is

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         o  o  o  o  o  o  o  o
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... where a "thing" is perceived as

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        o  o
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Rick Marken (910627) --

You've said it better than I could -- actually the "side-effect" notion didn't occur to me until after I'd written what I did (in fact, while I was falling asleep). When you say "Yow!" you're taking about a quarter of a second to emit the whole spectrum of phonemes from ee to oo by controlling a continuous motion of the tongue and lips. So how long does it take to generate EACH phoneme along this continuum? I think this makes the "side-effect" explanation even more pertinent. We have to avoid confusing objective measures of things like phonemes (spectrograms, for instance) with their perceptual phenomenology. My impression is that the PERCEPTIONS I am controlling in saying "Yow" are more like "ee", "ah", and "oo", with the intermediate ones happening by accident -- I can't get from "ee" to "ah" or from "ah" to "oo" continuously in any other way, and I certainly don't have any control of the transition (at normal speed). I can, of course, control the intermediate phonemes if I deliberately make this a transition-level task -- say "Yow" very very slowly, monitoring the intervening phonemes as they show up and making the transition smooth and continuous.

I think we can just take it for granted that articulation is always under feedback control. The side-effects that show up are, of course, open-loop -- but they're not controlled and are reproducible only if there's no disturbance. Disturbances of the side-effects wouldn't -- couldn't -- be resisted. If there were a little balloon in my mouth that you could inflate and deflate very rapidly while I say "Yow" in a normal manner, I couldn't resist such a disturbance between the "ee" and the "ah" -- but I probably couldn't hear it, either. A control system controls only what it senses.

Martin (Cricket-Man) Taylor (910626 ff) --

I think that if you explained cricket to me I could come reasonably close to understanding it! Now, 360-degree baseball with one base makes sense. Considering the source.

I agree generally with Rick Marken's observations about level of control.

If you employ a high level of control you're going to control more slowly. But for a skilled person (as opposed to a person trained in one specific predefined task), there are usually ways to reduce the level of control. Way, way back, Robert K. Clark and I did a series of experiments intended to discern levels by using reaction times (somewhere around 1955). We tried many experiments and continually ran into the problem that the subject found ways to redefine the perceived variable so as to make it a lower-level perception. For example, we wanted to see the reaction-time for a sequence. An element of the sequence was a brief flick of a cursor to the left or to the right (returning instantly to center). The subject was to press a button on the (rapid) sequence "left-right" but not on the sequence "left-left." Timing began, of course, with the second element, as the sequence couldn't be identified until then.

Initially, subjects showed about the reaction time we expected. But with practice, many of them abruptly began showing a much shorter reaction time -- closer to the time we associated with second-order control, control of sensations. Since we set the criterion that ALL subjects had to show about the same reaction time, we had to discard this test. On questioning, the subjects with the very short reaction times explained that all they had to do was fixate on the place where the rightward excursion would occur, and ignore the initial leftward excursion. They were ignoring the sequence, and responding just to a flash of light in the right place. It was gratifying that their reaction times were very close to second-order reaction times, but not very gratifying in that we weren't measuring what we intended to measure.

But I don't think that is the whole story. Before I got into control theory I became pretty good at Ping-Pong. I found that the key to returning very fast serves and smashes was NOT to try to predict where the ball was going. Instead I just watched very closely how the other person was moving the paddle -- without even intending to hit the ball back. I would find myself moving and starting to swing my own paddle back before the other's paddle actually hit the ball -- more often than not the right way. So my experience is much like yours.

I conjecture that this is like moving your point of view down a few levels from the place it normally occupies (the cognitive/logical level). The experience is quite different from what we consider normal "consciousness" (which I think means viewing the world from symbol-manipulating levels, at least for theoreticians). There's no time to name what the other person is doing, or to say to yourself "that's a feint." When you occupy a lower level, there is a great deal more going on in each discernible moment, or rather discernible moments are much closer together because those lower systems both perceive and act much faster than the higher ones. Time slows down, it seems. But there's no "thought" as we're accustomed to experiencing thought.

In order for this to work, however, the experienced world has to remain pretty straightforward, so you can find the key perceptions that allow you to control without having to pay attention to higher-order aspects of the world. In an article on baseball (the reference is packed somewhere), the author reported that outfielders catching fly-balls do NOT calculate the trajectory of the ball and figure out where they must stand in order to be where the ball will come down. What they do is move so that the

ball appears to rise slowly at a constant rate and constant bearing, until the moment of the catch. Just before that moment, there are variations due to different styles of catching, but the point is that the fielder gets to the right place without making any predictions or calculations of a complex nature, starting with the crack of the bat. Finding the right low-level variable to control (here, transition) vastly simplifies the control task.

If the world gets more complex, this order-reduction doesn't work so well. If the other Ping-Pong player knows as much as I do, he or she (I've been soundly beaten by both on a regular basis) just makes sure that the lower-order variables I'm using for control are misleading. Then I find myself automatically and swiftly going the WRONG way. Much too late, the higher levels yell "Oh, Hell!" as the ball flits past. I expect that cricket batsmen learn the same tricks, and that gullys occasionally yell "Oh, hell" if only to themselves, knowing they've been had at a higher level.

The proposition about awareness at different levels leaves a big research question hanging. How does consciousness participate in control at ANY level? It sometimes seems to me that we really control only a few things at any given time. Lower-level systems have to continue working, of course, in great multiplicity. But at the level where we're consciously controlling, only those systems in the conscious mode seem to be acting, and the number of them that can act at once seems limited not by the neural equipment that's available at that level, but by the horizontal capacity of awareness. In what state are the other systems at that level? Are they just turned off? Do they even exist (the stored-organization idea)? What about the effects of systems operating at higher levels than the level that is currently conscious (e.g., superego)? Thinking up appropriate experiments and getting them into usable form in computers is going to be an enormous undertaking. We are seriously understaffed. I never thought I would see a need for Big Science, but this problem looks Big to me.

Gary Cziko (910627) --

>More seriously, do you have a published reference by Gibbons or anyone
>which applies PCT to law. I have Gibbon's "Justifying Law" but it does
>not make explicit reference to PCT or you (at least I haven't found it).

I'll send you a copy of Hugh Gibbons' "The Death of Jeffry Stapleton." I only have a couple of copies. I hope Hugh is coming to the next meeting; if so, he will probably have a greatly revised version to pass out. The first half of Jeffry is law; the second half is control theory applied to how lawyers (and others) think. In "Justifying Law," Gibbons had only just run across my '73 book, and was still citing Plans and the Organization of Behavior (but the article is a masterpiece anyway, I think).

I wish that everyone on this net were coming to our meeting in August. Send in your registrations NOW so I don't have to wait to find out who is coming. Every meeting has been different in some interesting new way. The

next one is going to be just like all the others.

Best,

Bill.

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Date: Sat, 29 Jun 91 11:00:04 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Mary on time, women in CSG

Subject: speed, women in the CSG

[from Mary Powers]

When I crashed my car last November, I watched every detail of the hood (bonnet, Martin) crumpling up in slow motion. I'm thinking this has something to do with awareness being focussed on a lower level than usual - something like the eternity it takes to do a run on on of Bill's lower-level tracking experiments relative to the apparent speed of a higher-level run. If our perception of duration is calibrated at the levels where awareness is usually directed, there could be a sense of longer duration when awareness is drawn down to where perceptions, reaction times, etc. are faster.

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

Why so few women in the CSG? There are two others besides myself, Perry Good and Diane Gossen, who are Reality Therapists who have found a more solid footing in CT than did Glasser in his brief flirtation (and show a lot of guts doing so, because Glasser doesn't approve). Other women have come and gone (one biologist was really offended at the idea of purposive protozoa - her concept of intention was all tangled up with consciousness, I think, though paradoxically she could buy the idea of Gaia). I don't know the ratio of male and female graduate students who have been exposed to CT - if there are a lot of women, are they more concerned with breaking into the establishment and less inclined to gamble on CT being the wave of the future?

Ditto established women in the various fields CT embraces. Do they feel the same chilly climate as the women in physics and astronomy reported on in the 6/21 issue of Science? Not conducive to hopping down CT's bunny trail - it would simply compound that outsider status.

In the book "A Different Voice", by Carol Gilligan, the author takes out after a study by Lawrence Kohlberg which seems to show that women do not proceed as high through levels of (moral) development as do men. Her point is that women form their moral code differently, through a network of relationships, rather than through a hierarchical kind of structure (like systems concepts, principles, etc?). This would suggest, if one believes her, that

the structure of CT is male-oriented, in this culture anyway. Certainly so if there is an implication that concepts of relationships don't quite qualify as systems concepts (confusing to have the same name as one of the levels lower down). Also if one's experience in a social or professional hierarchy has been unpleasant, maybe the idea of *being* a hierarchy is a turn-off, provided one has also got past that big initial red flag, the word "control", at which a lot more women than men (in my experience) go bananas and stop listening, again perhaps because of more personal experience of being a controllee.

Rather than continue this woolly wondering why there aren't more women in the CSG, I'd like to mention a couple of reasons why they might like to be, in case any are listening in. There was a time, here in Colorado, when silver lay around in big lumps on the ground. You didn't mine it, you just picked it up. CT is like that now. Everywhere you look in the life sciences there are lumps of data to be picked up and worked this new way. A new and rich frontier, ready and waiting for people to boldly go. And the people who are doing it are extremely nice. They actually believe that other people are control systems too. They may (or may not) be ambitious, aggressive, and bound for glory, but they do not put each other down, or undercut, or sneer (I can't believe how nasty some scientists are to each other as reported several times in Science lately). There's a certain amount of reviewer-bashing, but sooner or later a recognition (if not acceptance) of the personal organization the rejecting reviewers are defending with ignorance and misinformation. It may be that CT attracts only people who are already golden rule-ish and not into pushing people around or thinking some people more human than others. May our tribe increase! Other theories about how people are organized haven't worked very well - Bush's theories about Saddam, for instance, didn't seem to take into account that if he pushed, Saddam would push back, just like Bush himself would do (of course Bush was really hot to use that old standby, overwhelming force) - but CT gives the clearest picture so far of how "they" are organized, and lo and behold, it's just like me in fundamental ways. The fact that one can fit one's whole self (except maybe the tippy-top level) into the theory one is applying to everyone else is one reason why CT is so satisfying. It includes everything clear up to and including one's moral code (thou shalt not push people around, etc., etc.) along with some damned good explanations for why (because they'll push back, or wait and get you later, like any properly functioning control system). As I said, CSG people are extremely nice. Any woman - or any man, for that matter - who thinks CT at all interesting would also find the CSG to be a congenial group. We may have

disagreements, but we don't get nasty.

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Date: Sun, 30 Jun 91 20:57:41 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: ed ford <ATEDF@ASUACAD.BITNET>
Subject: stuff and system concepts

From Ed Ford (back from vacation) June 30, 1991

To new CSGnet members - If you haven't received our CSG newsletter and would like to be on our mailing list, please let me know the next time you post something on the network. Giving your mailing address at the end does help me pick up new members.

David - Please send a copy of Towards A Perceptual Control Theory Psychotherapy. Sorry to learn you're not going to be at Durango. I'll miss you, my friend.

Rick - Would you send me a copy of your paper on "Hierarchical Behavior of Perception"

Mary (910628) - my counseling and teaching experiences reveal there are really no differences on the acceptance and understanding of CT. I find men and woman tend to pick up the ideas at equal speeds (unless there are engineers in the group). Women also seem to translate the ideas into their daily living as efficiently as men.

A very, very belated reply to Rick Marken (910617.1030)

>Are there always the SAME values & standards? Are you saying only a
>particular set of values and standards lead to inner & community
>peace?

As I understand CT, all concepts are created from lower level experiential levels. Since our individual experiences vary as we grow and continually create perceptions, then when we create concepts, not only do we create them according to our own individual personal goals and from our own created memories, but we create similarly named concepts from a variety of differing experiences. Thus the problem of trying to create a similar "understanding" of a commonly understood system concepts. An additional problem is that when I attempt to describe my concept at principles and program level, I assume that the thoughts that I generate and the words that flow from me are going to be the same as those that are perceived and created in the receiving living control system. Obviously, they aren't.

With this in mind, I address the question. I think there are values and standards that lead to individual and community peace. The problem is taking those system concepts and setting them to standards and criteria that are universally understood and applicable. I am not, I repeat not, talking about revealed truth. I am talking about my attempt to arrive at some system concepts, priorities, standards, from which actions can be taken such that people can live a more satisfying way. For example, my ideas of commitment and quality time

as I've defined them seem to work well universally with couples and in parent-child relationship. The standards and criteria I've set seem to lead couples to an experience of intimacy which provides the kind of satisfaction that satisfies their internal idea of happiness with another. Thus, I've been able to help others achieve what seems to be a goal common among the variety of people with whom I work. Obviously, describing that experience is like your wife trying to explain to you what it is like to have a baby. To those who've had the experience, no explanation is necessary, to those who haven't, or who can't, no explanation is possible.

A recent workshop participant told me "having read your books and listened to you speak, I get a certain sense of where you're coming from." And that's my problem. It's hard to describe a system of ideas (SC level) in lower order terms and have it adequately understood - not because of the listener - but because of the way we're designed, especially due to the variety of experiences (or lack of) we've had from which we have created similar words. "Love your neighbor" and "respecting the rights of others" are great ideas. I shudder to think of the millions who have suffered from the hands of those who claim they are living by those ideals. But in my own way, if I can help people achieve a similar experience which brings lasting satisfaction, I think I have broken ground toward finding universally acceptable system concepts.

>From my point of view, controlling a line on a screen is as real as
>controlling the number of extra marital affairs on has....

I agree the theory is the same in both instances, but humans deal with each other primarily at the highest orders and their purpose for controlling perceptions not only varies, but is far more difficult to define and understand and a lot more complicated to deal with.

I have found control theory and the perception of humans as living control systems THE single most important tool to helping people deal with their conflicts and finding satisfaction in their lives. The strategies I've derived from this theory boggle my mind. In fact, I no longer see myself as a reality therapist. Reality therapy is only a small piece of the control theory pie. As one teacher said after a two-day workshop in Pasco, WA: "Control theory gives the counselor such a broad understanding of the client. It gives me so many more options and allows me to explore so many more ways to help people deal with their problems." It is hard for someone that doesn't do what I do and isn't faced with the complex human problems with which I deal (experiences) to perceive how CT is so useful in the area in which I deal. And, I might add, it is hard for this social worker to understand the complex world of ideas and concepts with which you theorists deal.

>The question is whether ANY particular values of any controlled
>variables can ever be considered absolutely RIGHT from a control
>theory view .....a system concept that is satiated by principles
>that have to do with cooperation. Maybe these are the absolute
>references - the "right" references - you & Joel are talking about.

Your final conclusion to all this was beautifully said. I think the test for systems concepts is the harmonious cooperation that they provide regardless of the environment (the last four words were added to deal with Bill's concern of the application of the principle "that in order to love thy neighbor, you better stay in the right neighborhood and not let inferior unlovable people move next door".

The bottom line in all this is that when you deal with system concepts, your are dealing in an area that by it's very nature isn't easily understood. The variety of experiences that define these areas are so varied from one living system to another. And how another's system concepts are prioritized, how their standards and criteria define their limits, and the variety of actions all make this an area that is easily confused and hard to deal with, much less understood. The best example for those who are married is trying to understand one's spouse. (I gave up trying to understand my spouse 17 years into my marriage and things improved remarkably. Now, after almost 41 years, I'm still very happy.)

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