CSG_9109

==========

Date: Sun, 1 Sep 1991 16:09:28 edt
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Peter Cariani <peterc@CHAOS.CS.BRANDEIS.EDU>
Subject: Adaptive prosthetics (re: Bruce Nevin's device)
In-Reply-To: "Bruce E. Nevin"'s message of Tue, 27 Aug 1991 13:54:43 EDT

A couple of years ago I looked into what was going on in the realm of adaptive prosthetic devices (either sensors or effectors), because I was interested in robotic devices that adaptively construct their own sensors and effectors (which I think would come under the CSG rubric of "reorganization). Adaptive prosthetic devices for people are very similar in that they alter the (semantic) relation of the signal the human receives to circumstances in the world at large.

Now I thought it would be a very natural idea for engineers to develop flexible prostheses where the user has many degrees-of-freedom to control (and optimize to his or her needs). This strategy would be most useful in realms where a good theory of exactly how the perceptual apparatus works is still lacking (as in cochlear implants or visual prosthetics or ambulatory prosthetics).

At the time, it seemed that 1) there was relatively little work being done in designing and developing prosthetic devices, and 2) very little of this work had an adaptive strategy in mind (it was all the experimenter fixing the encoding scheme and testing various schemes on subjects). I was very much impressed by the possibilities that had been demonstrated (like the visual vibrotactor arrays of Bach-y-Rita in the 1960's), but it seems like progress is painfully slow -- perhaps these problems aren't receiving the funding levels they deserve.

I have heard of efforts to make an acoustic transducer that would in real time alter various acoustic parameters of an incoming signal and rsend the new acoustic signal down the ear canal. This would be the sort of device one could wear. We've also thought it would be interesting if one could wear a delay line on one ear, and see what the effects on auditory localization would be (the auditory analog of the inverted-eyeglasses experiment).

There are many possibilities.

I have a number of comments regarding neural coding in the auditory system, which I will try to send later. From experiments which we have done on the auditory nerve on the encoding of pitch and from others' work using synthetic vowels, it appears (to me and to others) that the information being utilized to determine pitch/vowel formants is encoded in the pattern of interspike intervals across the array, rather than in the average discharge rates. The outputs of these filters are not some (scalar) signal level, but a distribution of interspike intervals(which is a multi-dimensional entity). I'll try to say more later, when I have more time. (Bill Powers and I discussed some of these issues a while back on the CSG board....)

Peter Cariani

==========

Date: Sun, 1 Sep 1991 16:27:10 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>

From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Sonograph on an AT

+ | \ - v |
==========
Date: Sun, 1 Sep 1991 17:11:35 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Sonograph opn an AT

[From Bill Powers (910901)]

When I sent this file it went awfully fast -- most of it disappeared. So here goes again: Sonograph on an AT.

For Bruce Nevin, Martin Taylor, other experimental linguists:

I've been making some progress on writing a sonograph program for my AT (286, 10 MHz). I have a Real Time Devices model 200 A/T board that can take samples at 4000/sec. Not having a microphone yet, I hooked up a little transistor radio's earphone jack to the analogue input, and am able to record 1-sec samples of voice (4000 data points).

The rest of the program is just an implementation of Bruce's description of how a sonograph works. A filter tuned to frequences from 0 Hz to 980 Hz is scanned across the data; the output is rectified and smoothed, and is plotted across one line on my VGA gray-scale monitor as one of 16 intensities. Then the filter frequency is increased 2 Hz and the scan is done again, with the display line displaced upward appropriately. It takes about 10 minutes to develop a 480 x 4000 sonogram. The result looks pretty convincing to my ignorant eye.

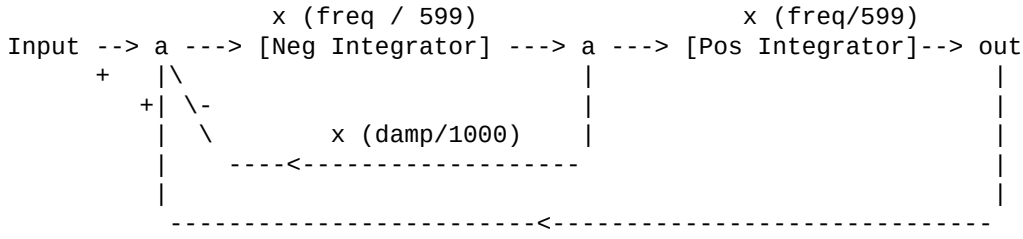
You programmers might be interested in my filter: it's a digital rendition of an analogue solution of a second-order differential equation! Here it is, in C:

```
-----------------------------------------------------------
int freq,input;  /* globals */
int a = 0;
int out = 0;    /* globals for re-initialization */

int filter(int input)
{
 a = a - muldiv(damp,a,1000) - muldiv(freq,(out + input),599);
 out = out + muldiv(freq,a,599);
 return out;
}
-----------------------------------------------------------
```

"Muldiv" is an assembler program that multiples the first two arguments to yield a 32-bit product, which is then divided by the third argument to return the result. This allows computing 10000 * 25000/15000 without loss of accuracy due to overflow of the intermediate product. The same thing could be done with long integers but much more slowly.

The above function implements (crudely) a two-integrator feedback circuit:



Adjusting "freq" changes the center frequency; adjusting "damp" changes the bandwidth of the filter. With "dt" in the integration equal to one computing cycle, numerically equal to 1 and physically representing 1/4000 sec, the constants make "freq" numerically equal to the filter frequency (including effects of damping). I use about 20 for "damp" which seems to make the filter sharp enough.

What I want to try is to see how the frequency bands shift as one changes the articulator configurations in naturally independent ways -- going from eee to ooo by rounding the lips, from aah to lll by moving the tip of the tongue, or from aah to ngg by moving the back of the tongue. I expect that you guys can save me some time here. It may be possible to construct a perceptual function that will show variables that depend almost independently on each natural degree of freedom of the articulators.

If this can be done, it seems to me that deaf people might find it much easier to use visual indicators of correct speech. Instead of having to sort out which effect of articulation alters which aspect of a sonogram, the person could be told "Use the tip of your tongue to control this one, the rounding of your lips to control this one, and the back of your tongue to control the other." Or whatever proves to be the case. Pitch would also be represented along with loudness. Of course the same display would be used to show other people's speech for passive listening.

How many channels are really needed for this? I'm using 480 channels spaced 2 Hz apart - is that overkill? And what sampling interval is really needed? It takes 10 minutes to produce a 480 x 4000 sonogram now, which is 1/600 of real time. With analogue filters (the above circuit takes a dual operational amplifier -- total cost under $2 per channel retail) it would be easy to achieve real-time response, but I'm going to see how much I can speed up this program.

So -- I haven't read my mail yet, but will send this first. I'd hate to find out at this point, when I'm so happy about the way this worked, that you've anticipated me.

Best

Bill P.

==========

Date: Sun, 1 Sep 1991 20:41:07 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Sonograph opn an AT

[Martin Taylor 910901 20:40]
(Bill Powers 910901)

A worthy effort, building your spectral display! 480 channels is great overkill, though. 64 is probably enough.

Spectrograms have not been found very useful in helping the deaf to control their speech, though. It takes a higher level of abstraction.

Martin Taylor

Date: Sun, 1 Sep 1991 19:37:55 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Uncontrolled variables; prosthetics

[From Bill Powers (910901b)]

Martin Taylor (910831) --

>... there is a lot to analyze before we can get to the notion of a
>"note".

Amen. Control theory can help us get there, but it obviously can't supply the details, or principles as yet unknown. I'm impressed by the amount that is known, and very optimistic about what control theory can accomplish in the hands of people like you. You don't have to go on long to make me feel like a dilettante.

I agree with you that there is very likely to be simplicity hidden in all those confusing relationships. Maybe by viewing language as a process of controlling inputs it will be easier for those intimately acquainted with the confusion to see a way out of it. There's not much I can do to help with that, except to continue working with the things I understand (which seem to dwindle as I learn what others are up to).

On the dimensionality of perception:

>The result, as I see it, is that there is a 3 or 4 order-of-magnitude
>difference in the dimensionality of the available control signals and
>the dimensionality of the thing being controlled. This is not a viable
>relationship. You can't control a 2-D variable with a 1-D controller,
>nor a million-D variable with a 600-D controller. Where does this leave
>us?

I think it leaves us in a world in which we perceive many things, but only some of them can be directly affected. Especially at the lower levels, the uncontrolled perceptions far outnumber those that are under control. At the intensity level, for example, we control light intensity (iris reflex), sound intensity (tympanic reflex), and signals from tendon and spindle receptors -- and that's about all. Practically all the individual visual inputs are uncontrolled at the lowest levels, meaning that many millions of signals are not involved in first-order or even second-order control systems of any higher-level significance.

It's not quite as bad as it sounds, however. Consider the low-level

aspects of controlling finger position, as in reaching out to touch something. When you see yourself doing this, you see the object to be touched (and its surroundings), and your arm, hand, and the finger in question. What you control, however, is just the position and orientation of the finger and target as projected onto your retina. And it's not even all of the finger that's involved: you aren't controlling the fingernails or the wrinkles at the knuckles or the little hairs you can see (or the liver spots). You aren't controlling the shirtsleeve or the wrist sticking out of it. The longer you examine what is going on, the fewer perceptual variables, you realize, have any preferred states at all.

But when you bring your finger into the reference-relationship to the target, all those other perceptions change. They change exactly as they must, given the properties of the arm and hand and the intended result. They go along for the ride because they can't do anything else. This accounts for the behavior of vast numbers of the low-level perceptions which are uncontrolled, yet are not independently variable either.

Again we're brought back to that neglected issue, attention. When you look at your hand and arm, you can easily select something other than your finger as the object that is to touch the target. Then that part of the scene -- say, the back of your hand -- becomes the controlled variable, and you establish a reference condition for IT. Now the finger goes along for the ride and has no specific preferred state. It's as though we can select which aspect of the world is to be controlled, and by doing so we allow other aspects to go uncontrolled. My simplistic model in which all control systems, once acquired, simply sit there and control their inputs no matter what, is clearly not going to be acceptable much longer. There is much more fluidity in what is controlled than I originally proposed. This is fine with me -- it would be a shame if a model approaching its fortieth birthday still said everything worth saying.

I did forsee at least part of this problem, in proposing that at every level there would be *uncontrolled* perceptual signals. I had the idea that these signals would be processed as usual, at every level, by the appropriate type of perceptual function, but the resulting signals would simply be passed on to the next level up, no local control system being involved. This is perfectly in line with your statement:

>It seems to me absolutely necessary that there be open-loop reduction of
>data in the perceptual input section of the overall control system.

Perceptual signals that ARE controlled at a given level would be maintained against any direct kind of disturbance; only extreme disturbances could "get through" to the next higher level. When I point at a target, disturbances of my hand are handled at the lower levels if they are of normal kinds, speeds, and sizes (gravity, for instance). So the relationship-control system normally never senses any problem due to such disturbances.

On the other hand, my perception of the target position is *uncontrolled.* The target can move or be moved independently of me, and under normal circumstances there is nothing I can do to keep it from moving. However, I can still control the perception of relationship, because I do have control over one of its elements: my hand position. The uncontrolled perception gets through unopposed and tends to alter the perceived relationship; I alter the reference position of my hand, and

thus prevent this incipient change in the relationship from becoming significant.

So to sum up, your observations are acute and correct. We live in a world made up mostly of uncontrolled perceptions (at the lowest levels). Some of these perceptions are automatically forced, by laws of nature, to go along when others are controlled, so they aren't really controlled OR uncontrolled: their behaviors are *entailed* in the control of a few of the variables to which they are connected. Other perceptions are truly uncontrolled, in that they can vary independently. These variables account for the fact that high-level perceptual variables can still be disturbed, for uncontrolled perceptions contribute to high-level ones. Usually, however, we can manage to keep the high-level variables under control by altering OTHER components that contribute to them, over which we do have direct control.

We can't control the sun's sending rays down to overheat our craniums, but we can put on a hat. We can't control the place where the ice-cream truck parks, but we can move ourselves close to it and get a goodie anyway. We don't have to worry about our shoes and feet and belly-buttons moving along with us to the ice-cream truck; they're attached to us and come along with us without our having to desire specifically that they do.

That takes care of many of the perceptions you are worrying about, but not all of them. The Crab Nebula is still up there, expanding, and we have no way to affect it. Our perceptions of it are not our doing, save for the choice of looking or not looking. Not even all perceptions for which we have set reference levels are controllable: there is still apartheidt in South Africa; there are still hungry people; we suffer affronts and injustices we would rather not suffer, yet we have no means of affecting these things. Our principles and self-concepts demand that we have intentions and opinions that we have no way to implement; all we can do is try to learn how to have the necessary effects. We will all die without having gained control over important experiences that matter to us. The error signals keep us trying.

We are, in other words, still evolving and still learning. Our model of the human system is far from complete, but so is the real system of which it is a model.

I agree with you, therefore, when you say

>But statistical reduction of the data based on correlations among
>sensors could permit a reduction of input dimensionality to the same
>range as control dimensionality, thus allowing the theory (J.G.T.'s or
>B.P.'s) to work.

To that, I would just add that statistical reduction isn't the only means, and that despite the reduction there still remain many perceptions that are variable and unaccounted for. I think that their presence (which exists at any stage of development) in some way accounts for the fact that levels of control exist: the levels are attempts to cast the world in controllable terms by conceiving it in more general -- and fewer -- dimensions. Whatever the current highest level, there remain uncontrolled perceptions of that type, thus calling for yet another level of integration to be developed. This must affect evolution: Peter Corning, in his theory of Synergy, can cite chapter and verse. If Peter is

listening, or if someone who knows him is, I hope he can be persuaded to start up a thread on this topic, which will illuminate many areas we have talked about on this net.

You conclude:

>But not all the effects in perception can be controlled.

... and you can see that we are in complete agreement.

Peter Cariani (910901) --

You've been very quiet -- nice to hear from you.

The area of prosthetic devices is ripe for applications from control theory (including your ideas about systems that construct their own controlled variables). A few years back, there was a show on PBS television about a researcher (Ohio? Indiana?) who was using computer control of topical electrical stimulation to leg muscles to permit paraplegics to ride bicycles (three-wheeled) and even walk. I should remember his name but I don't -- a Russian name. To this old control theorist, the show was inspiring because the patients worked so hard and were so delighted at their recovery of skills they thought had been lost forever. But I was vexed by the fact that this method was being carried out open-loop: the computer was simply programmed with patterns of stimulation that the patients triggered off with push-buttons. It would have been so easy to fasten potentiometers to the knee and hip joints, and close the loop! I still think this should be tried. The patients lurched and staggered, overshot and undershot, all because there wasn't any feedback to make the joint control stable and predictable.

Your ideas about adaptive devices should be tried, too. To them I would add that we need to specify the criteria for adaptation -- I don't think we can build them into the device itself, but have to leave it up to the human being to say "that's not good enough -- adapt some more." The E. coli principle: it's amazing how much control you can exert just by varying WHEN a random adaptive change is instituted.

As to interspike intervals, I still want to argue with you about that -- unless there's some reason to prefer the reciprocal of frequency that isn't trivial. Pulse-frequency coded signals will naturally carry many trains of spikes that show interspike interval variations -- because the signals being carried are varying. As I said when we talked about this before, the real problem in deciphering details of interspike intervals lies at the receiving end. When you cross a synapse into a target cell, you're in an analog world of chemical concentrations. Once you're in that world I don't think it makes much difference whether you think frequency or interval. Individual interspike intervals no longer can have any significance. We have to look at the net result in terms of concentrations, which vary on a much longer time-scale.

But you could convince me if you had the right ammunition.

Best to all

Bill P.

==========
Date: Mon, 2 Sep 1991 11:57:25 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Williams CT Bibliography

[from Gary Cziko 910902]

Greg Williams has sent me the fourth version of his "MATERIALS RELATED TO LIVING CONTROL SYSTEMS THEORY." This is a complete bibliography of all known books, papers, articles, and chapters related to what we are now calling Perceptual Control Theory. It spans 1957 through 1991 and references are organized by year.

Since many of the hardcore CSGnetters may have picked up a copy of Greg's bibliography in Durango, I will NOT be sending it by default to this group or any other individual. Anyone wanting a copy should send me a personal note.

The file is 71 kilobytes long.--Gary
==========
Gary A. Cziko Telephone: (217) 333-4382
University of Illinois FAX: (217) 244-0538
Educational Psychology Internet: g-cziko@uiuc.edu (1st choice)
210 Education Bitnet: cziko@uiucvmd (2nd choice)
1310 South 6th Street
Champaign, Illinois 61820-6990
USA
==========
==========
Date: Mon, 26 Aug 1991 16:27:10 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: control vs. influence

[Martin Taylor 910826 16:20]
(Bruce Nevin (910823 0732))

>
>In CT as Bill has articulated it an elemental control system (nice
>term!) can only get its reference signal from some other elemental
>control system.
>
I agree. I'm sorry my wording might have made it seem otherwise.
>
>Even so, this is influence and not control. Control is compulsory.
>Given a reference signal with a certain value (rate of neural firing),
>an elemental control system has no choice but to calculate the
>difference between its reference signal and its sensory input. Unless
>some other control system has changed the connections, it has no choice
>but to output this error signal to the reference-signal input of one or
>more other control systems. One control system *sets* the reference
>signal of another.
>
There may be a critical point here. "Control is compulsory." But as
my answer to Bill P. suggests, most elemental control systems are

subject to many simultaneous attempts to supply a reference, and in
some way those attempts must be reduced to one, and that one must
be commensurate with the perceptual input being controlled. Somewhere,
there must be a function relating the set of reference attempts to the
signal used as a reference, and unless it is wired in, it must be mutable.
In general, I think these "reference abstraction" functions must be
mutable, because there could otherwise be no learning. Whether they
are on-the-spot mutable is another question. But their mutability
softens somewhat the "Control is compulsory" nature of the hierarchy
(actually heterarchy, or in McCullough's term, 'anastomatic net').

No, it is not true that one control system sets the reference signal of
another, when more than one control system tries to use the same other.

Martin Taylor
==========
Date: Tue, 3 Sep 1991 07:43:58 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: reference signals: dependent variables???

[From Bill Powers (910903.0730)]

Martin Taylor (910826) (!)

Your post of 826 just arrived. I don't know if this has been straightened
out, but there's one remark to Bruce Nevin in it that echoes an earlier
one of yours that also made my ears prick up in passing:

>... most elemental control systems are subject to many simultaneous
>attempts to supply a reference, and in some way those attempts must be
>reduced to one, and that one must be commensurate with the perceptual
>input being controlled.

I may again be misconstruing what your words are intended to convey, but
the picture your words seem to imply is that the perceptual signal for a
given system exists first, and then a higher-level system must match a
reference signal to it (thus making the reference signal into a dependent
variable). Was this your intent? I did reply in this general area
concerning how multiple systems at one level set combined -- effective--
reference signals at a lower level. Did that post get to you? I didn't
mention the present point in that post except by implication.

Best

Bill P.
==========
Date: Tue, 3 Sep 1991 14:08:35 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Martin Taylor <mmt@DRETOR.DCIEM.DND.CA>
Subject: Re: reference signals: dependent variables???

[Martin Taylor 910903 14:00]
(Bill Powers 910903 07:30)
>
>Your post of 826 just arrived. I don't know if this has been straightned

>out, but there's one remark to Bruce Nevin in it that echoes an earlier
>one of yours that also made my ears prick up in passing:
>
>>... most elemental control systems are subject to many simultaneous
>>attempts to supply a reference, and in some way those attempts must be
>>reduced to one, and that one must be commensurate with the perceptual
>>input being controlled.
>
>I may again be misconstruing what your words are intended to convey, but
>the picture your words seem to imply is that the perceptual signal for a
>given system exists first, and then a higher-level system must match a
>reference signal to it (thus making the reference signal into a dependent
>variable). Was this your intent? I did reply in this general area
>concerning how multiple systems at one level set combined -- effective--
>reference signals at a lower level. Did that post get to you? I didn't
>mention the present point in that post except by implication.
>
I intended no committment as to the prior existence of the perceptual
signal, only that the reference and the perception that is to be matched
to that reference must have the same character. Since you agree that
much of what we perceive is uncontrolled, I should think you would
also be uncommitted as to whether the perceptual signal existed before
the elemental control system was committed to a particular referent.

Yes, I did get your earlier posting on the issue of the combination of
reference signals.

We have consciously been putting aside questions of learning, other than
the general statement that reorganization must be possible. To say
something on the issue of how the referent and the percept come to be
commensurable, we must discuss how learning occurs. Is it not the same
question?

Martin Taylor

PS. I shall be away from Sept 11 till Oct 15 (and maybe longer), but I
hope to be able to not miss any mail on CSG-L. Just don't expect replies
over that period.

==========

Date: Tue, 3 Sep 1991 14:43:02 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Language Control

[from Gary Cziko]

I figured it was time for me to chime in on the control of language subject
that Bill Powers, Martin Taylor, Bruce Nevin and Joel Judd have been
kicking around lately.

I can appreciate the problem of applying real-time disturbances to a
subjects speech. Since this appears so difficult, why don't we start with
something a little simpler, i.e., just showing that certain aspects of
spoken language can be controlled not by articulation but by stick
wiggling, knob figgling, and/or mouse figgling?

This could involve simply taking some prerecorded speech, a radio newscast

for example, and applying disturbances which the subject could counteract by moving a handle or mouse. Some simple variables would be speed (even some very inexpensive tape recorders now allow varying the speed without disturbing the pitch) and pitch. I suppose that it would also not be too hard to manipulate the intonation contour (from perfectly monotone to exagerated pitch contrasts, the way the British English sounds to me).

To look at more micro-level variables, I suppose one could also present just single syllables and play with something like voice-onset time. Or take a single phoneme sequence which is meaningful in both French and English (e.g., sea-si; mare-mere; pear-pere; tear-terre; peer-pire) and disturb it along the continuum which will change it from English to French pronunciation and have subjects keep it at one end or the other. Here it would be very interesting to see how the ability to pronounce these words in the foreign language was related to the ability to control their sound using the mouse.

So instead of starting out with the daunting task of real-time disturbances to the subjects own speech, why don't we first try to just demonstrate control of some interesting variables using pre-cooked disturbances?

I may have the resources to try this out this semester if I can get a few other faculty interested, but suggestions will be needed from you experimental linguists on the network. I would particularly welcome suggestions on interesting and feasiable variables.--Gary

==========
Gary A. Cziko Telephone: (217) 333-4382
University of Illinois FAX: (217) 244-0538
Educational Psychology Internet: g-cziko@uiuc.edu (1st choice)
210 Education Bitnet: cziko@uiucvmd (2nd choice)
1310 South 6th Street
Champaign, Illinois 61820-6990
USA
==========
==========

Date: Tue, 3 Sep 1991 14:47:40 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: jbjg7967@UXA.CSO.UIUC.EDU
Subject: Re: more sound stuff

[from Joel Judd]

>I wish I could remember better, because
>there was a very interesting study of "absolutly bilingual" French-English
>students in a bilingual school in London, in which there were differences
>among the students that could not be correlated with the student's first
>language, their preferred language, or the language they judged themselves
>to be easiest in. But they did correlate with the language that their
>friends said they were best in. If I am remembering correctly, what this
>says is that the students behaved perceptually according to what language
>an outside observer said they controlled best behaviourally. Interesting
>for HCT if so.

If you come across the ref, please let me know. Molly Mack did what sounds like a similar study in 1984 with French/English children, and similar findings. She didn't, however, get info from subjects' friends.

Joel Judd
==========
Date: Tue, 3 Sep 1991 15:12:10 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: compulsion, spectrograph

[From: Bruce Nevin (910903 1242)]

Martin Taylor 910831 20:40
>not all the effects in perception can be controlled.

Don't you mean that they can't all be controlled at the same time?
Assuming some sort of summation passed up the control hierarchy,
any particular elemental contributor to the perceptual summation
might be affected by behavioral outputs on occasion but not on another.

This is related to the converse situation, where more than one control
system may contribute its error signal to the reference value of some
lower-level control system. Again, the plural signals are summed in
some way so that for the lower-level system there is just one reference
signal.

This post from you relating to this came into my mailbox a second time,
bounced off Neptune by the listserver, I guess.

[Martin Taylor 910826 16:20]

>most elemental control systems are
>subject to many simultaneous attempts to supply a reference, and in
>some way those attempts must be reduced to one, and that one must
>be commensurate with the perceptual input being controlled. Somewhere,
>there must be a function relating the set of reference attempts to the
>signal used as a reference, and unless it is wired in, it must be mutable.
>In general, I think these "reference abstraction" functions must be
>mutable, because there could otherwise be no learning. Whether they
>are on-the-spot mutable is another question. But their mutability
>softens somewhat the "Control is compulsory" nature of the hierarchy
>(actually heterarchy, or in McCullough's term, 'anastomatic net').

>No, it is not true that one control system sets the reference signal of
>another, when more than one control system tries to use the same other.

You have referred to this possibility (more than one elemental control
system (ECS) on level n+1 "controlling" a given ECS on level n) as
reason to doubt the "compulsory" character of relations within the
control hierarchy. But I was attributing that character as a property
within the ECS, and saying that it is not a property within a living
control system such as a person. Given a reference signal of strength
r, an ECS *must* compare r with its perceptual input i and it *must*
output the difference as an error signal. This is completely unaffected
by the occasion of more than one error signal from level n+1 being
combined to produce r.

If A and B on level n+1 both contribute to the reference signal of C on
level n, that reduces the "compulsory" correspondence of either A with C
or B with C. On the one hand, that explains the refractory nature of

living control systems under coercion (by way of the limiting case, conflict). On the other hand, it corresponds in an interesting way to interpersonal relationships in which people seem to have their reference levels set by other people. Perhaps this is what you have in mind?

Consider a military hierarchy, or the "authority" experiments that Kent McClelland reviews in his discussion of power in his fine paper. One could surmise that there are multiple sources providing input to the reference signal for certain high-level control system concerned with interpersonal relations, governing who is judged credible, whose mid-level requests or commands for action are taken as setting reference signals for action, etc. Not all of these persons and other contributors need be physically present to provide that input, most of them are in fact present in memory and imagination. (As when the client heard his therapist's voice in his head saying "don't drink!" so he decided not to on a particular occasion, as recounted by Claude Steiner.)

But all that is physically present are environmental events interpreted by the subordinate as intensities, sensations, transitions, configurations, etc. All of them probably evoke memories and initiate imaginative processes, in particular the processes we experience as understanding gesture, language, etc. Having worked its way up the hierarchy to a fairly high level, this input may contribute to the reference signal for control systems governing other hierarchies down to effectors and action. There is no direct input of signals from control systems in one person to the reference-signal "wires" of control systems in the other. The entire depth of the control hierarchy literally stands between, and it does so in each person.

Bill's familiar box diagram helps me to see the disanalogy. We might suppose that a single ECS implemented in neurons may be internally complex, but we ignore all but the inputs and outputs identified in the box diagram. Why not have a single box diagram for a living (human) control system? Then we could think about social-level control systems. In such a diagram for one human, there would not only be innumerably many more inputs and outputs at the extremities (sensors and effectors at the bottom, supposed interpersonal reference-signal inputs at the top), but also the "comparator" box in the middle would be enormously complex--the whole intervening control hierarchy in fact. This makes clear to me that we are engaged in an error of logical type when we do this.

Peter Cariani (Sun, 1 Sep 1991 16:09:28 edt)

>adaptively construct their own
>sensors and effectors (which I think would come under the CSG rubric of
>"reorganization). Adaptive prosthetic devices for people are very similar
>in that they alter the (semantic) relation of the signal the human
>receives to circumstances in the world at large.

As I understand it, reorganization concerns connections among control systems and at the low end of the hierarchy connections between control systems and sensors and effectors. The only way it could be taken as concerning the effectors and sensors themselves is in the sense that the entire hierarchy from sensors up to level n of the hierarchy could be taken as constituting sensors for control systems on level n+1, and similarly for effectors.

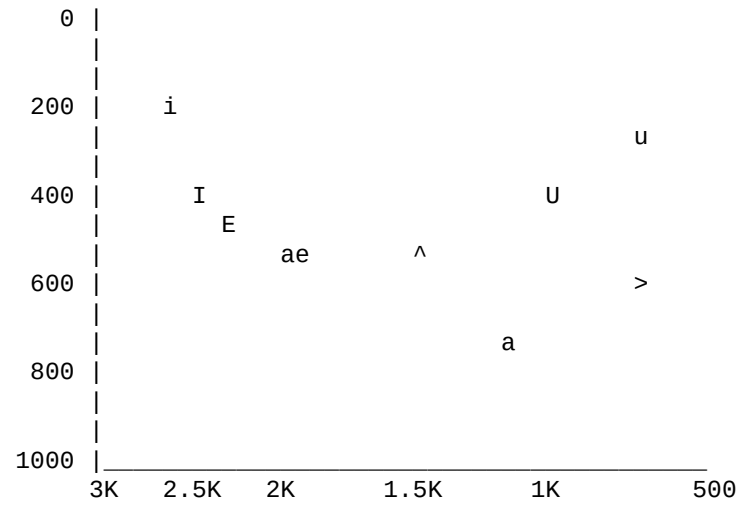
I have looked at a couple of your articles, which Chuck Tucker very kindly gave me. I was unable to find inputs for reference signals in any of your block diagrams. Without this there can be no hierarchy and indeed no control in the sense of HCT. Am I missing something by too hasty reading?

Since you're just the other side of the river maybe we could chat about mutual and respective interests sometime. I'm at 873-3992 about 7 to 3.

Bill Powers (910901)

I'm impressed by your putting together the beginnings of a sound spectrograph on your PC. I am also watching closely. I can always hope for the financial Noah's bane to abate.

A useful way to plot vowels against formant frequencies is to have F2 on the x axis and F1 on the y axis--with the high end of the numerical range at the origin in both cases. This approximates the socalled "vowel triangle" representing the front/back and high/low articulatory positions of the the tongue (front is left, back is right, the back vowels of English also have the lips rounded, which has some affect on formants too):



Your range is not enough to capture F2, I think.

Got to run.

Bruce Nevin
bn@bbn.com

==========

Date: Wed, 4 Sep 1991 08:10:20 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: clarification: formant graph

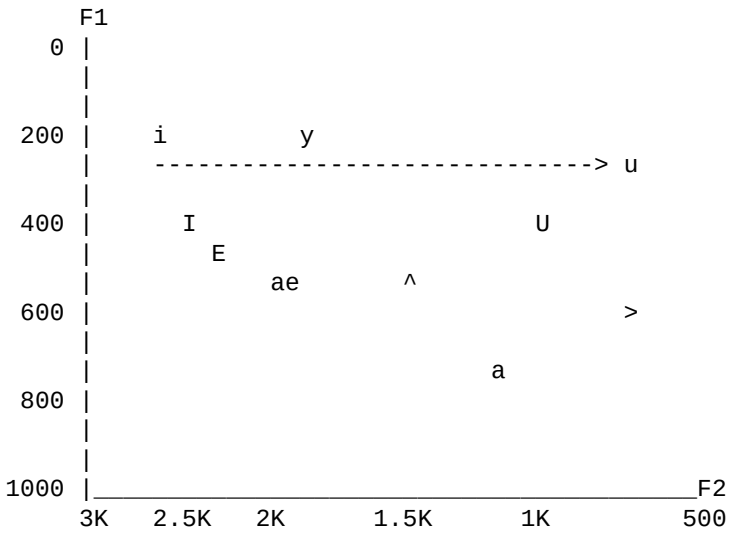
[From: Bruce Nevin (910904 0703)]

I meant to explain the symbols for vowels but ran out of time:

i as in beet
I as in bit
E as in bet
ae as in bat
^ as in but
a as in bah!
> as in bought
U as in foot
u as in boot

It is an interesting perception to hear F2 descending smoothly from ~2500Hz to ~1900Hz to ~700 Hz with F1 relatively constant around 200-250 Hz as you sweep smoothly from i through y (french u, German u-umlaut) to u. Plug your ears and listen very closely for a high-pitched buzz descending over a low-pitched buzz. (A good place for this: take a bath and immerse your head in the bathtub, ears full of water.) The transition from i to y reflects only lip rounding, and that from y to u reflects only the backing of the tongue. A similar effect obtains if you interpose a back unrounded vowel, like Turkish barred-i, which I believe would come somewhere above ^ on the graph: in this case, you get the effect of tongue backing first, then the effect of lip rounding. However, back unrounded vowels are *much* harder for English speakers to control. It's easier to follow the instruction "say i, now round your lips while still saying i" than it is to follow "say u, now unround your lips without moving your tongue." This appears to be because the back of the tongue is less articulate than the blade and tip. It may also be in part because, to an extent as a byproduct of this, there is less articulatory space for the back of the tongue.

Here's the graph again, with the two axes labelled, and with an arrow showing the transition i-y-u:



(I forgot also to emphasize that the F2 scale is logarithmic.)

I hope this helps give a picture of the configurations of formants that we seem to be controlling.

Bruce
==========

Date: Wed, 4 Sep 1991 07:41:19 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Reference signals; Testing control; sonograph

[From Bill Powers (910904.0630)]

Martin Taylor (910903.1212) --

>I intended no commitment as to the prior existence of the perceptual
>signal, only that the reference and the perception that is to be matched
>to that reference must have the same character.

In a one-dimensional-signal model, the "character" of a perceptual signal is not carried in the signal. All the signal can do is increase and decrease in magnitude. The character of the perceptual signal is in the way it depends on combinations of signals at lower levels, and that is determined by the form of the input function. This is why the character of any isolated percept is subjectively so elusive, and in fact disappears after prolonged experience in isolation. It exists not in the experienced signal, but in the unexperienced neural calculations.

It follows that once a control system has formed, complete with comparator, all values of reference signal have meaning: even a value of zero means "seek a zero magnitude of your perceptual signal" (i.e., remove it from your experience; avoid it).

>Since you agree that much of what we perceive is uncontrolled, I should
>think you would also be uncommitted as to whether the perceptual signal
>existed before the elemental control system was committed to a
>particular referent.

I repeat that even absence of a reference signal has meaning: a control system without a reference signal will attempt to bring its perceptual signal to zero if it is turned on. The control system can't be "committed" to a particular reference signal (I like to reserve the term "referent" to mean "that to which the perceptual signal corresponds" -- the meaning of the perceptual signal). It can't because it has no control over the reference setting it receives, if any. Any signal arriving at its comparator from any source (even the probes of Wilder Penfield) will specify some level of the perceptual signal that is to be achieved. the control system does not know where its reference signals are coming from.

Control systems are never committed to any one value of reference signal. They bring their perceptual signals to (or at least toward) whatever momentary magnitude exists at the reference input of the comparator.

The problem of setting appropriate reference signals that vary in an appropriate way and go to the appropriate subsystems is entirely that of the superordinate system. It is up to an event-controlling system to figure out how it must pattern its output signals going to each contributing configuration-control system. The configuration-control systems don't care what reference signals they receive. Only the superordinate system can know what is appropriate, and even it knows this only in terms of effects on its own perceptual signal. But I'm putting too much intelligence here into a single system: this is really a problem for the reorganizing system, which isn't even concerned with the

variables that one control system becomes organized to control. I'm describing the problem that has to be solved, but I don't mean to suggest a smart little man sitting in each level, solving it.

EVERY arrangement of the functions and signals does SOMETHING. Modelers soon discover this, to their dismay. The question is, how does that something become something useful to the organism?

All of the control systems at a given level are available for use by any higher-order system that sends reference signals to them. They do not have to be reserved exclusively to any one higher-level system (even at at a given moment). Each lower-level system simply matches the magnitude of its perceptual signal to the net magnitude of reference signal that it is receiving, even zero. As I said previously, the higher-level systems can still achieve their respective goals independently because they are not perceiving in terms of just one lower-level signal: they perceive functions of many lower-level signals. Specifying the value of one of those functions still leaves many degrees of freedom of the lower-level world available for manipulation by other higher-order systems. This is how my one-dimensional model achieves that "distributed" character you are looking for in the overall system.

It doesn't matter to any higher-level system that the signal it sends to a given lower-level system is not the final value of reference signal for that lower system. The higher system can't check up on that -- it can't perceive what is going on in any one lower-level system. It perceives only the final result it constructs out of many lower-level perceptual signals.

Gary Cziko (910903) --

Excellent suggestion. It should be possible to give a person control over artificially-generated aspects of vocalizations, and then to insert variable disturbances. As you say, this will permit exploring controlled variables without the immense difficulties of disturbing articulators or distorting the feedback path. I think your idea is the closest to being achievable of any we've seen in this discussion. How about it, linguists?

Bruce Nevin (910904.1512) --
Re Social Control:

>This makes clear to me that we are engaged in an error of logical type
>when we do this. [analogize social control systems to individual ones]

Hurray, you get my point.

Re Peter Cariani's ideas:

>I was unable to find inputs for reference signals in any of your block
>diagrams.

I wonder about that, too, Peter. How about it?

Re audio spectrograph:

Thanks very much for the ideas. I see that I must squeeze a little more

speed out of my system to reach 3K. This is going to strain my simple filter algorithm -- I should be able to reach a sampling speed of 6K, but that leaves only two samples per cycle, which is probably going to get the computations into trouble. But we'll see.

The presentation you showed is almost exactly the sort of thing I wanted to do -- find a function of the formants that could be presented in place of the details in the sonogram. As this is a two-dimensional display, I'd use two perceptual functions: the sum of frequencies and the difference in frequencies, for instance (or the product and the ratio). The idea is to give the user simple one-dimensional meter readings -- the "abstractions" mentioned.

If I can't get my system to do a full-resolution sonagram (I can't decide how to spell that), there's another trick that might work: tracking each of the formants with a variable-frequency filter. By the way, I'm seeking a heck of a lot more than four formants! There's information at every harmonic of the fundamental buzz. Somebody has been throwing away a lot of information, presumably for a good or at least defensible reason.

Off to see the ruins at Chimney Rock.

Best to all

Bill P.

==========

Date: Wed, 4 Sep 1991 09:08:45 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: Gary on language control

Using a mouse to keep a disturbed audio signal sounding "normal" has interesting possibilities, especially if the control turns out *not* to match the disturbance. That would mean that the undisturbed audio was itself disturbed relative to the participant's reference. Calibrate for this by having them make the undisturbed audio sound normal, I suppose.

To make suggestions, it would be helpful to know what kinds of equipment you may have, and what it can do. This would have to include means for metering both the disturbed output and the controlled output.

BTW, did the newcomer who wanted info on CT and education get a copy of your paper?

In some haste,

Bruce

==========

Date: Wed, 4 Sep 1991 11:25:12 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: formants on a PC

[From: Bruce Nevin (910904 1043)]

Bill Powers (910904.0630)

>Hurray, you get my point.

A few months ago. Influence of one hierarchy on another seems to be horizontal rather than vertical, and on any corresponding level of the two hierarchies. Cf. my suggestions about the necessarily indirect character of divine intervention: If you're god or a messenger thereof how do you influence a world full of autonomous hierarchical control systems (free will)? By suggestion. Cf. also how hypnosis works. All of which is precisely not germane to understanding hierarchical control, only to understanding relations among hierarchical control systems. I'm not putting this down here for the sake of provocation, I haven't the bandwidth now, but only to contextualize: I have not needed persuading that social "control" is not hierarchical and that it is necessarily illusory.

>I see that I must squeeze a little more
>speed out of my system to reach 3K.

You don't need to reach 3K if you leave out the high front vowels i and e, only about 2500 (including F3) or 2000 (including only F1 and F2 plus F0, the fundamental). A little artificiality in your linguistic samples, or inability to track F2 for the high front (unrounded) vowels, is the tradeoff for reducing your frequency span.

>sonagram (I can't decide how to spell that),

Try: "sound spectrograph." A sonogram is a different beast.

>By the way, I'm seeking
>a heck of a lot more than four formants! There's information at every
>harmonic of the fundamental buzz. Somebody has been throwing away a lot
>of information, presumably for a good or at least defensible reason.

It would appear that language users throw away the higher harmonics for language distinctions, though not for factors of voice quality. Higher frequencies don't seem to make much difference for perception of phonemic distinction, they just tag along. Might be more important for some bursts and sibilant or fricative sounds. Subject to redetermination, of course, if we can demonstrate that people in fact do control for energy at higher harmonics.

>there's another trick that might work: tracking each
>of the formants with a variable-frequency filter.

If you can track F2 even crudely in real time, you might be able to skip over the gap between F1 and F2 when F2 is high (say, 1800 Hz or above), and reserve the bandwidth for higher frequencies. This is tricky, since immediately preceding and following sounds associated with consonants (and, farther out, other vowels) may exploit the region you are skipping.

Here is part of a table from Fischer-Joergensen's 1954 paper in the Lehiste _Readings in Acoustic Phonetics_. It gives the limits of variation for Danish vowels for a number of speakers in a number of utterances, but that is at least an indication. There are more recent

data that are more precise, but I haven't laid my hands on them. (I'm omitting front rounded vowels that don't occur in English).

	F1	F2	F3
i	225 -250	2000-2600	2800-3600
e	275 -300	2100-2600	2650-3400
E	350 -400	1900-2250	2500-3000
a	550 -650	1650-2100	2400-2850
>	350 -400	825-1000	1900-2600
o	300 -350	625 -750	2000-2600
u	225 -275	650 -850	2000-2300

Fischer-Joergensen says F4 is between 3300 and 4100 in unrounded vowels, and between 2900 and 3800 in rounded vowels. F3 is weak in the back rounded vowels. He suggests that the "distances between formant regions" is significant, presumably rather than the pitch of formant centers. This early speculation has been supplanted by an enormous amount of subsequent work to which I don't presently have access.

Bruce Nevin
bn@bbn.com

==========

Date: Wed, 4 Sep 1991 14:17:30 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AEROSPACE.AERO.ORG
Subject: Social Control

[From Rick Marken (910904)]

I'm back. Actually, I've been having a rather exciting little side fling with Martin Taylor. I successfully sent a copy of the excel version of my spreadsheet model of a hierarchy of control systems to him and he's got it up and running. I will try to post an ascii version of the worksheet to the net so that those who have a Mac with excel can try it out for themselves. I might also try to post the lotus version if anyone is interested. I don't have time at the moment but I should be able to post it by the end of the week. It will probably be a pretty big file so I'll post it to Gary Cziko who can distribute it to those who request it.

The speech discussion has been quite interesting but I want to try to start a new (or old) thread for us soft-headed members of the list who do not flinch at grandiose over-generalizations. This may be related to the request from Jack DeGolyer who says:

> I would also like to hear a definition
>of instruction from the control theory angle.

Whatever instruction is, I think is definitly not control. Instruction is an attempt to teach control systems to control -- it is a process of helping a control system develop "output functions" and "perceptual functions" that allow the learner/student/person to control the variables he/she needs to control.

One of the main problems people seem to have with teaching stems from the fact that the teachers are themselves control systems. Thus, teaching (parenting, etc) tends to be oriented towards producing a perception (for the teacher) of

behavior that is seen as the desired behavior (the behavior that constitutes "learned"). I think this creates enormous problems for the learner and the teacher.

I don't have time to go into it right now -- but I want to submit the following: people who are very skilled at controlling variables in the inanimate world may expect this skill to be transferable to their dealings with living systems. These skillful controllers who apply their efforts to the control of living systems are a major cause (if not the major cause) of inter and intrapersonal conflict in the world. How can we convince them that their control skills in the inanimate world will not (ever) lead to success in dealing with the animate world?

Hasta Luego

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)

==========

Date: Thu, 5 Sep 1991 08:11:40 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: book on speech

[From: Bruce Nevin (910905 0705)]

I finally remembered to go back down after getting the kids to bed and dig out my copy of

Lieberman, Philip, and Sheila E. Blumstein. 1988. _Speech physiology, speech perception, and acoustic phonetics_. New York: Cambridge U. Press.

Lieberman and Blumstein have their heads screwed on right. An excerpt I just accidentally hit upon this morning on the train (pp. 129-30):

> Experiments that make use of rapid perturbations of the vocal apparatus show that speakers have access to different patterns of automatized motor control that share a common goal--producing an acceptable acoustic signal. The proper execution of the bilabial stop consonant [b], for example, involves a speaker's momentarily occluding the supralaryngeal airway with his lips. Gracco and Abbs (1985) in an articulatory perturbation experiment used a small torque motor that applied a force to a speaker's lower lip. The speakers in this experiment were producing a series of syllables that started with the consonant [b]. The experimenters first used the motor to apply a force to the speaker's lower lip 40 milliseconds before the point in time that the lips would normally reach a closed position. The perturbing force would have prevented the proper execution of the bilabial stop consonant [b]. However, the speaker compensated by

> applying more force to his lower lip to overcome the perturbing force; the speaker also applied a slight downwards movement of his upper lip. The experimenters then applied the perturbing force 20 milliseconds before the normal lip closure. The speaker in this case compensated by a large downwards movement of his upper lip, extending the duration of the lip closure gesture. The speaker, in other words, used two different automatized motor patterns that had a common goal--closing the lips to produce the consonantal stop closure. The compensating motor activity for both perturbation conditions occurred within 40 milliseconds, far too short a time interval for cross-modal auditory feedback. The compensating activity must be initiated by afferent signals from the perturbed lip to the motor cortex and effected by control signals directly from the motor cortex. The compensating action involves goal-directed activity which can be effected by different patterns of muscular activity. The same speaker under different timing conditions compensates by mainly increasing the activity of his lower lip--moving it upwards--or by mainly moving his upper lip downwards. The speaker appears to have a neural representation in the motor cortex of an abstract linguistic _goal_, closing one's lips for a [b].

The citation is:

Gracco, V., and Abbs, J. 1985. Dynamic control of the perioral system during speech. Kinematic analysis of autogenic and nonautogenic sensorimotor processes. _Journal of Neurophysiology_ 54:418-32.

Lieberman is known for his persistent critique of prevailing Rationalist biologicism (species-specific innate neurological mechanisms for acquisition and use of language), cp. his 1975 book _On the origins of language_ and 1984 _The biology and evolution of language_, writings on child language acquisition, etc. For example, following the passage quoted above Lieberman adduces cross-linguistic (multi-language) evidence that the control systems involved here are learned rather than innate. Elsewhere in the book he points out that control of breathing characteristic of those born at high altitudes, by which they "breathe more effectively (LeFrancois et al 1969)" (p. 96) and consequently "need to take in 50 percent less air than you do when you and they perform the same task" apparently cannot be learned by adults after a cutoff age similar to that for acquisition of language. It is Lieberman who has done the work with primates demonstrating that their lack of language is at least attributable to their physiological inability to form different vowels due to the configuration of their vocal tracts. I mentioned this some while ago. Lieberman is at Brown. I am not familiar with Blumstein. The bibliography lists a number of things that she coauthored with Kenneth Stevens at MIT and others.

The book includes lots of data about formants, physiological variables, air pressure measurements, etc.

I recommend this book highly to anyone interested in modelling control processes involved in speech.

Bruce Nevin
bn@bbn.com

==========

Date: Thu, 5 Sep 1991 08:03:48 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>

Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Lieberman; Sound spectrograph

[From Bill Powers (910905.0745)

Bruce Nevin (910905) --

What a find! I'll get an interlibrary loan request in for Lieberman & Blumstein right away, if Ft. Lewis College doesn't have it (probbly not). Need I suggest that you write up a little essay on control theory and linguistics and send it to the appropriate place?

The sampling rate on my A/D program is now maxed out at around 8000/sec, so I have hopes of seeing, perhaps, F3. I can see now that these formants are really bands of harmonics of the fundamental. When I say "YOW", I can see that "triangle of vowels" you illustrated (F2). Next step: I need an amplifier for the microphone. I now have an assembler-language routine that maintains up to 500 filters in parallel and lays down the spectrum from left to right on the screen. Still a factor of 60 away from real time, though, even with only 100 channels. Maybe when the lowest harmonic is identified, the filters can then jump frequencies by harmonics -- that would reduce the requirements to only 20 or 30 channels, and maybe less if the filters can track on a changing frequency. It seems to me that voice inflections would greatly distort the shapes of formants -- there may be a way of removing the frequency variations of the fundamental. But first I need a cleaner audio signal -- I'm resolving only about 10 levels in amplitude without an amplifier. Off to Radio Shack.

Best

Bill P.

==========

Date: Thu, 5 Sep 1991 13:34:56 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Lieberman

[from Gary Cziko 910905.1300]

Bruce Nevin (910905 0705)

>Lieberman, Philip, and Sheila E. Blumstein. 1988. _Speech physiology,
> speech perception, and acoustic phonetics_. New York: Cambridge
> U. Press.
>
>I recommend this book highly to anyone interested in modelling control
>processes involved in speech.

This is quite amazing. I was just about to ask you for a book that would get me up to speed on this acoustic stuff. What is even more amazing, the person on my campus who(m) I am trying to get interested in doing some studies on controlling speech (and who has done mostly work on phonetic production) got her Phd at Brown University under, you guessed it, Philip Lieberman!

There is something I would like some quick clarification on, however. All

this talk of formants at specific frequencies has me confused. Perhaps my problem is that most of what I know about the physics of sound comes from my study of music, not language. In music we talk of overtones and timbre, and as far as I can tell they seem analogous to formats and vowels. But if a format is an overtone (or Bill would call it a harmonic) of a fundamental frequency, how can you peg it to specific frequency or band of frequencies? It would seem to be that it must be expressd as a MULTIPLE of a fundamental frequency. I can say /i/ in a low voice and in a voice an octave or so higher. Mustn't the formants move up the scale along with the fundamental frequency?

Please explain.--Gary

P.S. Earlier you said:

>Plug your ears and listen very closely for a high-pitched buzz
>descending over a low-pitched buzz. (A good place for this: take a bath
>and immerse your head in the bathtub, ears full of water.)

When I immerse my head in water, water rushes into my mouth when I try to say these vowels. This results in a gurgling/gargling sound which appears to have very strange formats. Also, I find that it is best to do this after showering first, otherwise it doesn't taste too good. There must be something I'm doing wrong here . . . glub, glub, glub . . .

==========
Gary A. Cziko Telephone: (217) 333-4382
University of Illinois FAX: (217) 244-0538
Educational Psychology Internet: g-cziko@uiuc.edu (1st choice)
210 Education Bitnet: cziko@uiucvmd (2nd choice)
1310 South 6th Street
Champaign, Illinois 61820-6990
USA
==========
==========
Date: Thu, 5 Sep 1991 14:56:51 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: sound spectrograph

[From: Bruce Nevin (910905 1435)]

(Bill Powers)

It is exciting to see this developing so rapidly.

Do you have availability/cost info on that real-time board?

I will send you a copy of the survey of speech analysis systems. It may give you some ideas. Lieberman and Blumstein will give you many many more I think.

Reading quickly, I have been reminded that work in the late '60s and early '70s (chagrined to say I had forgotten, but my focus was not on phonetics at the time) demonstrated that the supposed correlation of formant "coordinates" with tongue position breaks down on closer inspection (radiography). The tongue movements are in fact not so systematic as had previously been supposed from introspections of

phoneticians. Articulations, it appears more and more convincingly, are behavioral outputs rather than targets, and the reference values are acoustic, at least for vowels.

For consonants, it is difficult to see how one could produce a sound normally produced by constriction with the tongue within the oral cavity (t ch k d j g n ng l) other than with the part of the tongue that is usually considered the articulator for that sound. However, for [b] as described in the passage I quoted this morning the closure at the front of the oral cavity could be effected by other means, given sufficient interference with labial movement. I can even do it with the tip of my tongue between the lips (if they are close enough) and the sound is fairly convincing. Similarly for p and m. Articulating a w is harder, sounds "lispy," and is quite difficult with certain vowels adjacent, but might be doable with practice.

I do want to write a piece on CT and language. Thanks for the encouragement. Wish I had a laptop for the train!

Be well,

Bruce Nevin
bn@bbn.com

==========

Date: Thu, 5 Sep 1991 14:58:40 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: glub, glub

Bruce Nevin 9/5

(Gary Cziko)

I guess you figured out that my directions were incomplete, and that you should keep your face out of the water :-)

Yes, formants are made up of harmonics or overtones--the ones that are *not* filtered out by acoustic characteristics of the space between the larynx and the lips/nostrils. And yes, the formants do move up and down with the fundamental pitch. That is one reason why ranges are given. Lieberman and Blumenstein cite some data on man/woman/child voice characteristics that are interesting. One proposal about the function of semantically empty greetings like "hi there!" that are found (with different words, of course) the world over is that they give us an opportunity free of informational content to tune in to the fundamental and formants of the other's voice. To get a running start of language/dialect differences too, I suspect.

I have had this insistant fantasy intruding itself recently that I should give up on Penn and connect with Lieberman at Brown. He's certainly a lot closer to where I live. If the Penn department continues to be as frought with craziness, obduracy, and possible sadism, I may act on that fantasy. Should I get in touch with your friend to find out more?

In haste, the train won't wait and it's raining . . .

Bruce
bn@bbn.com

==========

Date: Thu, 5 Sep 1991 16:49:39 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Martin Taylor <mmt@DRETOR.DCIEM.DND.CA>
Subject: Re: Reference signals; Testing control; sonograph

[Martin Taylor 910905 16:35]
(Bill Powers 910904.0630)

Bill presented a long comment on my problems with the development of appropriate commensurability between perception and reference for an elemental control signal, starting as follows:

>
>
>Martin Taylor (910903.1212) --
>
>>I intended no committment as to the prior existence of the perceptual
>>signal, only that the reference and the perception that is to be matched
>>to that reference must have the same character.
>
>In a one-dimensional-signal model, the "character" of a perceptual signal
>is not carried in the signal. All the signal can do is increase and
>decrease in magnitude. The character of the perceptual signal is in the
>way it depends on combinations of signals at lower levels, and that is
>determined by the form of the input function. This is why the character
>of any isolated percept is subjectively so elusive, and in fact
>disappears after prolonged experience in isolation. It exists not in the
>experienced signal, but in the unexperienced neural calculations.
>

I'm not going to quote any more of it, because except for one point that I will quote, I was and am aware of and in agreement with all of what Bill says, except for the fact that Bill appears to see his comment as a response to my postings. I think we are talking at cross purposes, and I am not sure why. Possibly it has to do with differences in perception of what specific words mean (such as "character" above). I'm not going to try posting any more on this right now, because I think there is still a serious and deep discussion to be held, and I will not be around after next Tuesday until mid-October or possibly early November. I'll probably take it up again then.

The one bit I do want to quote is:

>
>The problem of setting appropriate reference signals that vary in an
>appropriate way and go to the appropriate subsystems is entirely that of
>the superordinate system. It is up to an event-controlling system to
>figure out how it must pattern its output signals going to each
>contributing configuration-control system. The configuration-control
>systems don't care what reference signals they receive. Only the
>superordinate system can know what is appropriate, and even it knows this
>only in terms of effects on its own perceptual signal. But I'm putting
>too much intelligence here into a single system: this is really a problem
>for the reorganizing system, which isn't even concerned with the
>variables that one control system becomes organized to control. I'm
>describing the problem that has to be solved, but I don't mean to suggest

>a smart little man sitting in each level, solving it.
>
You here provide a part of an answer to the question of "character" that I intended to ask. That answer is that the transform exists ENTIRELY in the weightings that relate the error signal for the super-ordinate control system to the various reference signals it tries to impose on the subordinate control systems. Nothing affects the interrelations of input signals, and nothing affects the relations of attempted reference signals from different superordinate control systems at the subordinate level.

It seems to me that this is a design choice, putting all of the problem of relating perception to reference into the lap of the superordinate control systems (or to the reorganizing system that they call on for aid when they can't effect their control). It is not intrinsic to the principles of hierarchic control systems. Is there a principled reason for it, that makes it other than an arbitrary design choice?

Martin Taylor

==========

Date: Thu, 5 Sep 1991 17:30:45 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Martin Taylor <mmt@DRETOR.DCIEM.DND.CA>
Subject: Re: Lieberman

[Martin Taylor 910905 17:10]
(Gary Cziko 910905.1300)

>
>There is something I would like some quick clarification on, however. All
>this talk of formants at specific frequencies has me confused. Perhaps my
>problem is that most of what I know about the physics of sound comes from
>my study of music, not language. In music we talk of overtones and timbre,
>and as far as I can tell they seem analogous to formats and vowels. But if
>format is an overtone (or Bill would call it a harmonic) of a fund{amental
>frequency, how can you peg it to specific frequency or band of frequencies?
> It would seem to be that it must be expressd as a MULTIPLE of a
>fundamental frequency.

No, formants are not harmonics. Harmonics are overtones. Timbre is related to the formants of the musical instrument (but has other aspects as well). Vowels are created by sounds that have a harmonic structure that is affected by several (4 or 5, usually) formants, though linguistically only 2 (or maybe 3) formants matter.

Any steady pitch can be analyzed into a set of harmonic lines--frequencies that are multiples of a fundamental. Not all of these lines have the same amplitude (unless the source is a precise train of impulses of infinitesimal duration). The relative amplitudes of the harmonics is in part a function of the source mechanism (the vibration of a reed in an airstream, for instance, or in speech the opening and closing of the vocal cords to interrupt the airflow out of the mouth). But in part the relative amplitudes of the lines are a function of the spaces surrounding the source, and in particular of the resonances of air spaces and material objects (such as the back plate of a violin, or the human oral cavity). Harmonic lines near the resonant frequencies are augmented relative to harmonic lines having no resonances in the instrument near their frequency.

The spectral shapes of these resonances are the formants. Typically,
a formant will have a width that covers several harmonic lines, so that
its shape can be readily seen by looking at a spectrogram that shows
only the lines. But F1 in humans is often near the second or third
harmonic of the fundamental (especially in females), and is narrow
enough that the next harmonic is not near its peak. So what you get
may be the second harmonic well down the low-side slope of the formant,
and the third harmonic well down the high-side slope so that it looks
as if the formant hardly exists (exaggeration here), or the (say) second
harmonic near the peak of the formant so that it is strongly augmented,
and the other nearby harmonics relatively suppressed.

In short, the harmonics come from the pitch of the source, the formants
from whatever the sound wave encounters after it leaves the source.
Formants affect the amplitudes of harmonics, but the centre frequency
and width of the formant has nothing at all to do with the frequencies
of the harmonics.

(By the way, timbre is affected greatly by the temporal character of the
onset of a musical note, as well as by the formants of the instrument.
If you electronically smooth away the onset transients, it is often very
hard to discriminate among instruments that you think have very
different timbres).

Martin Taylor
==========
Date: Thu, 5 Sep 1991 17:40:38 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Martin Taylor <mmt@DRETOR.DCIEM.DND.CA>
Subject: Re: glub, glub

[Martin Taylor 910905 17:35]
(Bruce Nevin 910905 14:36)

> And yes, the formants do move up and
>down with the fundamental pitch. That is one reason why ranges are
>given.

There's lots more reason than that, most of which the CSG group would
take as intuitive (the formant frequencies are not the reference signal).
Even for a single talker, the map of F1 v F2 for instances of a given
vowel overlaps the map for instances of another vowel. I was surprised
by the ranges Bruce quoted in an earlier post, because those ranges
showed no overlap. It is more nearly true to say that there is no
pair of values of F1 and F2 that can be uniquely identified with a
particular vowel (exaggeration, but truer than to say there is no overlap).

Martin Taylor
==========
Date: Fri, 6 Sep 1991 09:07:35 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: more on bees

The following is from

Genetic Algorithms Digest Thursday, September 5 1991 Volume 5 : Issue 27

-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-

From: ramsey@AIC.NRL.Navy.Mil
Date: Thu, 5 Sep 91 09:52:58 EDT
Subject: newspaper article of interest

I recently saw the following article in the Science section of the Washington Post and I thought the GA community might find it interesting.

-- Connie

Entomology: Low-Risk Strategy for Survival

Mutual funds or junk bonds? Blind date or the girl next door?

Risk-taking strategies determine the short-term well-being of individuals and the long-term evolutionary success of species. Bumblebees, Leslie Real reports in Science, appear to be a dramatic case of survival of the very cautious.

The University of North Carolina biologist constructed an enclosure in which bees had their choice of two kinds of artificial flowers: blue ones, containing 2 ml of nectar each; and yellow ones, one-third of which contained 6 ml of nectar and the rest nothing. Each flower type promised the same average amount of reward, but only the yellow was risky. Bees chose the no-risk blue 84 percent of the time, and made that decision after trying only a few individual flowers instead of waiting for a larger and, perhaps, more accurate sample.

What evolutionary advantage is there to this hasty and conservative strategy for "short-term energy maximization"? Limited bee-brain computing power may be a factor. But the system is also neatly adapted to the way food sources turn up in the highly competitive wild: Once a bee finds a promising flower patch, "it generally restricts its foraging to neighboring plants, ultimately exhausting the resources in that local patch," Real noted. So bees capable of making a fast decision will eat better. And since the best plants tend to occur in clumps, averaging over a large number of flowers would be counterproductive as well as harder to recall.

-- Curt Suplee

Bruce Nevin
bn@bbn.com

==========

Date: Fri, 6 Sep 1991 07:48:08 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: A/D Converters; Hierarchy

p
3$
Bruce Nevin (910509.1308) --

3$
3$
The A/D board I use is an old version of the Model AD200, from
3$
3$
Real Time Devices
3$
820 N. University Drive
3$
P.O. Box 906
3$
State College, PA 16804
3$
3$
(814) 234-8087
3$
(814) 234-5218 (FAX)
3$
3$
All Analog-input boards are 12 bits.
3$
3$
The AD200 is rated at 8 KHz sampling rate multiplexed, 0 - 5 volt input,
3$
4 channels ($235). It now includes 24 channels of digital I/O and
3$
several timer-counters as well.
3$
3$
The next model up (AD1000) samples at 25KHz, input range is -5 to +5
3$
volts, 8 single-ended channels (multiplexed), and costs $275. The top-end
3$
model (AD3100) samples at 200 KHz, has 8 single- or double-ended
3$
channels, several voltage ranges, programmable gain, and (like the
3$
others) digital I/O channels: $589.
3$
3$
Send for the catalogue -- it has many more goodies in it.
3$
3$
Here is my assembler code for the multiple-channel filter subroutine.
3$
This is set up for the Borland assembler, which takes care of all sorts
3$
of hard stuff for me. This is for interfacing with C programs.
3$
3$
Note: arrays alist[] and olist[] must be zeroed before first use of this
3$
routine. The routine is invoked for every data point in the audio
3$
recording ("input") without further zeroing of the arrays.
3$
3$
3$

```
.MODEL LARGE
3$
        .DATA
3$
thous    dw 1000
3$
        .CODE
3$
        public _multchan
3$
_multchan proc far
3$
        ARG delta:word,damp:word,
3$
            f0:word,input:word,         { don't really break line up}
3$
            alist:dword,olist:dword,nchan:word
3$
        push    bp
3$
        mov     bp,sp         ; foreplay required for C
3$
        mov     si,offset alist
3$
        mov     di,offset olist
3$
        mov     bx,0          ; starting frequency
3$
        mov     cx,nchan      ; initialize count
3$
start:
3$
        mov     ax,[si]       ; get intermediate integral value "a"
3$
        imul    damp          ;
3$
        idiv    thous         ; intermediate times damp/1000
3$
        add     ax,[di]       ; output
3$
        add     ax,input      ; plus input
3$
        imul    bx            ; bx contains frequency
3$
        idiv    f0            ;
3$
        sub     [si],ax       ;a=a-(input+output+a*damp/1000)*freq/f0
3$
        mov     ax,[si]       ; retrieve updated intemediate value a
3$
        imul    bx            ;
3$
        idiv    f0            ;
3$
        add     [di],ax       ; out = out + freq*a/f0 ("output")
3$
        add     bx,delta      ; bump frequency up
3$
```

```
        add     si,2          ; bump pointers
3$
        add     di,2
3$
        loop    start
3$
        pop     bp            ; what Theodore Sturgeon calls "afterward"
3$
        ret
3$
_multchan endp
3$
        end
3$
3$
The arguments passed to this routine are:
3$
delta:word,  ; Frequency step between channels
3$
damp:word,   ; damping for all channels, thousandths
3$
f0:word,     ; divisor setting frequency scaling, slightly < fmax/2
3$
input:word,  ; current amplitude of input signal
3$
alist:dword, ; address of first integration value
3$
olist:dword, ; address of second integration value, also output
3$
nchan:word   ; number of channels
3$
3$
alist and olist are arrays of 500 integers; nchan should therefore not
3$
exceed 500. The C calling sequence is
3$
3$
multchan(int delta, int damp,int f0, int input, int *alist, int *olist,
3$
        int nchan);
3$
---------------------------------------------------------------
3$
Martin Taylor (910905.1635) --
3$
3$
>-- the transform exists ENTIRELY in the weightings that relate the error
3$
>signal for the superordinate control system to the various reference
3$
>signals it tries to impose on the subordinate control systems.
3$
3$
As you say, we can get into this later. One of the interesting properties
3$
of a hierarchical control system is that the demands on the output
3$
(downgoing) transform are far less than they are on the input perceptual
```

3$
transform (upgoing). Control of oscillation would require, for example,
3$
an error-to-frequency converter (here is where the downgoing type-change
3$
occurs), and of course appropriate signs of connections to appropriate
3$
lower-order comparators, but this can be a crude transform. About all it
3$
needs to do is to make the output frequency change in the right direction
3$
in relation to error. The input function really defines the controlled
3$
variable and determines its specific form -- that is, non-linearities,
3$
weighted dependence on lower-level signals, and so on. If the loop gain
3$
is high enough, the exact form of the output transform makes no essential
3$
difference in the outcome, as long as it is of the right type and sign.
3$
The detailed weightings also make very little difference in the outcome
3$
-- all they can do is alter the side-effects of the control process on
3$
other control systems of the same (higher) level. This is very different
3$
from a top-down model, in which the output transforms determine
3$
everything. Now that you have Rick's spreadsheet model running, you can
3$
check these claims out for yourself.
3$
3$
 Part of our frustrating (and equally amusing) problem with getting our
3$
terminology to mesh comes from images that jar me. I'm not at all sure
3$
what you mean by saying that a higher order system "tries to impose" a
3$
reference signal on a lower-level system. What's to keep it from
3$
succeeding? All it has to do is output a signal; it has then contributed
3$
to the lower-level net reference signal. Do you mean "impose in such a
3$
way as to have a result that aids the higher-level control process?"
3$
3$
Is there still some suggestion of EXCLUSIVE use of a given lower-level
3$
system by a higher-level one? All the higher-level system could care
3$
about is that sending a reference signal to a lower-level system alters
3$
the higher-level perception in the direction that lessens error. A
3$
hypothetical reorganizing process in the higher system could work just by

3$
varying the signs of connections to lower levels (not bothering about
3$
weightings otherwise) and looking for an increase or decrease in the
3$
local error signal. When all downgoing signals reach all related lower-
3$
level systems, with signs that contribute to a lessening of error,
3$
control is achieved. This doesn't prevent other higher-level systems from
3$
also adjusting the same lower-level reference signals, in different
3$
patterns, at the same time, to control reasonably independent degrees of
3$
freedom of the lower-level world. No higher-level system "imposes" its
3$
reference signals on a lower-level one, to the exclusion of effects from
3$
other systems at the same higher level. Am I beating a dead horse?
3$
3$
>It seems to me that this is a design choice, putting all of the problem
3$
>of relating perception to reference into the lap of the superordinate
3$
>control systems (or to the reorganizing system that they call on for aid
3$
>when they can't effect their control). It is not intrinsic to the
3$
>principles of hierarchic control systems. Is there a principled reason
3$
>for it, that makes it other than an arbitrary design choice?
3$
3$
Yes, it is a design choice. One reason for it is that at any given stage
3$
of development, we have to have a complete working control hierarchy up
3$
to the highest working level (speaking both evolutionarily and in terms
3$
of single-lifetime learning and growth). Another is that despite our
3$
ability as adults to perceive and control in higher-level terms, we
3$
retain the abilities previously acquired at lower levels. Another is that
3$
a given level of control system has only a very specialized intelligence
3$
in my model: it deals in one class of perceptual variable, and its only
3$
function is to control variables of that type. To give it any knowledge
3$
of higher levels (which may not exist at a given time), we would have to
3$
find "antidromic" neural pathways carrying the required information, and
3$
also give the lower-level system the capacity to perceive that

3$
information in terms appropriate to the higher-level system rather than
3$
its own terms. It's too easy to smuggle intelligence into models of this
3$
kind; the only cure I know of is to try to model the intelligence,
3$
showing where everything that matters happens and how, in principle, it
3$
happens. To me, the ultimate test is whether the model can work in
3$
simulation (even if you have to fudge by just saying that the output of a
3$
box depends on its inputs properly without knowing how that could be
3$
accomplished. I don't know how perception of 3D orientation is
3$
accomplished, but I can put in a box whose output signal properly
3$
corresponds to orientation, which I happen to be able to see from my
3$
floating vantage point).
3$
3$
Nice clarification of harmonics vs. formants. From my newly-acquired
3$
authority as an experimenter in this field, I can state with confidence
3$
that you are correct.
3$
3$
>It is more nearly true to say that there is no pair of values of F1 and
3$
>F2 that can be uniquely identified with a particular vowel
3$
>(exaggeration, but truer than to say there is no overlap).
3$
3$
I like this. It says that we can have several control systems employing
3$
the same formants to control for different (orthogonal) variables that
3$
are different functions of F1 and F2.
3$
3$
Best to all
3$
Bill P.2
==========
Date: Fri, 6 Sep 1991 11:51:24 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: A/D ack and spreadsheet query

[From: Bruce Nevin (910906 1204)]

Bill Powers (Fri, 6 Sep 1991 07:48:08 -0600)

Thanks for the info on the A/D board. Currently payments on the family car and various other loans leave nothing for research equipment, but who knows? I'll send for the catalog.

Nice to know you're a Sturgeon fan too.

Rick:

I don't have Excell or Lotus, I have a copy of Lucid that a friend left on my PC. Do you have a description of what you have done from which one could build your model from scratch?

Bruce
bn@bbn.com

==========

Date: Fri, 6 Sep 1991 10:53:24 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: PCT and instruction

from Ed Ford (910906.1100)

(Rick Marken (910904))

>This may be related to the request from Jack DeGolyer who says: I
>would also like to hear a definition of instruction from the control
>theory angle.

and Rick then said:

>Whatever instruction is, I think is definitely not control.
>Instruction is an attempt to teach control systems to control - it is
>a process of helping a control system develop "output functions" and
>"perceptual functions" that allow the learner/student/person to
>control the variables he/she needs to control.

Both in instructing and in counseling, there is much to think about here. Beginning with the student, I think there are several reference signals that have to be operating here. First, does the student want to learn from me as a person? Second, does the student have a reference signal for the specific subject matter with which I'm dealing? Third, does the student have a higher order reference signal for a grade and thus, is what I'm teaching of less importance than the grade and thus the reference signal for the subject matter is only there as part of the major signal to "get a degree"? Fourth, how does the student perceive me in terms of a) my competence and b) my style of teaching and are those things compatible with the student's reference signals in those areas? Obviously, the above would apply differently at the various levels of education. At any rate, it seems there are lots of interrelated reference signals of various strengths that would be operating in the student's head when they sit in our classes. The bottom line for the teacher is hopefully to teach in such a way as to appeal to most of the living control systems' reference signals as possible.

With regard to counseling: A lot has to do with a) how the client

perceives the counselor as a person, b) how the client perceives the competence of the counselor and c) how the client perceives the role of the counselor in the counselling session. Eventually, I think clients have to perceive the counselor as a teacher, someone who will teach them how to deal more effectively with their life. What that means is that the counselor has to ask the clients questions in such a way as to help them examine their own internal control systems until they are able to identify their conflicts. Then the next job of the counselor is to teach them how to deal with these conflicts. Thus, clients should begin to perceive the counselor as someone who can teach them how to establish a plan to resolve one or more conflicts.

Since clients are unique living control systems who are in the counselor's office for help in restoring harmony to their internal system, the first order of business is to help them evaluate their systems at various levels. Are the decisions (at program level) they're making compatible with their values and beliefs (at system concepts), for example? Or, within the levels themselves (setting priorities at system concepts level, for example)? Once this is accomplished sufficiently, then the counselor gets them to compare their reference signals with their various perceptions with regard to how they perceive things including perceptual variables, the actions they're taking, the disturbances with which they're dealing, and the reference signal they've set. Once they've established the specific incongruities and have determined they want to work at resolving their problems (setting a reference signal to work at resolving the perceived conflict), it is generally at this point in time (as I mentioned above) that many begin to perceive their counselor as someone who could teach them a better way of achieving their goal (reference signal) or altering it in such a way that it is achievable.

With it comes to teaching others how to achieve goals, this assumes that the counselor is in better shape than the client, at least in the area of the problem presented.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, Arizona 85253 Ph.602 991-4860

==========

Date: Fri, 6 Sep 1991 15:14:30 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: Bruner: Child's Talk

[From: Bruce Nevin (910906 1427)]

Some time ago, before the CSG meeting in August, Clark McPhail very kindly provided me a copy of

Bruner, Jerome. 1983. Child's Talk: learning to use language. New York: Norton.

Unfortunately I haven't the bandwidth to review this fine and stimulating book properly. I would like to do what I can to encourage others to read it.

Bruner has been an important figure in the development of cognitive psychology. Best known to me is his 1950s (?) book _A Study of

Thinking_, and I am afraid that I know it but poorly, the memory of reading it is a bit wrinkled with age. He says in his introduction to the present book that he didn't begin studying language intensively until a stint at Oxford which began in 1972. He had been uninterested by the formalism of developmental linguistics (and linguistics) of the 1960s, by which he must mean predominantly generative linguistics. It was a shift to a more functional emphasis in some quarters at least that made the field more interesting to him. The book developed from various of Bruner's writings beginning about 1975.

The core of the book is a 2-year longitudinal study of two middle-class English boys from early infancy through about age 2. Every fortnight--more often if the parents told them something interesting was going on--two of his team (himself and two coworkers) visited their homes for about an hour and made a half hour of video and audio recordings, which they subsequently transcribed and analyzed in some detail.

> Very early in the enterprise, it was plain that children enjoy a privileged access to language, that the input to them from the linguistic community is systematically arranged [for them by the adults with whom they interact]. It was equally plain that children, in attempting to use language to achieve their ends, were doing far more than simply mastering a code. They were negotiating procedures and meanings and, in learning to do so, were learning the ways of the culture as well as the ways of its language. (p. 11)

Bruner shows how parents teach their children interactive games within whose restricted format they learn concurrently to master crucial aspects of how to conduct themselves as human beings of the sort that their parents are. He shows how games like peekaboo graduate--are graduated, rather, into language games in a Wittgensteinian sense, and how children learn from them the character of reciprocal (and exchangeable) roles, turn-taking, and exchange for the sake of the transaction rather than of the thing exchanged.

> One sets the game, provides a scaffold to assure that the child's ineptitudes can be rescued or rectified by appropriate intervention, and then removes the scaffold part by part as the reciprocal structure can stand on its own.

He shows how such "formats" support the growth of reference as the coordinated focus of attention in the two players, and the development of linguistic reference from babbling, to deictic expressions that are not identifiable as words but are already conventionalized expressions of the child's referential intent, to words used in referential sense.

> The problem of how reference develops can . . . be restated as the problem of how people manage and direct each other's attention by linguistic means. We may properly ask how _linguistic_ attention-management is superimposed on prelinguistic means and inquire as to how the first extends and modifies the second. . . . Linguistic conventions and standard forms do not leap full grown from the egg. They usually are slow transformations of initially primitive or "natural" procedures that become socialized in negotiation. (p. 69)
> Routinization of contexts would assure familiar, easily interpretable settings in which mother and child could locate or "place" objects and events to which they referred. (p. 70)

A fifth chapter traces the development of requesting, a form of language use that as Bruner points out is necessarily deeply enmeshed in context and in pragmatics. Again, there is the domestication of a "natural" act, of attempting to get something seen but out of reach, accompanied by noises of effort, into progressively more stylized and conventionalized gestures and utterances leading into appropriate use of language to make a request. Bruner discusses requests for objects, invitations to joint action (who could refuse?), and requests for help.

In all this, Bruner develops the theme sounded at the outset, that any putative biologically innate Language-Acquisition Device (LAD) cannot function without a socially given Language-Acquisition Support System (LASS).

> Whatever original _language_ endowment may consist of and however much or little of it there may be need not concern us. For whether human beings are lightly or heavily armored with innate capacities for lexico-grammatical langauge, they still have to learn how to _use_ language. _That_ cannot be learned in vitro. The only way language use can be learned is by _using_ it communicatively. . . . How indeed do we ever learn to get things done with words? (119-120)

> A principal vehicle of the [LASS] is what we have called a _format_. A format is a standardized, initially microcosmic interaction pattern between an adult and an infant that contains demarcated roles that eventually become reversible. They become . . . such familiar routines in the child's interaction with the social world that they are deserving of James Joyce's term, "epiphanies of the ordinary." They have a scriptlike quality that involves not only action but a place for communication that constitutes, directs, and completes that action. . . . In time and with increasing systematicity, formats are assembled into higher-order subroutines and in this sense can be conceived of as the modules from which more complex social interaction and discourse are constructed. In time and with increasing abstractness, formats become like moveable feasts. They are no longer tied to specific settings but can be "imposed" by illocutionary devices on a variety of situations. When they reach this more evolved form, they can properly be called speech acts in the Austinian sense. (121)

He does claim that the "intent to refer" is innate and not learned, as well as the capacity to recognize such intent in others, drawing on observations of animals among other things.

> Some basis for referential intersubjectivity must exist before language proper appears. Logically, there would be no conceivable way for two human beings to achieve shared reference were there no initial disposition for it. There is nothing more (or less) mysterious about this unlearned "othermindedness" than there is about the ethologist's contention that members of any species regard other organisms as conspecifics and act accordingly. It is a primitive that "other minds" are treated as if they were like our own minds. Another primitive is that there is a world "out there" that is shared by others. Human beings, I proposed, are born as Naive Realists, whatever other epistemological conclusions they may achieve later by reasoning. (122)

To this a priori argument on this point he also adduces empirical evidence. Given this "natural" basis, his focus is rather on why and how such activities are conventionalized in culture-specific ways.

> Our emphasis . . . has been upon _social_ processes that are shared by prelinguistic and linguistic communication. Certainly these processes (turn taking, role interchange, etc.) _do_ remain invariant across the change into language and provide a centrally important source of continuity. Indeed, I have even urged that the principal "motive" in language acquisition is the better regulation of these underlying social-cultural processes. (128)

> Context for the young child cannot be taken as a given, as simply "being there." Operative context, for the child or adult, is selected and constructed. The "rules" or criteria for its selection and construction will, of course, vary with the circumstances [and develop much as language acquisition develops].

Constructed contexts must be cognitively manageable (apt and concise rather than either inclusive or oblique). We must and do help our listeners properly to construe our utterances by various indications as to what kind of speech act we intend.

> A format is a contingent interaction between at least two acting parties, contingent in the sense that the responses of _each_ member can be shown to be dependent on a prior response of the _other_. Each member of the minimal pair has a goal and a set of means for its attainment. Each has the capacity to affect the other's progress toward the respective goals. The goals of the two participants need not be the same; all that is required is that the conditions of communal response contingency be fulfilled. (132)

This observation is I think important for understanding these matters in CT terms, and aligns well with distinctions we have made here between control and influence.

> Formats "grow" and can become as varied and complex as necessary. Their growth is effected in several ways. They may in time incorporate new means or strategies for the attainment of goals, including symbolic or linguistic ones. They may move toward coordination of the goals of the two partners not only in the sense of "agreement," but also with respect to a division of labor and a division of initiative. And they may become conventionalized or canonical in a fashion that permits others within a symbolic community (e.g., a "speech community") to enter the format in a provisional way to learn its special rules.

Judd, this is an important thing to ponder for SLA.

> Formats are also modular in the sense of being accessible as subroutines for incorporation in larger scale, long-term routines. A greeting format, for example, can be incorporated in a larger scale routine involving other forms of joint action. In this sense, any given format may have a hierarchial structure, parts being interpretable in terms of their placement in a larger structure. The creation of higher-order formats by incorporation of subroutine formats is one of the principal sources of presupposition. What is incorporated becomes implicit or presupposed. [And it or a

repetition in the larger context can often then be zeroed--BN]

> Formats, save when highly conventionalized, cannot be specified independently of the perceptions of the participants. In this sense, they generally have the property of contexts in being the resultant of a definition by the participants. The definition of formats communally is one of the major ways in which a community or culture controls the interaction of its members. Once a format is conventionalized and "socialized" it comes to be seen as having "exteriority and constraint" in Emile Durckheim's sense and become objective in Karl Popper's. Eventually, formats provide the basis for speech acts and their constraining felicity conditions. We learn how to invoke them by speech.
>
> One special property of formats involving an infant and an adult . . . is that they are asymmetrical with respect to the knowledge of the partners--one "knows what's up," the other does not know or knows less. Insofar as the adult is willing to "hand over" his knowledge, he can serve in the format as model, scaffold, and monitor until the child achieves requisite mastery. (133)
>
> . . . [F]ormats embed the child's communicative intentions into a cultural matrix; they are instruments for transmitting the culture as well as its language. Because formats have a sequential structure and a history . . . they permit the child to develop primitive concepts of aspectual time. At their simplest, they provide the child with a kind of manageable, middle-range future, defined by the course of the action rather than by abstract time or tense. Because they have an incorporative growth, they become important vehicles for the development of presupposition and for signaling presuppositions. Because they are finite, orderly, and interactive they also provide a context for interpreting what is being said here and now.
>
> Culture is constituted of symbolic procedures, concepts, and distinctions that can only be made in language. It is constituted for the child in the very act of mastering the language. Language, in consequence, cannot be understood save in its cultural setting. (134)

I am intrigued by the possible extension of Bruner's contextual formats into the information formats of sublanguage grammar, where only certain canonical sequences of word classes obtain.

I also speculate about the copying of control systems to new connections. Is it possible that, once a child has developed control systems of appropriate complexity and configuration to manage what is going on in a given social format, that kind of structure might be replicated by some internal control system-building function and tried out with other kinds of perceptions, then modified more readily and quickly than it was originally created, or than a new one might be created de novo? Might reorganization be nonrandom?

A stimulating book. I will be returning to it, I am sure. The simplicity and directness (nonabstractness) of its language belies the density and complexity of thought and experience that it represents.

Got to run.

Bruce
bn@bbn.com

==========

Date: Fri, 6 Sep 1991 13:07:50 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AEROSPACE.AERO.ORG
Subject: Spreadsheet

[From Rick Marken (910906)]

Bruce Nevin (910906 1204) asks:

>I don't have Excell or Lotus, I have a copy of Lucid that a friend
>left on my PC. Do you have a description of what you have done from
>which one could build your model from scratch?

Yes. You could build your own based on my description of the spreadsheet in the Behavioral Research Methods article (though Greg Williams might dispute that -- he thinks the article is pretty unclear. Go figure -- it was the only paper of mine that was instantly and enthusiatically accepted when first submitted). Again, the reference is:

Marken, R.S.(1990) Spreadsheet analysis of a hierarchical control system model of behavior. Behavior Research Methods, Instruments and Computers, 22, 349-359

I just posted a BinHex version of the Lotus 1.2.3 version of the spreadsheet to Gary Cziko to distribute to whoever wants it.(He should also have a BinHex version of the Excel spreadsheet as well). If Lucid is Lotus 1.2.3 compatible (many PC spreadsheets are) then you probably could read the Lotus worksheet into Lucid and run it.

Hasta Luego

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)

==========

Date: Fri, 6 Sep 1991 17:33:24 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: jbjg7967@UXA.CSO.UIUC.EDU
Subject: Re: Bruner: Child's Talk

[from Joel Judd]

Bruce Nevin (910906)

Ahhh...deja vu. What would PCT say--a virtually similar repetition of

perceptual inputs? This book came up just before you started corresponding on the net. A treasure trove of CT-related linguistic tidbits. It's great to know someone else who esteems Bruner's insights.

> The problem of how reference develops

All of this developmental description I think gives notice that language definitely deals with non-linguistic perceptions; they exist before language ability does

> Some basis for referential intersubjectivity must exist before
> language proper appears. Logically, there would be no conceivable
> way for two human beings to achieve shared reference were there no
> initial disposition for it. It is a
> primitive that "other minds" are treated as if they were like our
> own minds. Another primitive is that there is a world "out there"
> that is shared by others. Human beings, I proposed, are born as
> Naive Realists, whatever other epistemological conclusions they may
> achieve later by reasoning. (122)

It would seem that the questions of interest for PLA simply revolve around when and how perception at given levels of the hierarchy become functional:

> Indeed, I have even urged that the
> principal "motive" in language acquisition is the better regulation
> of these underlying social-cultural processes. (128)

At what level of sophistication does the 6-month old or 4-year old deal with these "underlying social-cultural" processes? How do we know? What controlled variables can be hypothesized for the developing child?

> And they (formats) may become conventionalized or
> canonical in a fashion that permits others within a symbolic
> community (e.g., a "speech community") to enter the format in a
> provisional way to learn its special rules.
>
>Judd, this is an important thing to ponder for SLA.

Pondered and a half. Think of all the baggage mature organisms bring with them to SLA. It's amazing, in some respects, that many do well in second or third languages--that they beome "native-like." On the other hand, why don't more people learning languages do well at it, especially when they are living in the L2 environment (migrant workers, refugees, etc.) and aren't ever going to return to their native/developmental environment? Obviously there are all kinds of factors that influence adult and even pre-pubescent SLA. So all we have to do is develop a sophisticated statistical manipulation that will take all these factors into account, and predict, based on various weightings of the factors, what the outcome of SLA will be for a given person.

See, SLA is simple: input factors ---> outcome. No problem.

>A stimulating book. I will be returning to it, I am sure. The
>simplicity and directness (nonabstractness) of its language belies the
>density and complexity of thought and experience that it represents.

If you liked that one, you'll love _Acts of Meaning_, which takes on psychology in general. Seriously, read it--it's the same length, and

synthesizes _Child's Talk_ and other things he's been doing for the past thirty years. The study he provides at the end is extremely provocative from a CT point of view.

I don't have a train to catch, but it's 5:30pm on a Friday and I'm controlling for 'weekend' and 'dinnertime.' 'Scuse my cynicism.

==========

Date: Sat, 7 Sep 1991 07:41:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: TJ0WAH1@NIU.BITNET
Subject: Page numbers

[from Wayne Hershberger]

Joel (910830)

>Wayne or Rick, if you have a copy of _Volitional Action_ text
>handy, or just happen to know, would you please send the page #s
>of Rick's article directly to Gary Cziko? Our library's copy is
>N.A. Thank you. Joel Judd

Rick had 2 chapters in Volitional Action: Conation and Control., with W. T. Powers being the second author on one:

Behavior in the First Degree. pp. 299-314.

Levels of Intention in Behavior. pp. 409-430. (with Powers)

Sorry for the delay Joel, I just now downloaded my E-mail for the last several weeks.

Warm regards, Wayne

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

==========

Date: Sat, 7 Sep 1991 13:08:13 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: experiments with phonetic control

[Martin Taylor 910907 13:00]

It seems that some part of the experiments we have been discussing may have been done. I was typing up the minutes of an international committee on speech processing, of which I am the secretary, and came across the following, which I had forgotten:
The Dutch delegate mentioned experiments on the Lombard effect by Chollet at ENST [in Paris, I think: MMT]. The claim is that speakers tend to shift their average spectra toward that of the noise [which would improve the average intelligibility, or at least maximize the transmitted information in the speech signal: MMT]. The German delegate [who is a choral director in

his spare time: MMT] said that singers do the opposite, matching their formants to the notches in the spectrum of the accompanying orchestra [which would make their voice stand out without maximizing information: MMT].

I have no more information than this, but it might be possible to obtain some, since I will be at the next meeting of this group at the beginning of October. But it does seem that environmental influences cause compensating behaviour in talkers and singers, and that the reference signal is not necessarily the same when talking for communicative purposes and when singing for musical aesthetic purposes.

Martin Taylor

==========

Date: Sat, 7 Sep 1991 15:22:04 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "by way of Gary A. Cziko g-cziko@uiuc.edu" <marken@AEROSPACE.AERO.ORG>
Subject: Spreadsheets

[From Rick Marken (910906b)]

To whom it may concern:

I just got word from Gary Cziko that he has the BinHex versions of both the Excel and Lotus versions of the control hierarchy spreadsheet model. He asked me (well, actually, he told me) to announce the availability of these spreadsheets to anyone who wants them. Gary said he successfully decoded and ran the excel version. The Lotus version should work OK too. All you need in order to use these programs is 1) the appropriate spreadsheet (Excel for the Mac or lotus 1.2.3 or compatible for the PC) and 2) a program
that will allow you to translate the ASCII file back to binary (a BinHex decoding program such as Stuffit 1.5.1 on the Mac).

If you would like a copy of the spreadsheet model, send your request directly to Gary Cziko (NOT TO CSG-L). Gary's address is

g-cziko@uiuc.edu (Internet -- 1st choice)
cziko@uiucvmd (Bitnet -- 2nd choice)

Once the spreadsheet is up and running, I would be happy to answer questions
about how it works, experiments you can try, etc. You can post these questions to the net or to me personally. The spreadsheet provides a nice, tangible look at the operation of a moderately sophsiticated hierarchy of control systems.

Gary is pretty excited about this method of sending programs via e-mail. I am too. This makes it possible, for example, to send executable versions of programs (such as my mindreading program) to anyone with a computer that can run the code. No more unsightly disk mailers. Just electronic blips and blaps. What a world.

Hasta Luego

Rick M.

****

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

==========

Date: Sun, 8 Sep 1991 02:04:20 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Peter Cariani <peterc@CHAOS.CS.BRANDEIS.EDU>
Subject: Interspike interval codes in the auditory system
In-Reply-To: POWERS DENISON C's message of Sun, 1 Sep 1991 19:37:55 -0600

Interspike interval vs average discharge rate codes in the auditory system

It's been interesting to follow the CSG discussion of the encoding of speech in the auditory system -- I'd like to add some points regarding the neural encoding of these complex waveforms.

For the last year I've been studying the temporal patterns of discharge of the auditory nerve array and some regularly-firing neurons in the cochlear nucleus. While I am new to the field of auditory neurophysiology and don't yet have a clear picture of where most of the field stands on these issues (i.e. I haven't been to many conferences yet), I can report what I have read in the literature and what I have observed over a dozen or so experiments recording from single neurons of the auditory nerve and cochlear nucleus.

The classical debate over the neural code in auditory physiology is usually characterized as being between "place" theories and "temporal" theories. Place theories essentially say that each auditory nerve is a specific "labelled line" -- it conveys by its connectivity to other neurons more centrally what the nature of its information is. In contrast, temporal theories usually emphasize the fine temporal patterning of discharge of each neuron, and the way that these timing patterns sum over the entire auditory nerve array. Usually the form of the place theory that is presented is the rate-place theory, where the average firing rate of each neuron indicates roughly the amount of energy present at frequencies near the auditory nerve's most sensitive frequency (its "characteristic frequency", or CF). This presentation of the debate is an oversimplification-- you could have localized analysis of interspike intervals (temporal-place theory) for example, as a reasonable alternative to the standard dichotomy.

Some of the major difficulties with a rate-place theory of speech recognition (or of any complex acoustic form for that matter) are:

1) 2/3 of the auditory nerve array is composed of fibers having high spontaneous rates of discharge (20-200 spikes/sec); most of these are saturated (firing at maximum rates) at 20-30dB above their thresholds, so at sound levels normally encountered (say 60 dB SPL) in speech situations, most of the array is completely saturated. To save the theory, one would have to postulate that somehow the "central processors" pay attention to low-spontaneous rate fibers at high and moderate sound levels and to high-spontaneous rate fibers at low sound levels. (High SR fibers generally have low thresholds while low SR fibers generally have higher thresholds). The rate

profiles of any one group of fibers does not seem to yield a robust indication of spectral pattern (of say a vowel) over the requisite range of sound pressure levels.

2) Many of the recognition tasks that we perform are relatively immune to various kinds of perturbations in the spectral domain. We recognize a particular vowel whether it is spoken by a man, woman, or child; we can separate particular instruments out of a small ensemble even if we have never heard those instruments playing those particular notes at the given intensities. Rate-place models essentially must posit a very sophisticated central pattern recognizer to make spectral templates for each speaker/instrument to decide how much of the energy at each frequency is associated with each source. Rate-place theories have a relatively harder time explaining why chords played on different ends of the keyboard nevertheless sound similar or why periodically repeated noise (or clicks) should have a pitch.

3) In the 1950's Licklider did some experiments with speech intelligibility where the acoustic waveform was clipped off at the peaks. If one does "infinite peak clipping", i.e. just keeping the zero-crossing information, the speech is still something like 95% intelligible. It's hard for me to visualize how a rate-place theory could deal with this -- it seems very suggestive that the auditory system is doing an analysis in the time domain rather than the frequency domain (Licklider at the time suggested that this analysis could be accomplished by an array of autocorrelators. His theory, which explains a wide variety of periodicity pitch phenomena, has been recently advocated in a paper by Meddis et al (Journal American Acoustical Society, May or June 1991). I believe there are whole populations of units in the cochlear nucleus ("choppers") which could perform a running auto-correlation on their inputs by firing more regularly (in synchrony with one another) when driven by a particular periodicity, thereby implementing a Lickliderian processing scheme.

4) Interspike intervals are much, much more robust over the entire range of normally-encountered sound levels than average discharge rates. Each auditory nerve fiber will produce interspike intervals which reflect periodicities in the stimulating waveform; the closer the particular stimulus periodicity is to the characteristic period (or maximally-resonant period) of the particular hair cell/auditory nerve, the more of such intervals will tend to be produced. Normally, "tuning curves" for auditory nerve fibers are discussed in terms of the discharge rate vs. frequency, but these cells also have "synchrony" tuning curves which are for a given intensity much broader -- even fibers with characteristic frequencies far from those frequencies dominant in the stimulus may show interspike intervals reflecting stimulus periodicities. In particular, the fundamental period (pitch period) of complex stimuli can usually be seen in interspike intervals across the entire nerve array. There is a nice paper by Cam Searle and Hugh Seker-Walker in the Journal of Acoustic Society of America (I think it's December 1989) which shows the discharge patterns across the auditory nerve array as a function of time. The interspike interval 1/F0 is found everywhere in the array. Intervals coresponding to 1/each formant frequency are dominant in the corresponding part of the array. Where these intervals are shorter than sustained physiological firing rates (say 200-500 spikes/second), one can see the intervals when several fibers having similar CF's are grouped together (a "volley principle") or when multiple presentations of the same stimulus to one fiber are grouped in a post-stimulus time (PST) histogram. The interspike interval account is also consistent with speech intelligibility studies where noise is used to

mask various parts of the spectrum. Interspike intervals reflect stimulus periodicities ("phase lock") up to 5kHz (above 1 kHz phase locking steadily weakens), and this is the critical range needed for speech recognition and various periodicity pitch phenomena.

In our (Bertrand Delgutte and I) own experiments on the pitch of complex stimuli (single-formant, sinusoidally amplitude-modulated tones, sinusiodally-amplitude modulated noise and others), the interval patterns in the array correspond very closely to the pitches that are perceived in the equivalent psychophysical experiments. The results appear to me to be quite striking; the dominant interval in the nerve array for many different kinds of stimuli is the perceived pitch interval, and the relative predominance of one particular interval corresponds with the strength of the pitch percept (for amplitude-modulated noise, which has a noticeably weaker pitch, the interval peaks are not as high (relatively speaking) as with the other stronger-pitched stimuli. In comparison the rate information is very noisy, spotty, and not very robust.

I think discharge rates may be used to distinguish changes in sound level, but probably fine-timing (interspike interval distributions) are utilized for analysis of auditory form. It's possible I'm jumping the gun here -- after all the auditory efferent system might be much more powerful and selective than anyone guesses -- it's not just a gain control on the array; each little frequency band has its own cochlear amplifier and a gain on that amplifier which is controlled by units specific to that frequency. It's probably a multi-dimensional control loop.

I believe that the interspike intervals corresponding to the pitch period are the commonalities across neural arrays which allow the various isolated percepts (e.g. formant intervals) to be "bound" together into a gestalt (vowel), much in the same way that there is correlated firing in the visual system binds different parts of the same object together when the object moves. This is obviously only a working hypothesis, but it is consistent with vowel-confusion studies where subjects are asked to distinguish two different vowels having the same fundamental period (and in trying to separate out the timbres of 2 instruments playing the same note).

I hope at least some of this is intelligible. I think the upshot of it all is that the cochlea can be looked at in 2 ways: first, as an array of narrow-band filters each of whose output is an average discharge rate (this might be good for intensity assessment and spectrally-based localization tasks, but it's poor for auditory form recognition). Second, the cochlea can also be looked at as an array of wide-band temporal filters (autocorrelators) each of whose output is a distribution of interspike intervals reflecting relative magnitudes of various stimulus periodicities and the resonant periodicities of the particular hair cell/auditory nerve. This kind of processing might be good for auditory form recognition, but poor for spectral analysis. This second mode of functional organization also enables multiplexing, since the inter-spike interval distributions are multidimensional entities. I think maybe spatial frequency channels in the visual system might be similarly organized into temporal discharge patterns and decoded with autocorrelators.

Interpretation of interspike intervals: cellular autocorrelators

---

While it is true, as Bill Powers pointed out, that once you get past the synapses things might look analogical again, there is still the question of

what kinds of elements are looking at the analog voltage fluctuations in the trigger region. If the recovery process of the neuron is a simple, monotonic decay of a threshold back to its resting value, one gets one kind of temporal behavior. If the threshold recovery is not monotonic (i.e. there are periods of enhanced excitability relative to rest), then the cell has a kind of electrical resonance -- it will have a preferred temporal behavior. A superexcitable phase has been observed in a wide variety of axons and cochlear hair cells also show electrical resonances. There are many, many kinds of repetitively-firing neurons and many kinds of complex recovery processes that are possible and/or have been observed experimentally.

Functionally, it makes a big difference whether we see the neuron as an analog integrator vs an analog autocorrelator or some other kind of elaborate timing element. The general principles of control theory hold in either case, but the relevant signalling/control variables will be very different and may have vastly different interactive properties. For example, coupling by oscillators allows for phase-insensitive asynchronous coordination because the interaction of periodic processes will produce periodic interference (as in beating between 2 tones) regardless of the relative phases of the 2 processes. This is very different from a coincidence-based coupling where precise timing is all-important.

A long discussion, to be continued......

I'm very interested in reactions to all of this, but I am going to a conference in Spain on the cochlear nucleus on Wednesday and will return on September 22, so I won't be able to respond in depth for a couple of weeks. The discussion thus far has been quite interesting, and I've learned quite a bit from you all. It's difficult for me to keep up with the pace and the sheer volume, but I'll try.

Peter Cariani 9/9/91

Dr. Peter Cariani
Eaton-Peabody Laboratory of Auditory Physiology
Massachusetts Eye and Ear Infirmary
243 Charles St
Boston, MA 02114

email: eplunix!peter@eddie.mit.edu
tel: H: (617) 524-0781
W: (617) 573-3747

==========

Date: Sun, 8 Sep 1991 09:49:56 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: saturn.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: anti-PCT attitudes

From: David Goldstein
To: people of CSGnet
Subject: question
Date: 09/08/91

The following statements are meant to represent some anti-PCT

attitudes which I have encountered. How could they be answered?

Proposition: PCT seems to say that it is part of the basic nature of people to be selfish. PCT is an elaborate rationalization for why it is better to take care of number one. Given a conflict between self/others, PCT says we must choose self or suffer intrinsic error signals. Conclusion: Why fight it, be selfish and be happy!

A related idea: If something that person A does or says has the result that person B has bad feelings about, that is the responsibility of person B. What person A does/says is only information. It is neutral. It is the contribution of person B which creates the bad feeling. Conclusio
ng which person B does
;~not understand, that is the problem of person B. What person A said must make sense to person A. If it makes sense to person A, then it is the fault of person B for not understanding it. Person B is deficient in some way and is responsible for the lack of understanding. Conclusion: Explain things the way you want to. If some others don't understand you, oh well! If they want to understand you, they have to make the effort. This is especially true for journal editors who receive PCT based manuscripts.

What sort of anti-PCT attitudes have you encountered? I would be interested in hearing them.

==========

Date: Sun, 8 Sep 1991 09:57:25 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: saturn.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: resend of anti-PCT attitudes

From: David Goldstein
To: people of CSGnet
Subject: question
Date: 09/08/91

The following statements are meant to represent some anti-PCT attitudes which I have encountered. How could they be answered?

Proposition: PCT seems to say that it is part of the basic nature of people to be selfish. PCT is an elaborate rationalization for why it is better to take care of number one. Given a conflict between self/others, PCT says we must choose self or suffer intrinsic error signals. Conclusion: Why fight it, be selfish and be happy!

A related idea: If something that person A does or says has the result that person B has bad feelings about, that is the responsibility of person B. What person A does/says is only information. It is neutral. It is the contribution of person B which creates the bad feeling. Conclusion: Say/do what you want to other people. If they don't like it, tough! They are causing their own bad feelings.

A related idea: If person A says something which person B does not understand, that is the problem of person B. What person A

said must make sense to person A. If it makes sense to person A, then it is the fault of person B for not understanding it. Person B is deficient in some way and is responsible for the lack of understanding. Conclusion: Explain things the way you want to. If some others don't understand you, oh well! If they want to understand you, they have to make the effort. This is especially true for journal editors who receive PCT based manuscripts.

What sort of anti-PCT attitudes have you encountered? I would be interested in hearing them.

==========

Date: Mon, 9 Sep 1991 13:52:39 cdt
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "McClelland,Kent" <MCCLEL@GRIN1.BITNET>
Subject: Modeling multiple control systems

[From Kent McClelland]

I've been back on the net for a couple of weeks but am still working on catching up with the logfiles from messages I missed in August. As a sociologist I've been quite interested in the thread on "Social Control" which began that month. Overall, I find the position outlined by Bill Powers, Rick Marken, and Bruce Nevin to be generally persuasive, particularly their skepticism about the existence of social "control systems" which operate in the same way as the control systems in an individual. On the other hand, my sociological training gives me some sympathy for the opposing point of view, the notion that social conventions have a "reality" external to individuals, as argued for example by Eric Crump (910820).

In spite of my general agreement, I wonder if Bill is perhaps stating his case too strongly when he almost makes it sound as if the social environment allows people unlimited degrees of freedom to do whatever they please in any situation. For example (Powers, 910821)

>The cop peering in through your car window could be a
>liberal or a Nazi. He could be following the book, interpreting the book
>or looking for a contribution to a worthy cause. He might cite you for
>speeding or for not having an emissions sticker, or both, or neither.
>That's up to him, not to the System. . . .

No doubt, the highway patrolman who pulls you over might decide to have a nice chat with you about the weather, or decide to beat you half to death with his nightstick, or do anything in between, entirely as the spirit moves him. Nevertheless, I feel quite confident in predicting that no highway patrolman will ever pull you over to give you a big kiss on the cheek. Some things, I would argue, are truly out of bounds in given situations.

In other words, while the social environment is surely not one big negative-feedback system, some or all of the people who constitute a person's social environment cooperate to impose organized disturbances which then place limits on the range of reference values the person can bring under control. This social constraint happens in much the same way as, to quote Bill (910821), "The physical environment also introduces constraints, but not purposive constraints." Kissing cops are nearly as improbable as pigs with wings. One important difference, however, between the social and physical environments is that some social constraints ARE purposively imposed by at least some of the people participating in the social environment. This

constituent purposefulness tends to make the social environment more complicated to describe than the physical, and it may also be the source of our illusion of social control.

In a later message (910822), Bill once again exhorts people interested in these issues to work on devising plausible models. With that goal in mind, I have a modeling question. First let me sketch in some background. We know from numerous tracking demonstrations by Bill, Rick, Tom Bourbon, and others, that the actions of a complex hierarchically organized set of control systems (a human being) can be modeled with great accuracy as a single control system, when the task is as simple as keeping a cursor in line with a target on the computer screen.

One of the demonstrations that Tom had set up at Durango allowed two people to work together on same tracking task, and my impression from that demonstration was that the joint actions of the two people could also be modeled with great accuracy as a single control system, at least as long as the two people were in agreement on the reference level for the task. To an outside observer, the movement of the cursor on the screen seemed about the same, only a little more precise (higher gain?) when two people were working together on the task than when one was working alone. When I was one of the people involved, the task seemed not to change, just get a little easier.

My question is this: Under what conditions can two (or more) independent control systems, working in parallel in the same environment, be modeled as a single system? How much discrepancy in reference levels, disturbances, system gains, speed of response, and the like are possible before the outside observer would need to posit two (or more) control systems at work instead of one in order to model their joint behavior? How would you devise a test for whether two independent simultaneously operating control systems had the same or different reference levels?

I suspect that if we could specify the conditions under which independent control systems can "cooperate" to produce behavior indistinguishable from one "super" control system, we would have made a start toward resolving the "social control" issues discussed in August.

Am I on the right track?

Kent

Kent McClelland Office: 515-269-3134
Assoc. Prof. of Sociology Home: 515-236-7002
Grinnell College Bitnet: mcclel@grin1
Grinnell, IA 50112-0810

==========

Date: Mon, 9 Sep 1991 12:51:41 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: UPROBER@BOGECNVE.BITNET
Subject: keeping track of posts?

[From Dick Robertson]
Does anyone have a good method of indexing past posts? Several times recently I have wanted to find something I saw last spring. But it requires going back through all of the files I have from that time.
Gary, is there a way of getting an index of the titles that went on a the net

in a given month? Thanks to whomever has a good solution for this problem.

Dick Robertson Dept of psychology Northeastern Il U
5712 Harper Ave. Chicago, IL 60637 (312) 643 8686 uprober@bogecnve

==========

Date: Sun, 8 Sep 1991 13:37:08 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Neural signals: frequency vs interval

[From Bill Powers (910908.1200)]

Peter Cariani (910908.0008) --

You and I are probably having some of the same problems that Martin Taylor and I are having -- people from different backgrounds use words that have different networks of associations, so while the central meanings get by all right, some of the peripheral meanings -- contexts -- are dissonant, creating errors ("I wouldn't have said it that way"). Too bad we don't have some sort of minterm expansion for ordinary language, so we could lay out all our meanings in some "primitive" form, as Bruce Nevin would say, without zeroings.

The contrast between interspike interval and frequency measurements is apparently an illustration of this point. You describe the "frequency" interpretation in several places as referring to the "average firing rate" of a neuron. This might or might not represent my position, depending on what averaging time you're talking about.

In my mind, the appropriate averaging time depends on what happens next in the signal path. When an excitatory impulse crosses a synapse, the mean concentration of the internal (dendritic) messenger molecules is given an upward jolt that decays at a rate depending on diffusion, chemical combination and recombination rate, etc.. The concentration rises again at the next jolt, and so on, decaying between jolts. The mean concentration comes to equilibrium when the rate of loss equals the rate at which new jolts arrive. This is a smoothing filter from the frequency standpoint. The chemical signal contains a DC component with a ripple on it. The DC component corresponds to the mean rate of impulse arrival. How big the ripples are depends on the rate of arrival of impulses. The envelope decay time (when incoming impulses cease) depends on the loss rate and not the impulse frequency; the rise time (when impulses abruptly begin arriving at a constant rate) should be essentially the same. So if we settle on the traditional description of rise-time, the time required to come within 1/e of the final amount of change, we can pick this as the "principled" averaging time for measuring impulse rates.

This averaging time will be pretty short: for a neuron with a maximum firing rate of 1000/sec, it would be around 0.01 sec or so (that's a ballpark guess; I don't know the actual numbers). It's probably nonlinear in that decay rate depends nonlinearly on signal amplitude. So this is roughly the time-scale on which we would plot envelopes of varying firing rates. This would imply that the bandwidth of a varying neural signal would cut off at roughly 30 Hz -- that is, the envelope amplitude would drop to about 0.7 at this frequency of variation of firing rate (relative to the low-frequency envelope). There would still be a response at higher frequencies, but the attenuation would increase with envelope frequency.

I'll try to say "envelope frequency" to distinguish between rates of variation in impulse rates and the impulse rates themselves.

With respect to auditory computers, therefore, the envelope approach can't account for computations that do things like mix two signals having impulse frequencies in the auditory range. Here we MUST have impulse-by-impulse interactions. Your point about non-monotonic approach of thresholds to the firing point is probably germane here: the "ripples" on the chemical signal must be significant for this to be an important effect, which implies in turn that the decay of chemical messenger concentrations must be very rapid indeed in these neurons. When the significant time-scale shrinks to the order of the minimum inter-impulse interval, a simple analog model of the neuron won't work any more.

So the conceptual model of the neuron that informs my thinking really applies only when we get to a level where auditory information is abstracted to the point where a envelope-frequency bandwidth of 30 Hz would be sufficient to carry it. I think that this is about what we see in sonograms -- that is, in the time-course of behavior of the various formants (not, of course, in the filters that resonate to formants). I can adjust the decay speed of the rectifiers in my sonograph program; it can get pretty long (more than 1/20 sec) before there is visible smearing of the changes. So I feel confident that computations applied to the time-course of formants can take place in neural computers in which frequency of firing is considered the signal-carrier and momentary envelope magnitude the signal.

The above discussion might help draw the line between the levels of processing in which your interval-based model and my frequency-based model are appropriate.

The next thing I'm going to try with my sonograph program is to set up tracking filters instead of just a fixed set. I am definitely NOT trying to think of a neural model that could do all this. I'm just trying to find a perceptual function that might work.

From what I'm seeing, all the information below about 1 KHz occurs at harmonics of the fundamental frequency -- and there's a subjective impression that vowels that sound the best-formed tend to maximize the energy at rather than between harmonics imposed by the mouth/nose cavities. I can almost feel the formants locking in at the frequencies that resonate the best.

If the program can recognize the lowest frequency (F0) quickly enough, it can use this basic frequency as a reference-frequency (not in the CT meaning) in a phase-sensitive detector, the smoothed output of which can then alter the frequency of the filter to center it on the fundamental. This same reference frequency, applied to tracking filters with ranges successively higher in the array of harmonics, can be used for tracking the higher harmonics, too, with phase relations preserved modulo F0. The outputs of the comparators of the tracking filters, that do the frequency adjusting, can then be used as the perceptual signals representing the frequencies at the higher harmonics (that is, the envelope-signal frequency will be the analogue of auditory frequency, although the auditory frequencies themselves will not be present in that signal). Another signal extracted from each filter will represent the amplitude

(separately) for each filter. This will take us out of the auditory frequency domain and into a domain where low-frequency envelope-signal variations now represent the content of the auditory signal at the various harmonics. The voice pitch will also have been removed, through relating everything to harmonics of F0 (or the voice -- I don't know which it will be yet). That pitch, of course, can be recovered from the correction signal of the fundamental-frequency tracking filter: it will be carried in yet another perceptual channel.

I don't know how much of this design is actually going to work as I visualize it now. At least the target is clear: to reduce the sonogram to separate information channels, each carrying information that varies on the time-scale of sonogram changes. This will accomplish, if it works, what Martin Taylor said is necessary: to find abstractions from the raw sensory signals suitable for processing at higher levels of perception. Once those low-frequency "envelope" signals are available, the problem becomes much like that of visual pattern-recognition, which is also a low-frequency phenomenon. I haven't any idea whether the result will be something like the way the human auditory system works: at this point, finding ONE way that works would make me smile.

I'm probably biting off more than I can chew here, but maybe out of my ignorance of well-beaten paths some novelty will arise that others can put to use.

Peter, a question about those auditory fibers that are spontaneously "saturated" at all times. This basically doesn't make much sense to me, if you mean the same thing I do by "saturated." To me, a saturated neural signal would be one that is carrying impulses as rapidly as possible all of the time, so that the frequency of firing is jammed up against the upper limit. I don't see how such signals could perform any function except perhaps as reference oscillators. Do you really mean that 2/3 of the auditory neurons are firing at a fixed frequency, as fast as they can? This is a very suggestive fact, if your description means what it seems to mean. We could be talking about a heterodyne system or single-sideband system here, which could bring various input frequencies into a common output frequency band (in separate channels, of course). This would mean that the next layer of processing could consist of elements with a single common design, receiving signals all of the same character, which would be lovely.

Enough for a Sunday.

Best

Bill P.

==========

Date: Tue, 10 Sep 1991 01:02:00 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Dag Forssell <0004742580@MCIMAIL.COM>

Subject: CSG book, CS demo, PCT attitudes

[from Dag Forssell]

(for Dick Robertson & Bill Powers):

I have just read Intro to Modern Psychology, which I bought at conference last year. I find it outstanding! (Have already ordered more from Greg Williams).

The introduction on paradigm shifts is good. I reported on "Discovering the Future: The business of paradigms" by Joel Arthur Barker (1-800-328-3789) and his inspiration "The Structure of Scientific Revolutions" by Thomas S. Kuhn (U of Chicago press 1970). Now I find that you are way ahead of me. Today I ordered Kuhns book. Less than $10! What took me so long? All of us promoting PCT are in the business of changing paradigms, and should make these books part of our core library.

I expect to find the source for the background info on gravity here. Right? As I write this I can't even find (but am reasonably sure here is where I read it) the statement that people used to think that objects fell at different speeds over land and over water. Each having nothing to do with water flowing downstream in rivers. Obviously there was no connection at all to the movements of the heavenly bodies! The paradigm of gravity created by one man and slowly accepted (by that powerful control system we call society? - or by persuading many individuals!) changed all that.

I find this a very useful conceptual analogy / introduction to William T(ewton) Powers paradigm.

The thorough description of the levels helped me! The definition of events as a perceptual construct, having nothing to do with the physical world answered a question I raised at the conference.

The discussion of principles strengthens me in my understanding of how many principles (clearly so in the natural sciences) add up to system concepts and (in softer areas) beliefs.

Bill is quite explicit that values (moral principles) such as honesty belong at principle level, not system concept level.

To me these matters are significant as I plan to explain to my students that a persons system concepts / beliefs (after the person has grown up and also reviewed them for validity and consistency) determine the persons values, moral principles (which are chosen or deduced given your system concepts / beliefs).

The chapter on physiology and all of part three is also much appreciated.

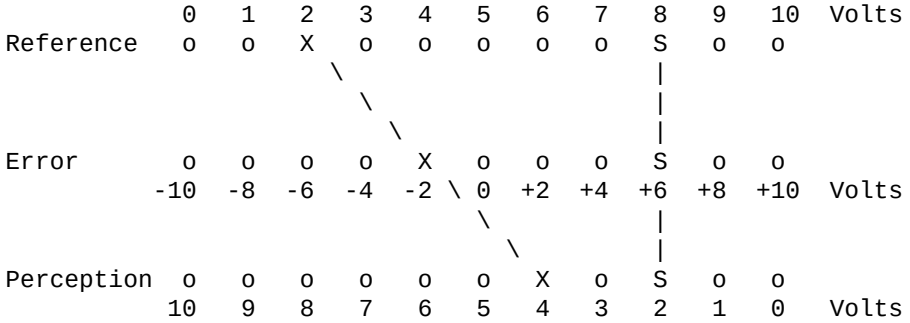
(for servo engineers):

CS demo. Single control system to begin with.

I would like to build a simple control system demo to fit in a briefcase and be powered by regular housecurrent. How about a joystick (reostat) providing a reference signal to represent the aiming of a cannon. I would like the reference signal to be visible, perhaps in the form of a voltmeter. The gun barrel would also be hooked to a rheostat and the signal visible in the same way. Now the error signal created and made visible. An amplifier (with output visible on a watt or amp meter or

perhaps through the glow of a lightbulb or tone (pitch, volume) of a small speaker) will drive a motor left or right with proper drive of the gun barrel.

It occurs to me that if the perceptual, reference and error signals are shown, each on a string of LED's, then the subtraction can be made graphically quite visible.



One of the things I want to come alive is the (rapid) conformance of the perceptual signal in response to a rapidly changing reference signal.

Surely some of the people on this net have thought about this and can suggest designs, components and sources of supply. Please give me some ideas.

Later, we can create visible conflict between two units and also build a hierarchy.

I will soon have Excel for Windows and will start to play with Rick's spreadsheet. This model is visualized as a portable, more intuitive supplement.

(David Goldstein)

PCT attitudes:

What is it to be selfish? Is that bad?

The answer to the question you pose lies, I believe, in your systems concepts and consequent values. These must be discussed.

Part of my systems concept is that I, myself, am an autonomous, self-governing control system.

Based on this conviction, I value the ability to function as an autonomous control system. This value can be expressed as in "Mastering Assertiveness Skills, Power and Positive Influence at Work" by Elaina Zuker (Amacom, 1983):

ASSERTIVE BILL OF RIGHTS

I have the right to be treated with respect.
I have the right to have and express my own feelings and opinions.

I have the right to be listened to and taken seriously.
I have the right to set my own priorities.
I have the right to say no without feeling guilty.
I have the right to ask for what I want.
I have the right to get what I pay for.
I have the right to make mistakes.
I have the right to assert myself even though I may
                                inconvenience others.
I have the right to choose not to assert myself.

Another part of my systems concept is that every other person
is also an autonomous, self-governing control system.

Based on this conviction, I value the ability of my fellow
humans (and at least some animals) to function as autonomous
control systems. This value can be expressed as:

"I grant you the right to ------- (the same list!)

Now if I am to be selfish and live up to my own values, I
will have to be a reasonably nice guy, (can't be a tyrant
because that would create error signals in me, myself) but
don't have to be a doormat either.

I read "Behavior the Control of Perception" three years ago.
Recently, I recognized that my memory of the reading was
inadequate. As I have again reviewed the book, I would like
to draw your attention to the chapter on conflict and
control. In the middle of page 255, Bill discusses a person
who has two goals: to be nice on the one hand and to be
strong and self-sufficient on the other.

I think this relates to your question about selfishness. My
conclusion is that a list like the Assertiveness Bill of
Rights above must be very carefully considered so that there
is no difference, no conflict, between claiming and granting
these rights. If indeed there is no conflict, then we can
all be decisive, selfish and very nice, all at the same time.

Dag Forssell
23903 Via Flamenco
Valencia, Ca 91355-2808
Phone (805) 254-1195   Fax (805) 254-7956
Internet: 0004742580@MCIMAIL.COM
==========
Date: Tue, 10 Sep 1991 09:40:00 MET
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Jan L. Talmon" <TALMON@RLMIS1.BITNET>
Subject: Re: anti-PCT attitudes

[From Jan Talmon (910910)]

David Goldstein (910908) writes:
> Conclusion: Explain things the way you want to. If
> some others don't understand you, oh well!!.....

It depends completely on the reference signal of A. If A has as reference
that B should understand yA's utterance, an error signal will occur in A
when B doesn't understand.

The same holds for the first comment made by David. PCT is not a
rationalization for selfishness. It only say that when the reference
signal at a high level is directed towards selfishness, a person
will be selfish.

==========

Date: Tue, 10 Sep 1991 09:32:52 EDT
Reply-To: Cybernetics and Systems <CYBSYS-L@BINGVMB>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Comments: Resent-From: "CHARLES W. TUCKER" <N050024@UNIVSCVM>
Comments: Originally-From: CYBSYS-L Moderator
<cybsys@bingvaxu.cc.binghamton.edu>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: CSS92 Call for Papers

I THOUGHT THIS MIGHT BE OF INTEREST TO SOME OF US. CHUCK

----------Original message----------
Really-From: CSS Conference92 <USERCS92@UMICHUM.BITNET>
Date: Mon, 9 Sep 91 13:23:56 EDT

CALL FOR PAPERS

1992 Conference on Computing for the Social Sciences

May 4-7, 1992 // University of Michigan // Ann Arbor

Sponsored by the Social Science Computing Association in
cooperation with the Bureau of the Census and the Oak Ridge
National Laboratory

You are invited to submit an abstract of a paper for possible
presentation at the third annual conference on Computing for the
Social Sciences. The conference theme -- Gateways to the Future --
focuses on the revolutionary capabilities for the management and
analysis of social, economic, political, and demographic data
brought about by the technological changes of recent years.

The conference will offer a forum for an expected 300 users,
and potential users, on the computing power, storage of mass
data, electronic networks, graphics systems, and applications
made possible by this new technology.

The program will follow five major tracks, with several sub-themes
featured in each track. Selected papers will be photocopied for
all registrants. Presenters will be have 30 minutes for presenta-
tion and Q&A. Papers will be reviewed for possible publication in
the Social Science Computer Review.

The conference will also include opening and closing general
sessions (with keynote speakers); opening panel discussions
introducing each of the tracks; hands-on tutorials and
special demonstrations for direct experience with tools,
applications, and data; an on-site contest using census and

survey data; and several food/social events.

The conference registration fee will be $200. Registration, accommodation, and travel fees will be the responsibility of each presenter. The deadline for submitting abstracts is December 1, 1991. The deadline for sending full text of selected papers is April 1, 1992.

MAJOR TRACKS:
1. DATA ACQUISITION, MANAGEMENT, AND DISTRIBUTION
Creating, managing, or accessing local and remote data archives; acquiring census/survey data; networking to remote archives -- including CATI/CAPI.

2. RESEARCH STRATEGIES AND ANALYTIC METHODS
Innovative applications of computing and information technology to the management and analysis of social data -- including Artificial Intelligence and simulation.

3. GRAPHICS AND VISUALIZATION
Graphics and visualization as tools for the analysis of data and the presentation of findings -- including graphical techniques for exploratory data analysis and geographic information systems.

4. INFRASTRUCTURE
Facilities, administrative and technical support, and funding required to create and maintain computer environments for social science instruction, research, and planning. This track will also discuss resources for the physically and mentally challenged.

5. NETWORKS
Resources available via local, national, and international networks; access to the networks; and electronic communication -- including file transfers, e-mail, and electronic conferences.

Any of these tracks may include special topics such as teaching methods; international collaboration; ethics and values; PCs/Macs; supercomputing; operating systems, user interfaces; or other topics that you may suggest.

If you have questions or suggestions about the program, contact:

Al Anderson, Program Chairman

University of Michigan

Phone: 313-998-7140

Fax: 313-998-7415

Internet: albert_f._anderson@um.cc.umich.edu

BITNET: UserLD52@umichum

If you would like to submit an abstract, send a fax or e-mail message to the Program Chairman with the following information:

- Your Name
- Job Title
- Organization Name
- Address
- Phone
- Fax
- E-mail Address
- Preferred Track 1 2 3 4 5
- Abstract and outline of your paper in 300-800 words

DEADLINE FOR SUBMITTING ABSTRACTS IS DECEMBER 1, 1991

If you are not submitting an abstract, but would like to receive registration material in January, send your name and address to:
Internet: css92@um.cc.umich.edu
BITNET: UserCS92@umichum

==========

Date: Tue, 10 Sep 1991 07:48:08 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Social systems

[From Bill Powers (910910.0700)]

Kent McClelland (910909) --

Re: Social control:

>Nevertheless, I feel quite confident in predicting that no highway
>patrolman will ever pull you over to give you a big kiss on the cheek.
>Some things, I would argue, are truly out of bounds in given situations.

Actually, in Colorado it's apparently possible to be pulled over and be given some sort of good driving citation. I don't know if you get an actual kiss.

Maybe it would be useful to distinguish between "social control" and "concerted control." When 20 people decide that an ocean-going lifeboat should be launched off the beach, each person adopts the reference signal "boat in water," grabs the boat, and drags it into the water. Of course if one person alone tried that, the boat wouldn't move. If 10 of the people adopted the goal "boat 50 feet further from the water," the boat wouldn't move, either. Concerted control is something like distributed processing. If the goals are aligned and the perceptions commensurable, you get the effect of a single control system with much greater output than any one system alone has (and higher loop gain).

A related kind of control would be "coordinated control." Now all 20 people together are unable to move the boat using a steady pull. However, if one (any one) of these people says "Heave! Heave! Heave!" (and the other 19 understand what this means and agree), surges of total force can be generated that are greater than the maximum possible sustained force, and the boat moves in steps. Each person agrees to synchonize the pulls with the voice signal, thereby giving the signal the status of a command.

To a bystander it might appear that all 20 people have suddenly turned into S-R systems (with one of them mysteriously proving autostimulation).

Then I suppose you could have "managed control." The skinny captain of the livesaving team watches the struggles on the beach for a few minutes, then claps his hands and shouts "Give 'er a yo-heave-ho!" The team, shamefacedly, agrees and starts singing "Yo, heave Ho!" and the boat starts to move as they pull in time with the song.

In all these cases the actual control lies inside individuals, and is conditional on agreement and understanding.

I think that just by remembering that control always lies in the individual, one can come to understand social phenomena without invoking some superordinate being or mystical force, much as McPhail and Tucker avoid such things in their analysis of gatherings. When I say that there is no social system, I'm denying the widespread sense that there is an impersonal system run by some gigantic and implacable (and rather stupid) monolithic entity analogous to a single human being. Of course there is a social system: it is not, however, a unitary control system but the outcome of all the concerned individuals interacting, cooperating, conflicting, joining together in concerted effort, seeking each other, hiding from each other, looking for dependence and independence, enforcing laws and fighting or ignoring them, and so on. Small groupings of people in this system occasionally and for short periods get their goals and perceptions to run sufficiently in parallel to accomplish something together that they could not accomplish alone. The rest of the time these same people interact differently with each other, often against each other. The net result, at any given time, can be any sort of system that is imagineable, including no-feedback and positive feedback systems. The result can imitate a hierarchy, a heterarchy, a random network, or simply randomness. The only thing that determines what kind of system it is, aside from physical constraints, is the perception and goal structure of each individual person that's in effect at that time.

I think that social laws can be deduced, but they will not be fixed universals. They will be contingent: IF a group of people adopts such-and-such a mix of goals and has such-and-such skills, THEN the following phenomena of interaction will emerge. One example of this sort of law is the degrees-of-freedom concept. When there are enough people sharing a given environment that the number of independent goals possible exceeds the available degrees of freedom in the means of achieving those goals, conflict (and all its symptoms such as aggression and violence) will necessarily appear. The growth of social systems can probably be traced to the various feasible means that exist for resolving such conflicts: taking turns, specializing, developing the idea of concerted, coordinated, or managed control, and so on. Each person in a conflicted society has a personal motive for adopting methods that will reduce conflict: the restoration of personal control.

The real question is not whether there is a "social system." It is what kind of system it is at the moment and in a particular locale. I think that the answer varies with place, personnel, and circumstances. There is always a system, even in the inanimate world. The whole universe is a network of interacting variables, which is all you need to have a system.

Best

Bill P.
==========
Date: Tue, 10 Sep 1991 14:59:24 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: various responses

[From: Bruce Nevin (910910 1221)]

Haven't written because busy getting a book out and sprained my wrist over the weekend.

-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-
(Martin Taylor 910907 13:00)

Some lovely examples! I have come across a few more too.

Lieberman describes how preadolescent boys, controlling for differentiation of male and female voice pitch that comes mostly automatically to adults, learn to speak with lip rounding. This lowers the upper formants for all sounds, with the general effect of making their voices sound deeper without actually lowering the fundamental, which can't be done without risk of injury. It is not until after puberty that male larynges tend to be larger and the longer and heavier vocal bands in them generate a lower fundamental frequency. Some male adults do this too, if their voices are higher pitched than others. Try speaking with lips spread in a smile or pursed to hear the difference. Once alerted to this, I notice that my 9-year-old daughter's male schoolmates do indeed speak this way, something that I never remarked before.

In addition to the description of the lip-restraint experiment that I quoted in a prior post, Lieberman also describes other experiments done with bite blocks, that is, a block held between the teeth while speaking.

I think a lot has been done that people don't know quite what to do with.

-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-
Rick Marken, Gary Cziko

Still haven't followed up on conversion capability of LUCID for spreadsheets, but will.

-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-
David Goldstein (Sun, 8 Sep 1991 09:57:25 -0400)

These responses suggest to me values prevalent in our culture being held as reference values at high levels in these people. Recall that in not all cultures is there a conflict between selfishness and altruism. It is only given that presumption that such interpretations are likely to arise.

-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-
What has been developed on parenting from a CT perspective?

-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-
Kent McClelland (Mon, 9 Sep 1991 13:52:39 cdt)

I was struck by Bruner's claim that the "formats" that adults afford to children to help them learn, and the formats of social interaction and language use that they develop from and through them, come to "have exteriority and constraint" and "become objective":

> Formats, save when highly conventionalized, cannot be specified independently of the perceptions of the participants. In this sense, they generally have the property of contexts in being the resultant of a definition by the participants. The definition of formats communally is one of the major ways in which a community or culture controls the interaction of its members. Once a format is conventionalized and "socialized" it comes to be seen as having "exteriority and constraint" in Emile Durckheim's sense and become objective in Karl Popper's. Eventually, formats provide the basis for speech acts and their constraining felicity conditions. We learn how to invoke them by speech.

In the cases of interest to sociology and social psychology, it seems to me, the shared reference values concern controlled perception of roles and relations and moves in a conventionalized, game-like sequence involving them. It's not "look, we can both keep this cursor on track" but rather "it's your turn to track that one now, and I know you know that, and I know you know I know it, etc., by prior agreement." Of course the notion "your turn to track" is "unrealistic," but only in the sense that any model may seem very simple and artificial by comparison with that modelled, and modelling the control of roles and relations required by convention to carry out tracking tasks might not be a bad next step.

I have no difficulty with (and argue for) `the notion that social conventions have a "reality" external to individuals.' I only argue against the supposition that this social reality reflects suprapersonal hierarchical control. Though control theory has enormous scope, it necessarily does not encompass all that is to be said about human and animal behavior. Necessarily? Relations among control systems, precisely because they are not matters of hierarchical control, are by definition not treated in it beyond the observation that our familiar presumptions about interpersonal control and power are wrong, and the beginnings of evidence that some patterns in social behavior are mere byproducts of individual control for values conceived as private rather than social (arcs and rings in the crowd program). Will the latter suffice? Lots of muck shoveling, perhaps, before we get at claims of the social sciences that bear deeper scrutiny.

Chuck Tucker sent me the introduction to _Romanticism and Ideology_ by Morse Peckham, which Peckham says summarizes his previous book _Explanation and Power: the control of human behavior_. While I think I find here a perspective that is congenial to mine in many respects, I have deep misgivings about much that he has to say. I intend to say more about this later when I have some more time to think and to write.

In this connection, though, let me say that Peckham identifies "explanation" with going up in a taxonomy of increasingly inclusive (and abstract) words. He argues, I think with validity, that the hierarchical structure of social institutions mirrors this verbal taxonomy and that social institutions have ideologies as the upper termini of their hierarchies. Now, Peckham seems to be stuck in S-R

thinking (e.g. "performances consist of responses appropriate to the presentation of particular signs," p. 6), and I need to sort all that out, but I think there is a germ of truth here, in that people create and maintain social institutions as reflections of internalized ideological values that they have assumed as their own (or rejected as belonging to "those others") as well as means for promulgating and maintaining those values. The ideologies are fabrications of words and particularly of the capacity in language for creating and using more or less arbitrary taxonomies of classificatory words.

Subsumption (Peckham's term) of two lower-order words A, B by a more abstract classificatory word C is based on an analogy between A and B. For Peckham, the mere use of C to refer to both A and B constitutes "explanation." An explicit analogy is a superior explanation, it seems to me, since it states just how you are using familiar characteristics of A to predict supposedly analogous characteristics of B, and perhaps vice versa. It is subject to test, where an abstract generalization (Peckham's writing is filled with them) is difficult to test until you apply it to cases. Modelling, as Bill and others have pointed out, is a method of explanation superior to both of these. A model is a special sort of analogy in which an A in one domain (the model) is constructed specifically to be analogous in testable ways to a B in the other, and is thought to be explanatory only so far as its analogies have been precisely defined to carry it. There is no C as a term subsuming B and some X in the world of experience to which B belongs unless B and X are also modelled by some B' and X' in the model A, any more than there a C as a term subsuming A and some other X in the world of models (but not in the model A). We may use such terms C, typically by way of drawing analogies between familiar modes of explanation and what the model tells us; for Peckham, use of generalizations C is where all the explanatory action is. Or at least that is my impression, and it is entirely possible that I haven't yet figured out what he is saying.

Back to your point:

>My question is this: Under what conditions can two (or more) independent
>control systems, working in parallel in the same environment, be modeled as a
>single system?

When are two (or more) autonomous control systems controlling for the modelling of themselves as members of a single system, according to mutually known roles and relations?

-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-
Dick Robertson (Mon, 9 Sep 1991 12:51:41 -0500)

I use grep on a UNIX system to search for arbitrary strings in files, including the string "Subject:" to see all the subject lines in message headers in a file. There are freeware and shareware versions of grep and other utilities for PCs. There are other string search programs. I haven't tried on my PC, but probably will have to, as I archive to PC and thence to disk to reduce my disk-hogging on my UNIX mail host.
-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-
Bill Powers (910908.1200)

Your Sunday post arrived today (Tuesday), so I guess our listserver is still a bit behind.

>From what I'm seeing, all the information below about 1 KHz occurs at
>harmonics of the fundamental frequency

All the information in a vowel occurs at harmonics of the fundamental.
Formants are what's left after a great deal of the energy (all of most
harmonics) has been damped out. Or am I misunderstanding you?

>there's a subjective
>impression that vowels that sound the best-formed tend to maximize the
>energy at rather than between harmonics imposed by the mouth/nose
>cavities. I can almost feel the formants locking in at the frequencies
>that resonate the best.

Very unlikely that we control for skull-specific resonances, given the
enormous plasticity of pronunciations as variables on dimensions mentioned.
Harmonics imposed by the supralaryngeal cavities vary depending on
configuration of those cavities by articulators, and it is that which
makes the different vowels. What could this subjective impression be, of
some vowels being more resonant or better formed? A function of hearing
some frequencies better? Finding more energetic formants at certain
values of a continuously changed i-I-e-E-ae-a->-o-U-u (or portion
thereof) on your display would make it less subjective.

I have jury duty tomorrow. Can't tell whether I'll be dismissed or
engaged, and if the latter whether for one day or a long stretch. I
hope have time to study your proposal re extracting F0 (pitch, vibratory
rate of vocal bands, "voice") and using it to separate out other
characteristics, and your very interesting dialogue with Peter Cariani.

-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-

Dag Forssell

I like your demo ideas.
-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-

        Bruce Nevin
        bn@bbn.com
==========
Date: Tue, 10 Sep 1991 14:36:47 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Closed Loop Threads

[from Gary Cziko]

Although Greg Williams is not (yet?) an active CSGnetter, he does receive
diskettes of all posts each month and it is from these that he puts
together Closed Loop, of which three issues have already appeared with a
planned frequency of four per year (sent only to paid-up CSG members).

Up until now, Greg has enjoyed complete autonomy in selecting threads for
Closed Loop. But the thought occurred to me that we on the net could make
suggestions.

Here is mine, Greg. Put together an issue on SOCIAL CONTROL.

You other CSGnetters may have your own choices. So let Greg know by
posting a note to the net.--Gary
==========
Gary A. Cziko   Telephone: (217) 333-4382
University of Illinois   FAX: (217) 244-0538
Educational Psychology   Internet: g-cziko@uiuc.edu (1st choice)
210 Education   Bitnet: cziko@uiucvmd (2nd choice)
1310 South 6th Street
Champaign, Illinois 61820-6990
USA
==========
==========
Date: Tue, 10 Sep 1991 12:45:36 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: John Maag <SECD001@UNLVM.BITNET>
Subject: Re: anti-PCT attitudes
In-Reply-To: Message of Tue, 10 Sep 1991 09:40:00 MET from <TALMON@RLMIS1>

As a fairly new subscriber to this list who has been quietly reading,
with interest, the postings on various topics, I became interested in
the question of how to deal with criticisms of PCT. My background began
out of a traditional operant behavioral paradigm. I have since
incorporated elements of cognitive theory into my research and teaching.
Recently, I have become more interested in paradigm shifts (certainly
not a new area) and the idea of moving away from micro-perspectives to
more macro-perspectives--that is moving away from a functionalist
approach to one that is more subjectively applied to systems and
organizations. Given this rather redundant introduction, the problem
that I have with PCT (or a criticism that I would appreciate receiving
some feedback on) is that it truly explains EVERYTHING in terms of a
micro-perspective. My opinion is that there are multiple realities and
multiple truths in existance. Ironically, I see a lot of similarities
between PCT and operant theories. No before someone reacts to this
statement, let me say that the similarities, I believe, reside in that
both theories are reductionist. I shall stop here and see what type of
response to this "criticism" people on this list may have to enlighten
me to a deeper level of PCT.
John Maag
University of Nebraska-Lincoln
==========
Date: Tue, 10 Sep 1991 14:49:55 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Cziko Paper

[from Gary Cziko 910910.1440]

Just a short note to let CSGnetters know that any comments on the paper I
circulated electronically ("Purposeful Human Behavior and as the Control of
Perception: Implications for Educational Research") will be needed by the
end of this week since I will be soon sending this off to Educational
Researcher, the principal "theoretical" journal of the American Educational
Research Association.

Since this is my first attempt to publish a piece on control theory, I have
yet to experience the frustrating rejections and maddening reviewer

comments that other active PCT authors have had to deal with. While I really would like to see this article published (and therefore am soliciting comments), a nice, unequivocable rejection would make me feel more like "one of the boys."

Either way, I can't lose (or is it rather that I lose both ways?)!--Gary

P.S. Thanks to those of you who have already provided comments and whom I have not yet thanked personally.

P.P.S. I would be happy to send a copy of this paper to any CSGnetters who have recently joined the network. Just send me a personal request.

Gary A. Cziko Telephone: (217) 333-4382
University of Illinois FAX: (217) 244-0538
Educational Psychology Internet: g-cziko@uiuc.edu (1st choice)
210 Education Bitnet: cziko@uiucvmd (2nd choice)
1310 South 6th Street
Champaign, Illinois 61820-6990
USA

Date: Tue, 10 Sep 1991 16:08:24 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Program Files

[from Gary Cziko 910910.1600]

I have had a number of requests for Rick Marken's Excel spreadsheet which I was amazed to find that I could distribute via email as a Binhex encoded file. Rick also sent me his Lotus (IBM) spreadsheet in Binhex form, but I have now found out that Binhex is used only for the Mac and that a comparable archive form for IBM PC files is PKZIP.

I also know that Bill Powers sends out his demo programs using some nifty self-extracting files, but don't know if these could be sent via email.

So what I am looking for is information on the best way to disseminate exectuable (program) files via email. Binhex seems to work fine for the Macintosh, but what about IBM? I think that we could give PCT a real boost by making the excellent computer demos developed by Powers, Marken, McClelland and others available via email.

Eventually, I hope to set up an e-mail accessible file server so that CSGnetters could ask _it_ for papers, bibliographis, programs, etc. instead of asking me. But the local compuer whizzes here don't seem to be able to do this as it should be able to be done via LISTSERV. If anybody out there knows how to do this and/or is willing to set up an email-friendly file server, please let me know.

Reactions and suggestions, please.--Gary

Gary A. Cziko Telephone: (217) 333-4382
University of Illinois FAX: (217) 244-0538
Educational Psychology Internet: g-cziko@uiuc.edu (1st choice)
210 Education Bitnet: cziko@uiucvmd (2nd choice)

1310 South 6th Street
Champaign, Illinois 61820-6990
USA
==========
==========
Date: Wed, 11 Sep 1991 08:08:40 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Linguistics; reductionism

[From Bill Powers (910911.0700)]

Bruce Nevin (910910) --

> In this connection, though, let me say that Peckham identifies
>"explanation" with going up in a taxonomy of increasingly inclusive (and
>abstract) words.

This way of "going up a level" seems to be a widespread concept of hierarchy. Another is J.G. Miller's, in which a format is laid down for systems at one level and is then repeated for systems of larger and larger size: atom, molecule, cell, organelle, organ, organism, social group, society, life ...

In my proposed hierarchy there is no defining principle carried over from level to level of perception, nor is going up a level a matter of viewing the same collection of items from a more abstract point of view, nor is it a matter of repeating the same organization on a larger scale of size. Each new class of controlled variable is orthogonal to the previous class. These levels were "discovered" (as far as one individual can make a discovery valid for everyone); each came as a surprise, like an unexpected change of subject. If you apply the viewpoint of any level to larger and larger collections of elements, you just get larger and larger examples of the same type of perception without going up a level. This is another reason not to extend the control-system model to societies: doing so is simply perceiving another collection of elements from the same level of perception.

>>From what I'm seeing, all the information below about 1 KHz occurs at
>>harmonics of the fundamental frequency

>All the information in a vowel occurs at harmonics of the fundamental.
>Formants are what's left after a great deal of the energy (all of most
>harmonics) has been damped out. Or am I misunderstanding you?

Yes, I was talking about vowels (and the undercurrent of lower-frequency harmonics that accompanies high-frequency variations due to lip rounding and details of tongue shape). I'm glad Mary was out yesterday while I was saying "BEED BID BADE BED BAD BOD BUD BAWD BODE BOOD" as rapidly but distinctly as I could, trying to get the whole series into the 3.5 sec of recording time available. The resulting sonogram is interesting: the stack of lowest harmonics grows from two or three (EE) to five or six (BAD) and then dwindles back to two or three again (OO). After the middle vowels, the high frequencies (about 1KHz up) drop out, more or less. Sure enough, if you say "OO" and then widen the lip opening without moving anything else, you're just about saying "EE".

I have a tracking filter working on one variable frequency. It's much easier to build circuits on a computer than with a soldering iron! Next, the hard part: figuring out how to get a bunch of filters to allocate themselves to whatever harmonics are present, without duplications or getting lost. I don't know if this is possible, but... onward, ever onward.

Thanks for the materials on existing sonograph software. I can see that if anything new is to come of my approach, it won't be something obvious. At least my program is cheap! There is certainly some powerful software out there.

Also, the piece on writing fast C-code was welcome. I was pleased to see my prejudice about the slowness of recursion backed up. I'd forgotten about register variables, and will see if that helps (although the inner loop, which calculates all n filters, is now completely in assembler code -- it does about 40 interations per second of a 256-channel filter).

>Very unlikely that we control for skull-specific resonances, given the
>enormous plasticity of pronunciations as variables on dimensions
>mentioned. Harmonics imposed by the supralaryngeal cavities vary
>depending on configuration of those cavities by articulators, and it is
>that which makes the different vowels.

What I meant was that (unconsciously) I seem to adjust the articulator positions for vowels so that the resonances fall exactly on the lower harmonics -- when the heard result sounds "best." It's possible to be off in the adjustments, so that there is sound energy between harmonics, or normally-missing harmonics are filled in. Then the vowel sounds sort of muddy or indistinct. Of course this could be one of those facts that ain't so -- this is not exactly a real-time display.

John Maag (910910) --

>... the problem that I have with PCT (or a criticism that I would
>appreciate receiving some feedback on) is that it truly explains
>EVERYTHING in terms of a micro-perspective. My opinion is that there
>are multiple realities and multiple truths in existance.

What is "micro?" Are you talking about reductionism? It seems to me that control theory can get pretty "macro" if you apply the principles at appropriately high levels. The concept that organisms control what they perceive to be happening to them seems macro enough to me.

As to multiple realities and multiple truths, I can appreciate the democratic motives behind such ideas, but in most cases where I have heard such a view expressed, it's a cop-out -- a way of saying that none of the existing theories works worth a damn. Some psychologists have taken a "microtheory" stance -- I don't know if this is still going on. The idea behind microtheories, as I understand it, is that theories are only good for the exact experimental conditions under which the data were taken; you thus need a different theory for every new circumstance. This tells us a lot about the status of theory in psychology.

I think there's a "Boss Reality" (to quote the author of *Castaneda's

Journey,* who is somewhere in my garage in a box). Our theories may not capture it, but when we test theories against observation, we're constrained by it. If control theory violated those constraints, we'd find that predictions from the theory wouldn't fit behavior. Control theory isn't just a "perspective." It commits us to expecting certain relationships to be observed, to the extent where a single counterexample would call for revising the theory. The "truths" of control theory aren't statistical. You may be judging control theory in terms more appropriate to other approaches in which being right 51% of the time is thought to be good enough if p < 0.05.

>Ironically, I see a lot of similarities between PCT and operant
>theories. No before someone reacts to this statement, let me say that
>the similarities, I believe, reside in that both theories are
>reductionist.

There, you said it. I deny that control theory is reductionist. It is *inclusive* in that it can handle phenomena at the level of biochemistry and muscle twitches as well as it can handle phenomena of will, desire, intention, interpretation, and so on. But it certainly does not say that cognitive and private phenomena are "nothing but" physics and chemistry. It is a model of ORGANIZATION.

The main DIFFERENCE between PCT and operant theories is that with PCT we can demonstrate that it is the organism, not the environment, that controls. Another difference in PCT is that we can explain, with this organizational theory, how an operant can possibly exist: how an organism can "emit outcomes" without doing so by regular means. That is the main question that Skinner begged, and begging it was the main reason for which he made the mistake of attributing the causes of behavior to environmental events. If he had ever asked how operants could exist, he would have discovered control theory. He was doing the right experiments. But his conviction about external control prevented him from reaching the right (consistent with Boss Reality) conclusion.

Best to all,

Bill P.

==========

Date: Wed, 11 Sep 1991 13:00:32 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: keeping track of posts?

[from Gary Cziko 910911]

Dick Robertson (910909)

>Does anyone have a good method of indexing past posts? Several times recently
>I have wanted to find something I saw last spring. But it requires going back
>through all of the files I have from that time.

Greg Williams has developed something that may be of use.

>Gary, is there a way of getting an index of the titles that went on a the net
>in a given month? Thanks to whomever has a good solution for this problem.

I don't think that there is any easy way for me to do this, but I will check it out.--Gary

Gary A. Cziko
University of Illinois
Educational Psychology
210 Education
1310 South 6th Street
Champaign, Illinois 61820-6990
USA

Telephone: (217) 333-4382
FAX: (217) 244-0538
Internet: g-cziko@uiuc.edu (1st choice)
Bitnet: cziko@uiucvmd (2nd choice)

Date: Thu, 12 Sep 1991 09:45:00 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Hugh Petrie <PROHUGH@UBVMS.BITNET>
Subject: anti-pct attitudes

[from Hugh Petrie 910912.0930]

Bill Powers (910910) already responded to John Maag's request for comments on the reductionism and micro, macro level of PCT (Maag, 910910). I wanted to add just one more reaction. At least as I use

the words, PCT is the antithesis of reductionism, or, for that matter, of a micro level. Reductionism for me would be some attempt to translate talk of perceptions, actions, intentions, etc. into talk of the firing of neuron B31 at such and such rates (obviously I know nothing of neurophysiology).

On the other end of that spectrum, as has been argued eloquently several times on this net in the past, most extant psychological theories, like behaviorism are not explanatory at all, they simply redescribe the phenomena. The excitement of PCT is that it really does appear to provide the "right" level of description to be explanatory. It can predict, phenomena can be modeled using it, and the "boxes" in the diagrams can be instantiated in physical systems, including living organisms. In short, it's logical level and characteristics are just right to give it a chance of actually being an explanatory psychological theory. I also believe that the evidence strongly supports it as well.

Hugh G. Petrie, Dean
Graduate School of Education
367 Baldy Hall
State University of New York at Buffalo
Buffalo, NY 14260

716-636-2491 (Office)
716-636-2479 (FAX)
PROHUGH@UBVMS.BITNET

Date: Thu, 12 Sep 1991 07:54:38 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Language; Social systems; language

[From Bill Powers (910912.0700)]

Picking at threads --

Bruce Nevin (910910) --

>Lieberman describes how preadolescent boys, controlling for
>differentiation of male and female voice pitch that comes mostly
>automatically to adults, learn to speak with lip rounding.

Little girls do this, too. When they play roles, whichever girl is being Daddy or Cop speaks that way -- you brought back memories! The pitch is lowered as far as it will go, and then lip-rounding brings the higher formants down, too. I used to think of this as trying to "sound important." I'll bet that if you asked your wife to imitate a pompous judge, she would do the same thing. So what is it about formants that is perceived as mattering for recognition? Ratios? Differences? Differences of log frequencies, of course are objectively ratios.

>I was struck by Bruner's claim that the "formats" that adults afford to
>children to help them learn, and the formats of social interaction and
>language use that they develop from and through them, come to "have
>exteriority and constraint" and "become objective":

Another way to view "formats" is to see the adults as setting up rules such that they will give the child the response the child appears to want only when the child adopts a particular mode of speech. Skinner would have called them contingencies of reinforcement. From the child's point of view, the adult behavior can be controlled by finding ways of speaking that lead the adult to speak or act as desired. This is akin to your idea for teaching second languages -- introducing clever disturbances that can be corrected only by making the right sounds. In this case, the nature of the external feedback connection (the format adopted by the adult) is a parametric disturbance, so that the child must output a particular way of speaking in order to get back what is wanted after that output passes through the adult.

This has applications to other kinds of social interactions, too.

>In the cases of interest to sociology and social psychology, it seems to
>me, the shared reference values concern controlled perception of roles
>and relations and moves in a conventionalized, game-like sequence
>involving them.

The "game-like sequence," once adopted by one person, consists of rules like "If he does or says A, I do or say B." As the other person, you can learn to perceive this rule experimentally. Of course before that can happen, you have to perceive a principle: "Hey, that was a funny thing for this person to say (or do) in relation to what I said (or did) -- ah, he's playing some sort of game. Let's see if I can figure out the rule."

>I have no difficulty with (and argue for) `the notion that social
>conventions have a "reality" external to individuals.' I only argue
>against the supposition that this social reality reflects suprapersonal
>hierarchical control.

The relevant "reality external to individuals" is, of course, other people. Other people do things for their own reasons. They seem to march to inner drummers, and often a lot of them seem to be marching to the same inner drummer (as near as you can figure). They also build things and leave them around: chairs, houses, roads, television sets, dinners.

Those things are just physical objects until you realize that someone had a purpose in building them, and figure out what that purpose might be, and try it out for yourself. Then you know what it feels like to march in cadence.

When you see enough people apparently reacting to you in accord with a rule of some game, and when you have deduced the rule well enough to predict how they will respond to your moves (or disturb you if you don't move), you may well come to think "OK, I guess that's the rule" and adopt it for yourself. This can leave you with the impression that this rule exists somewhere out there in space. It seems to affect everyone, so it must be imposed from somewhere else. It isn't just that your mother likes to put the fork on the left with the napkin, then the plate, then the knife and spoon on the right. That's they way they are SUPPOSED to be placed. It's a rule of etiquette, and etiquette isn't something people decide to do: they do it because it's right. Now the rule has become reified; it no longer seems that you or anyone else has a choice.

Our language is full of words that have the specific function of making social rules seem to be something other than a personal choice adopted after considerable effort. You have duties, responsibilities. You must do what is right. People have something called "authority" and it must be "respected." Children must learn to "cooperate" (i.e., do what they are told). This is a government of laws, not persons. People can be upright or transgressors. People have "rights."

When you start thinking about all the facets of society (as it is or as it should be) to which you wholeheartedly subscribe, you come face-to-face with the real price of understanding control theory. The sense of being carried along and protected by some benign regulating system external to yourself disappears: you are faced with taking responsibility for fundamental aspects of your life that, long ago, you turned over to someone else. You see other people not as being in the grip of the system, but as the authors of their own choices and their own actions.

In truth this basic freedom was there all along, but in getting involved with figuring out all the games that are going on, and in learning how to adopt the rules yourself and use them for your own ends, you, the adult, have forgotten what the point was. It's both liberating and frightening to realize just how much of your life is in your own hands.

A true model of behavior doesn't just describe the way people are. The way they are results from just one possible adjustment of the model, one possible set of parameters. A true model shows you other ways they might be, given changes in the parameters and in the alterable aspects of organization. One reason for which control theory has taken so long to be recognized and adopted is that the older theory wasn't even recognized as a theory -- it was simply the way things are. Something happens, and a person responds to it. That's just a fact. But when you realize that reference signals are adjustable, that stimuli are really disturbances of controlled variables, an apparent response to a stimulus suddenly becomes just one of the possible outcomes. If the reference signal changed or the perceptual function were reorganized, the same stimulus would be followed by a different response, or the opposite response, or no response at all.

Societies as they are now represent the outcome of one way human organisms can conceive of each other and interact with each other. Control theory shows that there are other ways. The job of control theory

is not just to describe social phenomena as they are, but to reveal those phenomena as a consequence of adopting just one mode of organization out of many that are possible. The same goes for language: language as we know it is just one way in which people can use conventions, rules, principles, to manage their interactions with each other. To understand language we have to see how the system might be different from the way it is -- merely fitting a CT interpretation to the situation as it exists is only a small first step.

==========

Date: Thu, 12 Sep 1991 07:59:33 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Language; Social systems; language

[From Bill Powers (910912.0700)]

[Previous send didn't seem to work -- sending again]

Picking at threads --

Bruce Nevin (910910) --

>Lieberman describes how preadolescent boys, controlling for
>differentiation of male and female voice pitch that comes mostly
>automatically to adults, learn to speak with lip rounding.

Little girls do this, too. When they play roles, whichever girl is being Daddy or Cop speaks that way -- you brought back memories! The pitch is lowered as far as it will go, and then lip-rounding brings the higher formants down, too. I used to think of this as trying to "sound important." I'll bet that if you asked your wife to imitate a pompous judge, she would do the same thing. So what is it about formants that is perceived as mattering for recognition? Ratios? Differences? Differences of log frequencies, of course are objectively ratios.

>I was struck by Bruner's claim that the "formats" that adults afford to
>children to help them learn, and the formats of social interaction and
>language use that they develop from and through them, come to "have
>exteriority and constraint" and "become objective":

Another way to view "formats" is to see the adults as setting up rules such that they will give the child the response the child appears to want only when the child adopts a particular mode of speech. Skinner would have called them contingencies of reinforcement. From the child's point of view, the adult behavior can be controlled by finding ways of speaking that lead the adult to speak or act as desired. This is akin to your idea for teaching second languages -- introducing clever disturbances that can be corrected only by making the right sounds. In this case, the nature of the external feedback connection (the format adopted by the adult) is a parametric disturbance, so that the child must output a particular way of speaking in order to get back what is wanted after that output passes through the adult.

This has applications to other kinds of social interactions, too.

>In the cases of interest to sociology and social psychology, it seems to
>me, the shared reference values concern controlled perception of roles
>and relations and moves in a conventionalized, game-like sequence
>involving them.

The "game-like sequence," once adopted by one person, consists of rules like "If he does or says A, I do or say B." As the other person, you can learn to perceive this rule experimentally. Of course before that can happen, you have to perceive a principle: "Hey, that was a funny thing for this person to say (or do) in relation to what I said (or did) -- ah, he's playing some sort of game. Let's see if I can figure out the rule."

>I have no difficulty with (and argue for) `the notion that social
>conventions have a "reality" external to individuals.' I only argue
>against the supposition that this social reality reflects suprapersonal
>hierarchical control.

The relevant "reality external to individuals" is, of course, other people. Other people do things for their own reasons. They seem to march to inner drummers, and often a lot of them seem to be marching to the same inner drummer (as near as you can figure). They also build things and leave them around: chairs, houses, roads, television sets, dinners. Those things are just physical objects until you realize that someone had a purpose in building them, and figure out what that purpose might be, and try it out for yourself. Then you know what it feels like to march in cadence.

When you see enough people apparently reacting to you in accord with a rule of some game, and when you have deduced the rule well enough to predict how they will respond to your moves (or disturb you if you don't move), you may well come to think "OK, I guess that's the rule" and adopt it for yourself. This can leave you with the impression that this rule exists somewhere out there in space. It seems to affect everyone, so it must be imposed from somewhere else. It isn't just that your mother likes to put the fork on the left with the napkin, then the plate, then the knife and spoon on the right. That's they way they are SUPPOSED to be placed. It's a rule of etiquette, and etiquette isn't something people decide to do: they do it because it's right. Now the rule has become reified; it no longer seems that you or anyone else has a choice.

Our language is full of words that have the specific function of making social rules seem to be something other than a personal choice adopted after considerable effort. You have duties, responsibilities. You must do what is right. People have something called "authority" and it must be "respected." Children must learn to "cooperate" (i.e., do what they are told). This is a government of laws, not persons. People can be upright or transgressors. People have "rights."

When you start thinking about all the facets of society (as it is or as it should be) to which you wholeheartedly subscribe, you come face-to-face with the real price of understanding control theory. The sense of being carried along and protected by some benign regulating system external to yourself disappears: you are faced with taking responsibility for fundamental aspects of your life that, long ago, you turned over to someone else. You see other people not as being in the grip of the system, but as the authors of their own choices and their own actions.

In truth this basic freedom was there all along, but in getting involved

with figuring out all the games that are going on, and in learning how to adopt the rules yourself and use them for your own ends, you, the adult, have forgotten what the point was. It's both liberating and frightening to realize just how much of your life is in your own hands.

A true model of behavior doesn't just describe the way people are. The way they are results from just one possible adjustment of the model, one possible set of parameters. A true model shows you other ways they might be, given changes in the parameters and in the alterable aspects of organization. One reason for which control theory has taken so long to be recognized and adopted is that the older theory wasn't even recognized as a theory -- it was simply the way things are. Something happens, and a person responds to it. That's just a fact. But when you realize that reference signals are adjustable, that stimuli are really disturbances of controlled variables, an apparent response to a stimulus suddenly becomes just one of the possible outcomes. If the reference signal changed or the perceptual function were reorganized, the same stimulus would be followed by a different response, or the opposite response, or no response at all.

Societies as they are now represent the outcome of one way human organisms can conceive of each other and interact with each other. Control theory shows that there are other ways. The job of control theory is not just to describe social phenomena as they are, but to reveal those phenomena as a consequence of adopting just one mode of organization out of many that are possible. The same goes for language: language as we know it is just one way in which people can use conventions, rules, principles, to manage their interactions with each other. To understand language we have to see how the system might be different from the way it is -- merely fitting a CT interpretation to the situation as it exists is only a small first step.

==========

Date: Thu, 12 Sep 1991 09:58:39 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: robotics conference

Of potential interest to some CSG folk:

Date: Tue, 10 Sep 91 13:12:27 PDT
From: Christof Koch <koch%citiago.bitnet@BBN.COM>
Subject: Conference announcement
To: connectionists@cs.cmu.edu

CALL FOR PAPERS

****
Intelligent Vehicles `92
July I and 2, 1992,
Radisson on the Lake Hotel near Detroit, USA
****

Organized by:
IEEE/IES Intelligent Vehicle Subcommittee

Cooperation with:
American Society of Mechanical Engineers
IEEE Vehicular Technology Society

IEEE Neural Nets Council
Japan Society for Fuzzy Theory and Systems
Robotics Society of Japan
Society of Automotive Engineers, Intemational
Society of Automotive Engineers, Japan
Society of Instrument and Control Engineers
(Some of them listed above are in the application process and
cooperations are not approved yet.)

The IEEE/IES Intelligent Vehicle Subcommittee is organizing international meetings once every year. In 1991, for example, an international meeting will be held on "Fuzzy and Neural Systems, and Vehicle Applications" on November 8 and 9, 1991 in Tokyo. The meeting in 1990 was on "Vision-Based Vehicle Guidance". For 1992, we are planning to have multiple sessions. We will consider publishing a book, in addition to the proceedings, by selecting good papers presented in the special session as is the tradition of this workshop. This workshop will be held in conjunction with IROS '92 (International Conference on Intelligent Robots and Systems) which will be held in North Carolina from July 7, 1992.

Topics:
Real-Time Traffic Control (Special Session)
Fuzzy Logic & Neural Nets for Vehicles
Vision-Based Vehicle Guidance
Other Related Issues including:
Navigation
Microwave Radar & Laser Radar
Communication Architectures
Advanced Electronics for Vehicles

Deadlines:
December 1, 1991, for one-page abstracts
February 1, 1992, for acceptance notices
April 1, 1992, for camera-ready papers

If you would like to have your name on our mailing list, please write "Intelligent Vehicle" and/or "IROS" on the back of your business card (or a card with your address, phone, fax, and e-mail), and mail it to:

Ichiro Masaki, Computer Science Department
General Motors Research Laboratories
30500 Mound Road, Warren, Michigan, 48090-9055, USA
Phone: (USA) 313-986-1466, FAX: (USA) 313-986-9356
CSNET:MASAKI@GMR.COM

==========

Date: Thu, 12 Sep 1991 09:51:51 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: jbjg7967@UXA.CSO.UIUC.EDU
Subject: pretend language

[from Joel Judd]

Bill (910911) {imagine surfer} that bodacious dude, among other like EXCELLENT words, said:

>Little girls do this, too. When they play roles, whichever girl is being
>Daddy or Cop speaks that way -- you brought back memories! The pitch is

>lowered as far as it will go, and then lip-rounding brings the higher
>formants down, too. I used to think of this as trying to "sound
>important." I'll bet that if you asked your wife to imitate a pompous
>judge, she would do the same thing.

This reminds me of an SLA example from a couple of years back that really caught me off guard. I had an ESL class and one day we got talking about how speakers of different languages seem to share general characteristics or mannerisms. I had been struggling, as I always do, with getting them to 'feel' what it's like to speak English, not just with their ears, or mouth but with their whole body. All of a sudden, this one guy (a Korean speaker I think) who usually had a marked accent in English, slouched down in his chair, visibly relaxed his face, and said in excellent English, "Yeah, they kind of talk like this..." and some other things I have written down at home. The other students laughed, and I said something like "that's it" or somesuch. It was really amazing the transformation that came over him. He never spoke like that again.

==========

Date: Thu, 12 Sep 1991 09:02:57 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: anti-PCT attitude & closed loop

from Ed Ford (910912.0900

David Goldstein (910908)

Regarding anti-PCT attitudes: I have never found any anti-PCT attitudes wherever I have taught the ideas, whether in my counseling office, at the colleges and universities at which I teach, at lectures, and at workshops, such as for school districts. I use the ideas to give people a basis for dealing with themselves or others and as a tie-in for the techniques I'm teaching. I would imagine if what you were saying or teaching were a threat to the person listening to you, then the person is going to deal with your words and ideas in the way their organized, not the way your organized.

Regarding what one says about which another may have created bad feelings: When I speak or write, in order to get my message across or to maintain enjoyable social relationships, I have to keep in mind the many reference signals that people generally establish. No where is this more evident than in the school of social work where I teach part time. There seems to be every cause represented in my classroom.

Regarding how one explains things: I think people have many reference signals going at any one time when I'm explaining things, whether in writing or verbally. I think if if people want their ideas understood, they should be explained as clearly as possible and in such a way as to appeal to the largest segment of people.

But, when it comes to anti-PCT attitudes, I just haven't come across any.

Gary, concerning your post:

>Up until now, Greg (Williams) has enjoyed complete autonomy in
>selecting threads for Closed Loop. But the thought occurred to me

>that we on the net could make suggestions.

At present, I believe that Greg is doing a super-human job editing Closed Loop down to manageable size. To ask him to take suggestions from CSGnetters, we may suffer the loss of our editor. His compensation comes to about twenty-five cents an hour (my guess). Perhaps others might want to create their own supplements in specific areas of interest.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, Arizona 85253 Ph.602 991-4860

==========

Date: Thu, 12 Sep 1991 13:59:02 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: AX53000 <AX53@MUSICA.MCGILL.CA>
Subject: myself, reductionsim

(From Francisco Arocha)

I've been following some of the discussion on the net for more than a month now, but this is my first attempt to participate. So I'll present myself first. I am an educational psychologist who works in the area of diagnostic reasoning but who, as many other psychologists, is dissatisfied with much of current theorizing in the field. My interest in PCT is part of my search for a more adequate theoretical framework for psychology. My main dissatisfaction with (cognitive) psychology is that most theories are metaphorical. They postulate cognitive processes, which even though may capture some of our intuitions about behaviour do not deal with the "real thing". I guess that what I'm looking for is a theory that is congruent with some basic philosophical assumptions of factual science (materialialism, realism, reductionsim/emergentism, systemism) and whose explanations although acknowledging and theorizing about mental processes, do not explain the mental by the mental. That is, explanations which are to some extent reductionst. Which leads me to the question, Why are social/behavioural scientists so much against reduction? Reductionsim has played an important role in the mature sciences, and in general underlies a healthy attitude to scientific research. For what have been able to read about PCT (not much, really), it seems to me that indeed it is reductionst, which is a good thing as long as it is recognized that higher level structures and processes have emergent properties that lower levels do not have.

==========

Date: Thu, 12 Sep 1991 14:06:51 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: replies to Bill

[From: Bruce Nevin (910912 1243)]

Bill Powers (910911.0700)

Well, the DA didn't like my looks and issued a peremptory challenge, so no jury duty. I'll be liable again in three years. I was kind of

looking forward to it.

>enough, if you say "OO" and then widen the lip opening without moving
>anything else, you're just about saying "EE".

It's very hard to do this without also moving the tongue, unawares. Try saying words with high or high-mid rounded vowels, like "loot, loon, look, good," with the lips spread. That gives you a lexical target and an articulatory target. Prolong the vowel. A back unrounded vowel (usually written small-cap I with a dash overstruck across it) results that is different from the [i] of "EE." Vowels like this are characteristic of e.g. Turkish, if you have any Turkish speakers around. "Valley Girl" speakers produce relatively unrounded vowels in words like "foot" and "good" (but not I think the higher vowel of "loot" or "food"). Also some Southern American English ("good" in Georgia, I think, Chuck?), where the unrounded vowels compensatorily shift to opening diphthongs ("it's heeyim" for "it's him").

In going the other way--producing a rounded vowel and unrounding the lips--it is very difficult to avoid shifting the tongue forward in process of unrounding. The unrounding is unnatural for us, and takes an effort that seems to implicate muscles farther back in the throat which inadvertently shift the tongue position to a more natural-feeling position. There is no familiar acoustic reference signal to guide us.

>What I meant was that (unconsciously) I seem to adjust the articulator
>positions for vowels so that the resonances fall exactly on the lower
>harmonics -- when the heard result sounds "best." It's possible to be off
>in the adjustments, so that there is sound energy between harmonics, or
>normally-missing harmonics are filled in. Then the vowel sounds sort of
>muddy or indistinct.

Now I get it. Yes, the larynx unimpeded generates a fundamental and harmonics of progressively smaller amplitude. (If the fundamental is 100 Hz, then you see harmonics at 200, 300, 400, 500, etc.) The supralaryngeal vocal tract absorbs energy at some frequencies and passes energy at other frequencies through unimpeded. For a vowel, the bands passed through are the formants. For the right combination of pitch and vowel quality, harmonics (which are narrow) coincide with the peaks of one or more formants (which are broad). Shift either the vowel quality *or* *the* *pitch* and the energy at a particular formant may be reduced because there is no harmonic at its peak, and its bandpass curve clips off the harmonics that do fall within it. If this is so, then, the vowel quality that sounds clear at one pitch should sound "muddy" to you at another, and vice versa. It doesn't seem to me that people change their vowel quality with pitch, but I think that ordinary speech accepts muddy vowels just fine so that isn't indicative.

>I have a tracking filter working on one variable frequency. It's much

Bill, if you find a way to track the pitch using CT principles, it will be a great showpiece for CT. People have been trying for years to write a pitch extractor, in various languages on various platforms, with only partial success. Go for it!

>judge, she would do the same thing. So what is it about formants that is
>perceived as mattering for recognition? Ratios? Differences? Differences
>of log frequencies, of course are objectively ratios.

I wonder if the shape of antiformants--the silences where harmonics are damped out--might be controllable, as delimited by the formants? Nasalization of vowels involves antiformants in the more specific sense of cancelling out parts of formants in the vowels, I believe.

Bill Powers (910912.0700)

>>I have no difficulty with (and argue for) `the notion that social
>>conventions have a "reality" external to individuals.' I only argue
>>against the supposition that this social reality reflects suprapersonal
>>hierarchical control.

>The relevant "reality external to individuals" is, of course, other people.

Two angles on this, the cellular consciousness angle and the furniture angle.

Cellular consciousness first. This is a point of view problem. The relevant reality (in the same sense) external to the cells in my body is the other cells. They are governed by and in part constitute a hierarchical control system, per theory and experiment so far. This is a thing of a radically different order from the cells and other structures in the body, and the cells so far as we know lack means of detecting or controlling for this higher-order reality. There is no convincing evidence that people together constitute social hierarchical control systems in an analogous way, and fundamental reasons (no way to implant reference signals, conflict instead of compulsory compliance) why so far as we now can tell they cannot.

We nonetheless seem to want to push this analogy and through reconstructable human history always have. The king is likened to the head in medieval society, the priest to the heart, the serf to the hands, and so on. Metaphors abound for finding one's place in social space. Are we just inventing to fill a need for top-level reference values?

Conversely, our ineptitude at this business of explaining a social level of organization to ourselves does not indicate that there is none. Assume, arguendo, that there is some higher level of organization of some sort perhaps inconceivable to us, as the organization of my body would hypothetically be inconceivable to my pancreas (were it capable of having conceptions of things). Just accept that there is, for the sake of the argument. For the next two lines of text. Statements like

>The relevant "reality external to individuals" is, of course, other people.

are in that case clearly reductionist. (OK, you can stop assuming a higher level now. The pain will go away if you rub it.)

This is a point of view problem because it is not clear that anything can have a point of view in any usefully relevant sense if it is not a hierarchical control system. But what do we know.

Now the furniture angle. A person walks into a room and makes to sit down in a handy chair. "Don't sit there!" Shocked expressions. Only a person in a certain role can sit there. That person may arrive any moment. The status of the chair, the role, the person holding the role,

the visitor, etc.--these exist only by virtue of the human participants maintaining certain reference values internally. If they ceased to live, or ceased to maintain those reference values for whatever reason, those social realities would be no more. If the cop ceased to hold certain reference values he would be cop no more and might indeed kiss you on the cheek. But that does not demonstrate that things like roles, statuses, etc. are unreal. The existence of many things that are undeniably real is contingent upon hierarchical control, notably our own existence as living, conscious beings. Do you deny your existence as a person because that existence is contingent upon reference values held by elementary control systems at various levels of your control hierarchy?

The furniture of our lives is all social constructs. The fact that we do the constructing out of our perceptions of culture-free objects and events is no more relevant than the fact that the objects around us are "really" mostly empty space, or the facts of quantum mechanics.

And indeed the objects and events exist for us only as HCS-constructs. It is not only social reality that is contingent. (Is that an orange flower? More energy in the UV range. And get a load of that gamma burst across the parking lot!)

The main concern on your part seems to be autonomy rather than ontology. There certainly are a lot of rules, but is that all there is in the social realm? All if-then program steps and nothing else? No, clearly that somewhat ill-defined range of levels between programs and configurations/transitions has culture-specific elements in it, all the business of words and symbols and signs. These are not rules, nor are they likely to be constituted as they are only by virtue of rules stipulating how one is to interact with them.

>Societies as they are now represent the outcome of one way human
>organisms can conceive of each other and interact with each other.
>Control theory shows that there are other ways.

On the one hand, anthropology shows that there are many ways. On the other hand people do need to coordinate their goals without expending all their efforts on arranging to do so, and if control theory suggests a better way than learned social conventions we should hear about it. Please elaborate.

>When you start thinking about all the facets of society (as it is or as
>it should be) to which you wholeheartedly subscribe, you come face-to-
>face with the real price of understanding control theory. The sense of
>being carried along and protected by some benign regulating system
>external to yourself disappears: you are faced with taking responsibility
>for fundamental aspects of your life that, long ago, you turned over to
>someone else. You see other people not as being in the grip of the
>system, but as the authors of their own choices and their own actions.

Worth repeating, so I did. The same experience arises when one becomes multicultural, multilingual, multidialectal, able to shift adaptively to the prevailing norms. To a slight degree we all do this. But the sense of those norms only appears when there is conflict with it, otherwise it is invisible, so to say it disappears with the epiphany of Control Theory seems to miss the mark. Rather, we offer ourselves a different sort of choices when conflicts about coordinated control do arise. A

different way of saying the same, I think. Different means, different experiences, can lead to the same shift in how one experiences. There's an account in the last issue of _Whole Earth Review_ of a meeting with a Tibetan Lama, in which he seems to have enormous plasticity of persona, exemplifying his references to Chinese jailors, teachers, others.

One of Harris's interests, as I mentioned, was to see how one might extend language in some fundamental way. Amelioration is my concern too.

Bruce Nevin
bn@bbn.com

==========

Date: Thu, 12 Sep 1991 13:50:47 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: Linguistics; reductionism

John Maag (910910) said:--

>>Ironically, I see a lot of similarities between PCT and operant
>>theories. No before someone reacts to this statement, let me say that
>>the similarities, I believe, reside in that both theories are
>>reductionist.

Bill Powers (910911) retooted:

>There, you said it. I deny that control theory is reductionist. It is
>*inclusive* in that it can handle phenomena at the level of biochemistry
>and muscle twitches as well as it can handle phenomena of will, desire,
>intention, interpretation, and so on. But it certainly does not say that
>cognitive and private phenomena are "nothing but" physics and chemistry.
>It is a model of ORGANIZATION.

Bill, perhaps you would consider posting your article from the February 1990 CSG Newsletter, "Dewey, the Libertarians, and Control Theory." If you no longer have easy access to the file, I could easily have it re-typed and posted.

This short, 2 1/2-page article is one of my favorite and, I feel, is relevant to the concerns about reductionism. I think a lot of CSGnetters who don't have this article will find it of great interest.--Gary

==========

Gary A. Cziko
University of Illinois
Educational Psychology
210 Education
1310 South 6th Street
Champaign, Illinois 61820-6990
USA
Telephone: (217) 333-4382
FAX: (217) 244-0538
Internet: g-cziko@uiuc.edu (1st choice)
Bitnet: cziko@uiucvmd (2nd choice)

==========

==========

Date: Thu, 12 Sep 1991 15:44:43 PDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AERO.ORG
Subject: Hello

[From Rick Marken (910912)]

The mainframe computer that handles email at my company has just been fixed. It was down since Sunday. It made me realize how dependent I have become on my csgnet fixes. Fortunately, they fixed the machine before I had to recover from the addiction. I have glanced over most of the posts since Sunday. Quite a bit of stuff. But I seem to be missing a post from David Goldstein which had to do with bad responses to PCT. David, could you send a copy of that post to me, if you know what I am talking about. It seems that there were a couple of replies to it. For example, I think Ed Ford said that he had had no problem with hostility to PCT. I think that it is true that people involved in clinical work tend to resonate to PCT more than do people involved in research work.
What do you think?

Gary -- regarding your paper. I don't seem to have a copy nearby but I remember reading two papers of yours and thinking they were great. No egregious transgressions of PCT dogma in either one (for the sake of those in the outside world, the dogma comment was a joke, the great papers comment was not). I look forward to hear of your experiences with reviewers.
I wonder if PCT irritates those in the educational establishment as much as it irritates those in the psychological establishment?

Best regards to all.

Rick M.

****

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

==========

Date: Fri, 13 Sep 1991 07:51:42 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: tracking frequencies

[From: Bruce Nevin (910913 0723)]

Progressing through Lieberman & Blumstein this morning on the train, I read his account of perception of fundamental and formant frequencies (pp. 39f).

Factors for fundamental include: variation in rate of change of fundamental frequency that normally occurs from one period to the next (Lieberman, 1961); how different supralaryngeal vocal tract transfer functions affect the speech waveform; possible variations in the glottal spectrum that can occur at the onset and end of phonation as the larynx responds to transient conditions in air flow, air pressure, and laryngeal muscle tension.

For formant frequencies: far beyond analyzing speech signal for local

energy maxima. Peaks of vocal tract transfer function define formants.
If no harmonics of fundamental happen to coincide with those peaks, the
energy maxima occur displaced from the formant frequencies that would
get through were there energy (harmonics) there. He suggests that,
since the same vowel schwa is heard whether the fundamental is 100 Hz
(man's voice, transfer function peaks define formants at 500, 1500, and
2500 Hz, harmonics coincide) or 200 Hz (harmonics do not coincide,
formant peaks shift downward). For vowel [i] he gives an example at 500
Hz (child's or soprano voice) with F1 shifted up and F2, F3 shifted
downward, with greater amplitude for F2, F3 than for F1.

Locations of peaks for formants of a given vowel varies from one
individual to another. This aspect of the transfer function defining
formants depends mainly on length of vocal tract.

We thus apparently must calibrate both for the other's fundamental
(pitch) and relative spacing of formants for a given vowel.

Lieberman refers to:

Flanagan, J.L. 1972. _Speech analysis, synthesis, and perception_.
        2nd ed. New York: Springer.

Gold, B. 1962.Computer program for pitch extraction. Journal of the
        Acoustical Society of America (JASA) 34:916-21.

Lieberman, P. 1961. Perturbations in vocal pitch. JASA 33.597-603

"Despite the persistent application of the most advanced engineering
techniques, a satisfactory "pitch extractor" is still not available,
although there have been important commercial applications since 1937
(Flanagan, 1972)."

        Bruce Nevin
        bn@bbn.com
==========
Date: Fri, 13 Sep 1991 08:34:23 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: two queries

[From: Bruce Nevin (910913 0758)]

What do you think of the notion that in a state of reorganization one is
suggestible?

Ed Ford:

Just reread an old post of yours (last April) to Joel Judd. Would you
recommend Chapters 9 and 10 in _Freedom from stress_ for suggestions
about parenting?

        Bruce Nevin
==========
Date: Fri, 13 Sep 1991 11:58:16 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>

From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: How and Why

[from Gary Cziko 910913.1130]

In response to the recent discussion on reductionism (Maag, Powers, Petrie, Arocha), I thought I would share some thoughts about a way that helps me understand what PCT is about and how it can be used to explain and understand behvior.

We are watching Jimmy Connors play tennis. He runs around the court, swings his racket, usually hits the ball back to the other side of the net. The first question we can ask about his behavior is HOW. How is he able to get the ball back into the opponents court without letting it bounce more than once in his court? To answer this how question, we will need to know something about how muscles work, how the eyes work, how perception and action are coordinated, etc. To pick just one aspect, to understand how muscles work, we will have to know something about muscles fibers and cells, chemistry, physics, etc. We can continue this HOW question all the way down to the individual molecules and atoms if we wish. This appears to be reductionism. We are explaining a phenomenon at one level by understanding the more micro-level phenomena on which it depends. This certainly seems to be a productive way of pursuing this question.

But if we ask WHY, the answers lie in the other direction. Why is Jimmy Connors hitting the ball back? So that he can win the game. Why? So that he can win the set. Why? So that he can win the match. Why? So that he can win the U.S. Open championship. Why? Because he wants to win the money or perhaps simply to show that he can still play tennis. Why? Because this is consistent with his principles and system concept. To answer the why question, then, we must move UP the perceptual/control hierarchy, not down. This appears to be just the opposite of reductionism. Donald Campbell calls this downward causation. With living control systems, the why question simply cannot be answered using how answers. I think this is what mainstream psychology (from behaviorism to cognitivism) tries to do.

So it appears to me that PCT is reductionist where it should be and wholistic where it should be. In this way it handles both the how and why questions and integrates them together in a way that I have not seen elsewhere in the behavioral sciences.

It is of some interest that while the WHY question leads upward and the HOW question leads downward in living control systems, this is very different for nonliving phenomena. If we ask WHY the moon revolves around the earth, we come up with a reductionist explanation based on Newton's laws of physics. And if we ask HOW the moon move around the earth, we seem to be asking just for a description (like How does your garden grow?).

Conclusion: There is nothing wrong with reductionism, it just doesn't give the whole answer concerning living control systems. PCT integrates both reductionist (downward) and wholistic (upward) explanations in a coherent theoretical framework which provides the first satisfactory account for the functioning and behavior of living systems.

So what do you think of that?--Gary

P.S. Why did I just respond the way I did? How did I do it? There's just

no escape from PCT!
==========
Gary A. Cziko   Telephone: (217) 333-4382
University of Illinois   FAX: (217) 244-0538
Educational Psychology   Internet: g-cziko@uiuc.edu (1st choice)
210 Education   Bitnet: cziko@uiucvmd (2nd choice)
1310 South 6th Street
Champaign, Illinois 61820-6990
USA
==========
==========
Date: Fri, 13 Sep 1991 13:24:19 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: Re: how and why

In addition, in the case of living things you can't even ask how
comprehensively without concurrently answering why.

You have a gift for clarity, Gary. Ever think of going into some
field connected with education ?-)

        Bruce Nevin
        bn@bbn.com
==========
Date: Fri, 13 Sep 1991 13:17:07 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: jbjg7967@UXA.CSO.UIUC.EDU
Subject: name dropping

[from Joel Judd]

I'm giving a paper at a SLA conference in Michigan in a couple of weeks.
After I talk I'll be handing out information about PCT publications, how to
subscribe to the net, and dropping a few names (if anyone asks) as part of
my appeal to credibility. The audience should be mostly ESL teachers and
researchers, but may include psychologists and/or educational types.

If anyone has articles or publications that they feel exemplify a PCT
approach to language learning, I would like to consider including them in
the bibliography I'm going to hand out. If there are any hidden linguists,
language teachers, or others on the net who would like to be recognized as
associated with the CSG, please let me know by the end of the month.
==========
Date: Fri, 13 Sep 1991 16:09:41 TZONE
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "William D. Zitrin" <WDZITRIN@RCAMNL.WR.USGS.GOV>
Subject: Lost Msg - Don't know

Forward-Path:<fheyligh@VNET3.VUB.AC.BE>
Return-Path:<CSG-L@VMD.CSO.UIUC.EDU>
Received: from VMD.CSO.UIUC.EDU [128.174.5.98] by RCAMNL.wr.usgs.gov ; 23 Aug
91 06:41:46 V
Received: from VMD.CSO.UIUC.EDU by VMD.CSO.UIUC.EDU (IBM VM SMTP V2R1)
   with BSMTP id 7954; Fri, 23 Aug 91 08:34:29 CDT

Received: by UIUCVMD (Mailer R2.07) id 7210; Fri, 23 Aug 91 08:29:25 CDT
Date: Fri, 23 Aug 1991 07:24:18 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@VMD.CSO.UIUC.EDU>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@VMD.CSO.UIUC.EDU>
From: POWERS DENISON C <powersd%TRAMP.COLORADO.EDU@VMD.CSO.UIUC.EDU>
Subject: Misc comments
To: "HEYLIGHEN Francis; Belgium" <fheyligh@VNET3.VUB.AC.BE>

[From Bill Powers (910823)] --

I got up early and have time for a brief comment on this morning's mail in addition to my tome written yesterday. You are, of course, glad to hear this. We all have too much space on our hard disks.

The discussions today delighted me.

I want to pick on Martin Taylor briefly. Martin, there is a glitch in your version of the model (or in mine). With my understanding, I would not be able to say this:

>At every level of the hierarchy, each elemental control system is
>subject to influences from many other sources that contribute to its
>reference signal, and it is irrelevant to the elemental control system
>where those sources might be, except that they are outside itself, and
>even that does not matter, since to the elemental control system the
>reference might just as well be an expression of its freely chosen
>desire to make its perception "just so."

I get the impression that in your model, reference signals (a) come into a control system from the outside world, and (b?) are then "freely chosen" by that control system. This violates several principles in my model. The first is that ONLY PERCEPTIONS arrive from the outside world. They may then be recorded to serve as potential reference signals (although that detail is not required for a simple model). But they are NEVER chosen by the same system that receives them in its comparator. It is ALWAYS a higher-level system (or equivalent process) that sets or selects the reference signal that enters a lower-level system. From the standpoint of a lower-level system, the reference signal is known only indirectly: some inputs create error and others are OK. If you were a thermostat, you would know that 63 degrees is too cold and 70 degrees is too hot because both would create error signals in you. But you wouldn't be able to understand why 68 degrees feels just right. That's given to you: it's a value received from above. You have absolutely no choice about what the "right" temperature is.

When your awareness is identified with a given level of control, some perceptions seem wrong and others seem right. You aware of why this is the case unless you move up a level. Maybe this is what you meant, but I didn't get the emessage clearly.

A suggestion concerning the auditory experiments. Why not start with something relatively simple? Using single-sideband equipment, you can alter the pitch of a voice in real time without altering the pace of speech. Little alterations in pitch heard by a speaker ought to result in

the actual pitch of speaking changing in the other direction. This doesn't get to the formant level, but it's a start. It's also cheaper than playing around with gas mixtures.

Note that if the heard pitch is restored to its former level, the heard formants would be altered, because (as Bruce pointed out) the pitch is controlled by the vocal cords, whereas the formants come from the mouth cavity filtering harmonics. Single sideband speech, when off frequency, seems to distort the relations among formants (but what do I know about that?).

Tom Hancock (910822) --

Sorry about the missed connection at the conference. I sometimes feel like that knot between the rubber bands.

Your basic idea is good, and I apologize for implying that you shouldn't use less than perfect correlations when you have to do something and they are all you can get. We have to start somewhere. I'm just defining a reference signal. Dick Robertson got some results that I didn't think were possible in his grade-control experiment. If he had listened to my prim carping he never would have found them. I would be ashamed if any of my criticisms had the effect of undermining your confidence in your own judgement. Don't give me that much power -- I'd just misuse it.

Why not just go ahead with your idea about measuring the integration factor using the Rikert scale? If you find that you get consistent numbers out of it you can go on from there. It's worth a try.

I would also suggest that there is another way to measure comprehension: that is to see whether the person can actually control for the meaning, directly. Give the person a means of varying the perceived situation, apply disturbances, and look for stabilization of the relevant variables near the correct state -- i.e., the state meant by the description or the instructions. In this way you bypass optimism and pessimism in self-judgements of understanding. If the instructions say "place the widget in the aforesaid relationship to the camfret," it tells you a lot more when the subject actually can do so despite disturbances than when the subject just says "I get it" -- 5 on the Rikert scale. It tells the subject something, too.

Bruce Nevin (910822) --

Trying something to see what happens is, I think, a direct experience of reorganization. Blind variation and selective retention. Of course the same phenomenon can be imitated by a higher-order system that has learned a successful algorithm (thus turning off reorganization). I would think that the FIRST time you wiggle the steering wheel to see which way the car turns, you are reorganizing. But after that, you get systematic about it when trying out vehicles of various types (boats with tillers), which implies learned control at a higher level. You're talking about control of relationships, I think.

Last remark. Martin Taylor (and David McCord, if you're still listening),

I think we need some systematic experiments with the effects of attention on control. What happens to the model parameters when you have to pay conscious attention to many things at once (controlling them or not)? If you need some help with the modeling aspects just holler -- many voices will respond. You undoubtedly have the technical resources to do the experiments. They're very simple to do. But nobody has done it yet, and we need some facts here.

Best

Bill P.

==========

Date: Fri, 13 Sep 1991 16:21:24 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: jbjg7967@UXA.CSO.UIUC.EDU
Subject: color my world

[from Joel Judd]

Before going home, I'd like to post a question. It's one of those questions which, because it's almost 5:00pm on a Friday, I'm not sure hasn't been dealt with before, perhaps in a different form or I'm missing something incredibly obvious.

Reading through some of the "input" literature in SLA today, this question popped into my mind as definitely needing some attention. Because of Krashen's Monitor Model, linguistic input became a focal point of much research (from the point of view of course that different input produced different output). It quickly became clear that this was much too simplistic and so it was decided that not all input became "intake" or linguistic input relevant/useful/needed (supply your own adjective) to a given learner. Since then the search has become one of determining what intake most helps a certain kind of learner in a certain kind of situation. Research bearing on questions of intake continue even as I post.

One of the reasons for the differentiation between input and intake was the changing of the naive assumption that learners were passive receivers of input; instead they are "...active participants in choosing the target language models they prefer and thus acquiring 'the right stuff' according to their values. In other words [non-native speakers have] 'input preferences' (or 'model preferences') in the sense that they consciously or unconsciously choose to attend to to some target language models rather than others" (Beebe 1985).

My question is: Can reference levels be seen as "coloring" perceptual input? If so, how? Or is it simply matter-of-fact input relevance/irrelevance at a given level in the hierarchy--what's relevant either produces error or it doesn't?

Actually I guess that's three questions. Have a pleasant weekend.

==========

Date: Fri, 13 Sep 1991 21:42:37 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>

Subject: Controlling intonation

[from Gary Cziko 910913.2130]

Bill Powers, Bruce Nevin, Martin Taylor, Joel Judd, other speech scientists:

I've started to work my way through the Lieberman and Blumstein book on acoustic phonetics and can now say I finally understand what formants are all about. It's quite amazing how the human perception system is able to fill in the frequencies that would match a given formant but are not actually there in the speech signal! Another nice example of perceptual constancy and very difficult to get a machine to do.

This got me to wondering if it would be possible to just disturb the fundamental frequency of a speech signal and feed it back to the speaker to see if compensating adjustments were made. For example, some questions in American English are marked by a continually rising intonation as in "Are you coming home?" (although British English seems to me to put a drop on the last syllable). Could we do some type of analog processing in real time so that it would come back to the speaker with a steadily falling fundamental frequency (sort of like putting reversing prisms in the subject's ears)? Of course, the disturbance wouldn't have to be this severe, but could vary all the way from no disturbance through a disturbance that would "monotone" the sentence to the reverse intonation.

Somehow I'm afraid that this would be difficult to do since Lieberman and Blumstein say that they still haven't come up with good pitch extractors for language. But maybe somebody out there knows otherwise.--Gary

---

Gary A. Cziko
Educational Psychology
University of Illinois
1310 S. Sixth Street
210 Education Building
Champaign, Illinois 61820-6990
USA

Telephone: (217) 333-4382
Internet: g-cziko@uiuc.edu
Bitnet: cziko@uiucvmd

==========

Date: Sat, 14 Sep 1991 07:43:18 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Cumulative record

[From Bill Powers (910914.0700)]

Mary found a copy of B. F. Skinner's "Cumulative Record" at the Humane Society's store and bought it (she's organizing their book section). It contains "Freedom and the control of man," in which we find one of my favorite quotes:

"But science insists that action is initiated by forces impinging on the individual, and that caprice is only another name for behavior for which we have not yet found a cause." (p. 8).

I've often said that Skinner could have discovered control theory if he had not been so sure he already knew how behavior works. In "A case

history in scientific method," Skinner, recounting his early days as a graduate student in psychology, tells us that he literally held the key to control theory in his hand.

"If you hold a young rat in one hand and pull it gently by the tail, it will resist you by pulling forward and then, with a sudden sharp spring which usually disengages its tail, it will leap out into space. I decided to study this behavior quantitatively. I built a light platform covered with cloth and mounted it on tightly stretched piano wires.... When the tail of the young rat was gently pulled, the rat clung to the cloth floor and tugged forward. By amplifying the fine movements of the platform, it was possible to get a good kymograph record of the tremor in this motion and then, as the pull against the tail was increased, of the desperate spring into the air." (pp. 81-82).

On page 82 the kymograph record is reproduced. It shows more than a tremor. There is a gradually increasing curve as the applied force is increased, with a sudden "desperate" rise at its end. This curve represents the displacement of the platform in the direction of the pull on the tail, caused by the rearward thrust of the rat's legs. Had Skinner not eschewed theory, he would have recalled (assuming he understood them) physical theories about force and motion. By pushing the platform backward against the torsion of the mounting wires, the rat produced a forward force on its body that prevented a backward acceleration due to the force on the tail. Had Skinner actually done this experiment in a "quantitative" way, measuring the pull and the position of the rat's body and calibrating the kymograph, he might have noticed that the rat's body moved less in the direction of the pull than the platform did. He could have deduced the loop gain of this control system. He might have realized that his theory of simple causation was inadequate, for the pull he felt from the rat's resistance, which he called "gently pulling on the tail," was as much the rat's doing as his. The rat was not resisting the pull; it was helping to create the pull. What it was resisting was being moved, before it failed and attempted to escape.

But all that Skinner saw was that pulling on the tail caused the rat to tug forward. He did not realize that the position of the rat's body, as a consequence, remained nearly undisturbed. He didn't see the role of the rat's intention to stay where it was. He was seeing cause and effect: pull on the tail; the rat tugs forward. That observation agreed with his believe about what science insists upon, so he did not pursue the analysis further.

Having exhausted the possibilities of pulling on the tails of rats, Skinner built a longer platform fitted with a kymograph, and measured the ballistics of movement as he enticed an unrestrained rat down the runway with some wet mash, clicking a clicker now and then to get a record of the rat's sudden stops. He saw tremors whenever the rat started moving or stopped, noting the visible shapes of the oscillations but not knowing what part of them was due to the rat and what part to the undamped mounting of the platform (only a theory of damped oscillations could have suggested that this was not entirely a record of the rat's behavior).

Skinner then added a return path so he wouldn't have to reset the rat manually every time. Then he automated the enticement so the rat's movement tilted the ramp and released some food. Then he got interested in the delay before the rat started down the ramp to get the food. Then he eliminated the ramp and had the rat reach into a tray for the food.

Then he put in a lever. Then he wrapped a string around the food dispenser's axle and made it lower the pen on the kymograph a little with each dispensation, producing a cumulative record on the smoked drum. Then he became famous.

Skinner just wanted to observe the facts. The facts, of course, were whatever struck his eye that seemed to fit his concept of causation. Later in life he continued the same policy. He did not measure his cumulative records of bar-pressing behavior to get quantitative data from them; he judged the records by their visual appearance: their slope, their "scallops." He never tried to quantify the relationship between rate of reinforcement and changes in behavior: he just said that reinforcement increased the probability of a response. The details didn't interest him. He stopped investigating when his point was made. He was not trying to discover how behavior works; he had known that from the start. He was just trying to achieve control of effects by manipulating causes.

On second thought, there was really no chance of Skinner's discovering control theory, after all. He held in his hand the fragile little skeleton balancing on its smoothly lubricated stack of joints, tugging on its own bones through elastic bands. He felt it matching its effort against his effort, pushing the platform out from under itself, and saw it remaining still as a consequence. And then he let it spring off into space, out of his grasp.

-- bill p.

==========

Date: Sat, 14 Sep 1991 07:45:40 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Tracking filter; C-code

[From Bill Powers (910914.0700)]

Bruce Nevin et. al.

Progress report:

Here is the c-code for a tracking filter. It's set up for Borland Turbo-C 2.0. It uses some special modules of mine, but you can substitute your own.

```
static char ch;          /* static because defined elsewhere, too */

main()
{
int dc,x,y,f,t;
int a,freq,out,damp,input;

 setatod();                 /* initialize a/d converter (for user
                               frequency control). I use a mouse here. */
 selectgraph(0,0);          /* initialize graphics
                               (special module, use your own) */
 setgraphmode(graphmode);   /* put display in graphics mode */
 clearviewport();
```

```
 a = 0; out = 0;  dc = 0;  /*Initialize variables used in integrations */
 damp = 160;  /* damping set to 0.160, equal to a Q of about 6 */

 x = 0; y = 10000; /* initialize oscillator */

t = 0;  /* initialize display x-dimension */

/* The following loop keeps going until a key is hit. Each iteration
advances in time by dt, the implicit time-increment. Many iterations are
needed to complete each cycle of an oscillation. */
 do
  {
   f = adread(1);       /* read in value from control handle,+/- 2048 */


    /* This is a user-controlled oscillator. "Muldiv(m1,m2,d)" gets 32-
       bit product of m1 and m2, then divides by d. This could also be
       done by casting to longs: ( (long)( (long)m1*(long)m2))/(long)d).
       Slower. Could also be done with reals, even slower.
       The oscillator runs at a frequency set by the position of the
       user's control handle.
    */

      x = x - muldiv(y,f,1000) + x/1000;
    /* x/1000 is a little NEGATIVE damping to keep oscillations going */

   y = y + muldiv(x,f,1000);
   if (y > 10000) y = 10000;  /* limit the size of oscillations */

   input = y/100;  /* scale down oscillator output */

 /* Below is the tracking filter. The filter part consists of two
    integrators in a closed loop. The first integrator receives the input
    signal, the output signal, and feedback from its own output that
    subtracts from its input -- a damping connection. It integrates this
    sum and inverts its sign. The output of the first integrator, called
    "a", is then integrated again to produce the output signal, "out".
    This is like the analog solution of a second-order differential
    equation with damping and a driving signal. Remember that the calcu-
    lations below just inch us forward one dt at a time, so that many
    passes are needed to generate the oscillating signals.

    The center frequency of this filter is set by "freq." As the scaling
    is set up, when "freq" = 2048, the center frequency is 1/(2*pi*dt),
    where dt is the physical time represented by one iteration of this
    loop. Actually, because of the crude method of integration, the
    filter will not work at the maximum frequency (freq = 4096), and is
    nonlinear above a frequency of about 1/(4*dt).

    When the filter is tuned exactly to the input frequency, the output
    is in phase with the input. If it is tuned too high, the output is
    phase-advanced; if too low, the output is phase-retarded. The
    output of the first integrator is 90 degrees out of phase at
    resonance, with the phase departing from 90 degrees as the filter is
    tuned too high or too low.
```

```
   /* a = integral( (input + output + damping) * frequency/2048) */

   a = a - muldiv((input + out + muldiv(damp,a,1000)),freq,2048);

   /* output = integral(a* frequency/2048) */

   out = out +  muldiv(a,freq,2048);

/*  To detect phase error, we multiply the input signal by the 90-
    degree phase-shifted output of the first integrator. At resonance,
    this yields a signal at twice the center frequency and with zero
    average value. When the relative phase becomes other than 90 degrees,
    the result is a cosine-squared term that has a non-zero average value
    -- positive if the filter is tuned too high, negative if too low.
    This is called "synchronous detection."

    The output of the multiplier is integrated to produce the corrective
    output signal "dc" that sets the tuning frequency.
*/

   dc = dc + muldiv(a,input,100) + 5;
   if (dc < 0) dc = 0;

/* Finally, the frequency is set proportional to the output signal: */

   freq = dc/10;

/* and we plot input and output */

   putpixel(t*10,vcenter-input/5,WHITE);
   putpixel(t*10,vcenter-out/30,WHITE);


   if(x < 0 && y >= 0 && t > 60)
      {
       t = 0;
       clearviewport();
      }
      else ++t;

  }
 while( !kbhit());
 ch = getch();
 closegraph();
 restorecrtmode();
};
```

When this loop starts running, you see two low-frequency sine-waves on the screen or perhaps one sine wave and a straight line. Moving the control handle raises the oscillator frequency (I'm actually using a mouse -- "adread" is part of a general handle-input module that allows using different devices). One sine wave is the input, the other is the output. After some initial hunting around, the second sine-wave locks to the frequency of the user-controlled sine wave, and from then on remains synchronized and in phase with it as the frequency changes over a range of at least 20:1. "dc" will follow fairly rapid changes in input frequency. I haven't made any attempt to optimize the frequency-controlling loop.

The frequency-indicating output of this filter is the signal "dc." This would correspond to what I've described in previous posts as the "envelope" signal. Its magnitude indicates the frequency to which the filter is tuned, and thus indicates the frequency being tracked. For a constant tracked frequency, "dc" is constant (with some ripple). As the frequency changes, "dc" also changes. Clearly, if "dc" were really a neural signal, the frequency of firing of this signal would indicate the frequency of the input signal -- but the output frequency would simply be an analog of the input frequency; it wouldn't have to be related to the input frequency in any harmonic or other manner. Only the output envelope would then matter, and it would indicate what the input frequency is without having that frequency. So this would be the first abstraction from physical frequency to a frequency-indicating FM signal.

Note that "dc" has a constant upward bias of 5 units per iteration. Thus just makes the center frequency sweep upward when lock-on hasn't been achieved yet. When lockon is achieved, the feedback cancels this bias out.

The next step is to try this with a impulse-wave input instead of a sine-wave, to see how the filter can be encouraged to lock onto harmonics of the input instead of the fundamental. When I get around to trying multiple filters, there may be a way of cross-connecting them (taking a hint from neuroanatomy) so that each one will force successive ones to seek higher frequencies to lock onto. If that works I'll just start throwing messy vocal inputs at the filters and see what comes out.

Oh, one unforseen effect of this design is that the lockon speed depends on the amplitude of the input. Is there is a phenomenon such that the frequencies of faint sounds are harder to perceive -- take longer to discriminate -- than those of normally loud ones? Also, the integrator in the frequency control loop will hold its setting if the input signal goes to zero. This would mean that the perception of frequency would persist over gaps in the input signal. I don't know how to make it go away when the input permanently disappears, though, without making the persistent perception of frequency fall to zero like a vacuum cleaner running down.

Best

Bill P.

==========

Date: Sat, 14 Sep 1991 09:48:35 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: the wrong perception

from Ed Ford (910914.0950)

I sent out the letter below to Dr. Corey because I thought that the statement he made needed answering. This text is widely used in university classes throughout the country. I think statements on control theory similar to those made by Dr. Corey must be challenged and corrected. I am not worried about people challenging us. I am concerned that the vast amount of work and dialogue that goes on among CSGers be at least known, recognized, and fairly treated.

September 13, 1991

Dr. Gerald Corey
California State University
800 N. State College Blvd.
CSUF EC 529
Fullerton, CA 92634-9480

Dear Dr. Corey,

I was recently shown a copy of your 4th edition of Theory and Practice of Counseling and Psychotherapy by one of my students at Arizona State University's School of Social Work, where I teach part-time. In Chapter 12 where you discuss William Glasser's Reality Therapy, you state (bottom of page 370) "Although the ideas of control theory are not original with Glasser, most of the recent work on this new theory and how it can be applied to systems is based on his observations, which are summarized in his 1985 book, Control Theory. How this theory can be applied to education has been well summarized in Control Theory in the Classroom (1986b). This most recent book, The Quality School (1990), applies these ideas to school management."

As the newly elected president of the Control Systems Group, I would beg to differ with your statement. First, Dr. Glasser teaches a different kind of control theory than William T. Powers, the person who developed this model in his book, Behavior: The Control Of Perception. Control Theory was around long before Dr. Glasser heard of it, and it continues to be researched and developed by men and women, most of whom are in universities doing research and critical modeling in this area. They include psychologists, sociologists, engineers, economists, law professors, consultants and supervisors in manufacturing plants including computer and aerospace industries, physicists, and practical applications people like myself. Many have published papers and books (including myself) on control theory.

We have established a CSGnet in universities throughout the world on Internet and Bitnet and we have a continuing discussion based on a variety of aspects and applications of control theory (CSG-L@uiucvmd.bitnet is our address). We have people on four continents involved in these discussions.

Dr. Glasser initially went to Powers to learn control theory but later went off on his own. Unfortunately, Dr. Glasser has taken control theory and taught it the way he thinks it is. However, Dr. Glasser not only doesn't teach control theory as he was taught it, to say that "most of the recent work on this new theory and how it can be applied to systems is based on his observations" is false.

Dr. Glasser's standing in the counseling community is such that they accept his writings and his claim. All I ask of you is to do a little research on this, then you make the judgement as to the needed corrections in your next edition. I will be glad to send you a copy of the research done in this area along with a copy of my latest book which reflects control theory as taught by control theorists.

Sincerely,

Edward E. Ford, MSW
President

==========

Date: Sat, 14 Sep 1991 14:08:37 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: How/why; ref perceptions

[From Bill Powers (910914.1000)]

(Gary Cziko 910913.1130) --

What a superb piece on how and why. It was especially brilliant to contrast the how and why approach with living and non-living systems. Bruce Nevin is right about your gift for clarity. Considering what I know of the field, however, I don't think you could ever make it in education. Overqualified.

Joel Judd (910913) --

>it was decided that not all input became "intake" or
>linguistic input relevant/useful/needed (supply your own adjective) to a
>given learner. Since then the search has become one of determining what
>intake most helps a certain kind of learner in a certain kind of
>situation....

>My question is: Can reference levels be seen as "coloring" perceptual
>input? If so, how? Or is it simply matter-of-fact input
>relevance/irrelevance at a given level in the hierarchy--what's relevant
>either produces error or it doesn't?

There are really two questions here: what determines the effective input, and what is the preferred state of it?

All the external world can do is make available patterns of energy at your exterior surfaces. Which of them become perceptions, and what those perceptions are, then depends entirely on the way your perceptual system is organized. It seems to me that behind the problem of "input" versus "intake" there is an epistemological assumption: that the person receives what you present. Without any way to test for what the person is actually perceiving, the teacher can't know what was input to the student, or (at higher levels) what the student constructed on that input after its effects got inside. You need to think like a control theorist to realize

that what YOU see isn't what the STUDENT sees -- not for certain, maybe not even likely.

You're right to distinguish between the perception and the reference level. Even with some reason to think that the student is perceiving in the right dimensions, there's still the question of the state of the perception that the student has selected as the target. Reference levels can indeed color our perceptions -- not by altering the perceptions themselves, but by changing our sense of their value. A perception that is present to a greater degree than the reference-level strikes us as excessive, overdone, too much. If the perception is below its reference level it's insufficient, lacking something. In either case it's the same perception, but we'd rather have more or less of it in some regard. According to 20th Century Western custom, we reify the sense of error and say there's something wrong with the perception.

>... the search has become one of determining what intake most helps a
>certain kind of learner in a certain kind of situation.

This, I think, is closer to the right approach. It recognizes individual differences rather than trying to find THE approach that will work for "the learner." Have you thought of how The Test might be applied to this problem? I'd be surprised if nobody in that field had thought of doing something similar to The Test even without a formal understanding of it.

Best to all

Bill P.

==========

Date: Sat, 14 Sep 1991 15:56:00 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: JHDCI@CANAL.CRC.UNO.EDU
Subject: Is PCT Top-Down or Bottom-Up?

I am still very new to the control-theory group and I think I understand some of the cybernetic features that drive it, but I haven't read enough about it to decide whether it is worthwhile pursuing. The advreligious zeal but I still
d

==========

Date: Sat, 14 Sep 1991 16:37:00 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: JHDCI@CANAL.CRC.UNO.EDU
Subject: Correction to message

My previous message got all mixed up because of incoming mail. I know I shouldn't write these messages on the fly but I get very impatient.

I was stating my hesitation about PCT which probably comes from lack of understanding. The advocates seem to be close to religious zealots in their enthusiasm so I hesitate to put it aside. My knowledge of cybernetics is minimal at best but I do grasp the concept. From what I read on the CSG-L list seems to be that all behavior: system, animal, human, etc. depends on inputs. These inputs are then modified recursively to approach another state. I assume the modifications come from more inputs.

This appears to be a bottom-up design in the constructivist tradition. Am I correct in this belief? The University of New Orleans Library doesn't carry anything on control theory and none of the bookstores in this area seem

to have anything either. Therefore I am not able to learn any more than I see on the list.

My field is curriculum and instruction with a strong bias towards educational technology. I presently teach two courses in computers in education and have studied neural nets and genetic algorithms and plan to incorporate them into my research in learning.

I guess I need to know a little more about control theory but I need more access to literature. I'm not averse to spending some money on the topic but I need to start from an elementary perspective. I am open to suggestions.

Jack DeGolyer[JHDCI@UNO.EDU]

==========

Date: Sat, 14 Sep 1991 16:12:43 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: counseling chapters and quality time

from Ed Ford (910914.1610)

Bruce Nevin (910913.0758)

>Would you recommend Chapters 9 and 10 in Freedom From Stress for
>suggestions about parenting?

The short answer to your questions is yes. The chapters deal with the specifics way you work with children, the techniques of how to dialogue with them, and the idea of setting standards in the home (and elsewhere). The ideas I suggest allow for the continued respect for the other living control system with whom you are dealing while at the same time teaching them to think on their own.

What makes the above possible is that you have already established access to working with your child. The child has to believe 1) that you care about him/her and 2) that you believe in them and you believe they can make it. This is what I hope comes through in the prior chapters, namely, trying to build a basis for dealing with children. Living control systems don't always want to deal with other living control systems. There has to be an ongoing established reference signal in each system that says "I want to deal with that person." That is the first and critical step to get the process rolling and keeping it rolling. In short, how do you gain access to dealing reasonably and rationally with children, or anyone. I think you have to create continually the kind of time with children (or others) in which they are more inclined to set a stronger and stronger reference signal for wanting to be with you. That doesn't mean you'll always succeed, it means they will more likely deal with you.

I had a rather intense and indepth conversation with Bill at the conference on this subject, i.e. my ideas on quality time. Since the very early 70's, I was trying to discover what it was that made relationships work (especially marriages and parent/child relationships), why some succeeded and some didn't. I was doing lots of work in that area and what slowly evolved in my mind was the concept and effectiveness of quality time. I eventually established the criteria for quality time. The first was to do activities that promote awareness of each other and create pleasure through mutual effort, thus

getting away from passive entertainment or just being in each other's presence. I used examples such as playing games, exercising together, working at the same project around the house, making things, dancing (ballroom), taking walks. The no-no's were watching TV, going to the movies, just being together, taking a drive, listening to music, watching others (passive amusement, not an active, creation of enjoyment together).

Second was the time must be spent alone with the person, since three or more tends to dilute the strengthening process. I believe all close relationships are built on a one-on-one basis. Third was that the time should be spent on a regular basis (minimum 30 minutes a day, five to six days a week. Three areas I found enhancing in a good marriage but non-productive and non-strengthening in a weak marriage were just talking together, eating together and having sex. I developed these ideas through working with many couples over many years and these criteria have always worked well in relationships where both were committed (young children are almost always committed). In fact, it works unbelievably with children, the younger the better. I have had phenomenal success with this idea. Whole families have turned around, become happier and more warm and loving. The real validity of these ideas comes when you work with families or couples that are not satisfied with their life together, many on the verge of divorce, and, if the commitment is there, remarkable things do happen.

As I began to learn control theory, I tried to understand this phenomena in terms of control theory. As a control theorist, I believe we treat others to a large extent by how we perceive them. It is this perception that we compare to our reference signal, which contains certain standards for relationships. We then decide if we want to work with them depending on whether we believe the perceptual difference is possible to close. In an unhappy marriage or parent/child relationship there could be a rather large error. The larger the error, the harder it would be to work with the other person. Thus the need for reducing the error through quality time before trying to reasonably and rationally trying to work things out.

Where Powers and I had our discussion was my setting standards or criteria for what seemed to work with phenomenal success for those very unhappy and sometimes violent marriages but in which both were very committed. Now the question was "how did this kind of time together bring about this perceptual change in terms of how the couple or parent/child viewed each other. It seems that the actions I take will effect the entire perceptual system. Thus, if I take a long walk with my wife, Hester, that satisfies our mutual goals at program level. Unless the action conflicts with a higher order, it would also satisfy the measured goal(s) and standard(s) that are set at the principles level, which correlate with my program level goal. It would also satisfy what I have set as a value at the system concepts level. Thus my perception of taking a walk with Hester would not only satisfy me at the program level, but throughout the entire hierarchical system, and especially at the system concepts level. Where I value a close relationship and want to continue to find satisfaction therein, I would experience a sense of satisfaction. What I am saying is that the payoff would come from all levels and since I am doing things that are in harmony with the entire system, thus the entire system should respond showing satisfaction as the perceptual error closes down.

During my discuss with Powers, if I recall correctly, he objected to my setting limits (principles level, i.e. standards, criteria, etc) on what constitutes quality time as it is reflected within my world. He felt every world, being unique, should determine that for itself. Obviously, I would agree. However, if I were to find a large number of people finding satisfaction through various activities which had in common similar characteristics, I would think that this might evidence some commonality among activities that provide satisfaction to relationships. Taking this a step further, he spoke of how he and Mary would go out of an evening, sit in a restaurant and read together. He said that would provide a very pleasant evening for both of them. The problem is that they are already getting along, so this could be just an enhancing experience.

The real test for any activity or establishing any kind of set of criteria for what constitutes a strength building activity is not how one perceives it helping when things are already going well in a relationship, but rather when things are not going well, when there is fighting, arguing, yelling, upset, and misery in a marriage. Those are the kind of circumstances under which any kind of valid test can and should be made as to whether the specific activity can help strengthen the marriage to the point where the couple would evidence a certain comeback, if you will, to the misery and unhappiness in the relationship. If the couple perceived, as a result of the activities, a reduction in stress, that they remain upset for less time when there are differences, and they seem to get over their upsets in a shorter space of time, then this would certainly add validity to the activities and the criteria of the activities.

Over the past 20 years that I've been developing this idea, I recognized certain common elements in the activities that helped couples rebuild what in many cases were very long term, miserable marriages. That's how I came to those criteria in my quality time chart. I tested these criteria out with hundreds and hundreds of families where marriages and parent/children relationships were falling apart, where divorce was being strongly considered, and where there were many unhappy people.

I am not a scientist, but it seems that my attempts to corroborate the above ideas show some kind of validity. I think the real key for testing these ideas is to establish the criteria in marriages that are not going well (no one comes to me feeling great wanting to feel miserable). If this phenomena is valid, then as a control theorist, it should integrate happily with control theory. I've seen this work especially well in children, even with teenagers who are dragged in by their parents, providing I get a commitment from the teenager, and, except in rare cases, I usually do. Quality Time is the only thing I find that really works and works well. I really believe I am on to something, that I have discovered what is so obvious (sound familiar), and it really works.

Bruce, thanks for the question. It led me to expressing the above thoughts. I have been thinking these ideas for a long time, mostly on my own, and it might be helpful to get some reaction to what I've said.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, Arizona 85253 Ph.602 991-4860

==========
Date: Sun, 15 Sep 1991 11:53:14 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: saturn.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: quality time

From: David Goldstein
To: Ed Ford, others
Subject: Quality Time
Date: 09/15/91

I have used your Quality Time approach with couples and with parent/child relationships and recommend it highly to others.

You may have mentioned this in your post but some of the ground rules are: no criticism and no discussion of negative past history.

The critical variable is that the people are willing to spend some time together. The suggestions you make for activities deemphasizes the talking and emphasizes the doing. The people involved are controlling for having a positive experience together and avoiding being/behaving in ways which will result in negative experiences. From doing Quality Time, the two people involved will realize that it is possible to have positive experiences together. Their perceptions of each other do change with this realization in a more postive direction.

If people commit the time together, this provides an opportunity for postive experiences to occur. If they follow the gound rules, then the chances of negative experiences occuring are reduced. Some people are unwilling to commit the time. Some people do not follow the ground rules and the same negative experiences occur. Some people challenge the suggested activities as not resulting in positive experiences for them.

If people who are having difficulties in their relationship are unwilling to work on the Quality Time program, they probably are not willing to work on any kind of program. It is simple. It is easy to follow. It does provide a way in which people can move the relationship in a more positive direction.

Some people have likened it to doing what they used to do when they first started to go out together, when courting. Others have related it to play therapy for adults. There are elements of both in Quality Time.
==========
Date: Sun, 15 Sep 1991 18:09:18 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: LEarning control theory

[From Bill Powers (910915.1600)]

Jack Degolyer (910914) --

>From what I read on the CSG-L list seems to be that all behavior:
>system, animal, human, etc. depends on inputs. These inputs are then
>modified recursively to approach another state.

I think it would be a good idea to read some of the CSG literature before trying to characterize the theory. One entry to the subject is my 1973 book, _Behavior: the control of perception_ (Aldine, 1973), and another, perhaps easier to get, is my _Living Control Systems_, a collection of papers (which should be read back to front). The latter is available from Greg Williams, 460 Black Lick Road, Gravel Switch, KY 40328 ($16.50 pp).

Control systems do not produce outputs that depend on inputs, except in a rather subtle and indirect way. Instead, they VARY their outputs so as to cause their inputs (as internally represented as a perception) to come to a match with an internal reference signal. Thinking of inputs as initiating this process is incorrect. The true initiator is the reference signal, generated inside the system.

The reference signal, derived from memory under control of higher-level systems, is an example of the perception as it is intended to be. The perception as it actually is is compared with this reference, and the difference is what drives behavior. External disturbances of the controlled input are resisted because they tend to create deviations of the perception from the reference value, which leads to action opposing the deviation.

Normally, the difference is maintained very near zero -- most living control systems are very good at controlling their own inputs. When the reference state is maintained constant, the perception is maintained constant, as are the external variables on which the perception depends. This is the arrangement Cannon called homeostasis, in the context of organ systems rather than motor behavior. When higher systems vary the reference signal, the control systems at lower levels make their inputs track the varying reference setting, producing voluntary actions even in the absence of external disturbances. External disturbances continue to be resisted, but now with respect to a variable reference state.

The term "recursively," as you applied it above, is reasonably close to what is meant, if the term is taken loosely and not in the sense of a computer-program recursion. The basic model behind this theory is an analog, not a digital model. All components in this closed-loop model operate simultaneously as in real physical systems, not sequentially as in digital computers having only one CPU. When the system changes from one state to another, all variables inside and outside it (related to the action) change toward new values at the same time, in parallel. The usual images employed in computer modeling of behavior do not apply. Our models are basically analog-computer models adapted to run in digital computers.

All this is the basic model behind "perceptul control theory." It sounds pretty mechanical. But buried in it are new principles of behavior that can be translated into terms applicable to ordinary life. Part of what takes time for people to assimilate this theory is pausing long enough to grasp the underlying relationships before returning to the real world to apply it. There really isn't any shortcut, though.

Welcome to the net. I hope you find all this zealotry comprehensible. Most people do if they stick with it a while.

Best regards.

Bill Powers

==========

Date: Mon, 16 Sep 1991 09:09:48 TZONE
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "James M. Peters" <jp2r+%ANDREW.CMU.EDU@CARNEGIE.BITNET>
Subject: Re: How and Why
In-Reply-To: <6AADBC896420006B@BITNET.CC.CMU.EDU>

I have been a silent observer for a while and find the discussions on the net interesting but have not had the time to do the background reading to feel that I could contribute much to the discussion (one of those items on my every lengthening "to do" list when I get some free time). However, I have done some work in cognitive, computational modeling (a la Herb Simon) and have addressed the role of reductionism in modeling complex, psychological processes and would like to response, briefly, to Gary's remarks.

1. Although clarity of expression is a very valuable trait, clarity often comes by over-simplifying. I think reductionism has a role to play in both How and Why questions. The trick is finding the appropriate level to stop the inherent infinite regression that accompanies both How and Why. For example, if we only address How through reductionism, we will never be able to integrate the pieces back into a wholistic understanding of how the muscles interact with the nerves and the higher order cognitive processes. How Jimmy returns the ball is also a function of where he thinks the opponent will be when the ball is returned which is also a function of what Jimmy thinks the opponents overall strategy is. This analysis begins to move us up the control hierarchy. It also illustrates that How and Why are not independent either. In terms of Gary's analysis of the Why question, the direction of the regress is apparently up the hierarchy but it doesn't always have to be, it could be laterally across the same level of the heirarchy. What we are really doing by asking why is working back through a causal chain. We can do that by moving up the hierarchy, which moves us faster, or by moving back through links at the same level of the hiearchy. For example, Jimmy may be playing in the tournament to make money, achieve additional fame, prove to himself he can still do it. etc. Many of these reasons would be at the same level of the control hierarchy.

2. My intent is not to diminish the validity of Gary's remarks but to point out that they are slightly over-simplified and may obscure some complexities in modeling what Jimmy is doing and why. I run into these problems all the time in building programs that simulate human reasoning and have come to the following conclusions:

a. Behavior is adaptive, which means it is a function of the skills, abilities, and information processing capabilities of the person; the demands the task places on the person; and the goals the person is trying to achieve.

b. Each of these factors are multi-dimensional, which implies that behavior is determined by not only simple main effects but complex interations between large sets of variables. This complex interaction

is implied by the concept of adaptivity. The problem is confounded because the person can both affect the environment and also the representation (s)he uses to understand the problem.

c. Understanding and predicting complex behavior of the sort described in b. requires a joint reductionist/wholistic approach where wholistic models are used to guide reductionist investigations of limited sets of variables and interactions. The problem is that experiments can be used to test reductionists hypotheses but not wholistic ones. Experiments can only be practically run with 3 or 4 independent variables and first order interactions. More variables require too many subjects to be practical and still maintain some semblance of statistical power. Experiments are necessary to provide solid support for hypotheses (I realize you can only reject hypotheses with experiments, but we really don't think of it that way most of the time). The answer is a team approach where wholistic models are developed through observations of behavior and by incorporating as many of the results from existing experiments as possible. These models are then used to guide further experimental (reductionist) investigations by pointing out interesting potential independent variables and interactions. The results of these experiments are then used to enhance and improve the model, which acts as an accumulator of results. In this complementary way, wholism and reductionism work together to help us understand both what and why people do what they do.

Jim Peters

==========

Date: Mon, 16 Sep 1991 12:29:34 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: Re: Tracking filter; C-code
In-Reply-To: Your message of Sat, 14 Sep 1991 07:45:40 -0600

[From: Bruce Nevin (910916 1235)]

Bill Powers (910914.0700)

I will have to study your code (admirably commented!) to understand what you are doing. My C is a bit foggy. But from your description this seems to me to be a fantastic result.

I don't know of experiments re slower perception of pitch with low amplitude, but it does make sense. Likewise attenuation on cessation: my subjective impression is that this fading echo effect is exactly what happens. I wonder if reverb effects in music (cp. "new age" stuff like that of Kitaro) have a "dreamy" quality because they mimic at a level that is consciously monitored something that normally happens only at an intermediary or "internal" level of processing and is normally damped out by other control systems.

Gary Cziko (910913.2130)

So far it looks to me like a presumption, and no more, that the perceptions people control are perceptions of formants. In the search for acoustic correlates to articulatory-phonetic observations, people invented the sound spectrograph. A spectrograph shows concentrations of energy that had been observed by ear and dubbed formants as early as the

19th century. This fit the source-filter theory of speech production. Later, it was realized that there might not be any acoustic energy present at the actual center frequency of a formant, harmonics straddling but not falling on it. The appearance in the spectrograph that there was energy there was an artifact of the bandwidth of the filter summing together energy from adjacent harmonics. The presumption may turn out correct but so far it looks to me like a product of expectation and historical accident.

Bill's frequency-chasing control systems might track each harmonic whose amplitude came above a given threshhold. A higher-order control system might construct formants from these, or antiformants, or something else we haven't imagined. There is no particular reason that it should construct something corresponding to "what the vocal tract would pass through undampened if there were a harmonic there," is there? Or does it make sense to have a map of the transfer function for a "standard" or "norm" pronunciation of each vowel against which to map perceived harmonics? (Actual pronunciations, as Martin pointed out, overlap a lot.)

Bill's suggestion about sideband equipment would do for diddling with pitch, though the disturbance would have to be generated manually. The code he posted Saturday shows promise of introducing disturbances in more sophisticated ways. There could be no background noise, since that would change pitch too. There may be other articulatory/acoustic changes made with e.g. question intonation.

In UK English my impression is that the utterance-final drop in pitch is markedly less for questions than for assertions, and may even hook back up again a bit at the end.

Ed Ford (910914.1610)

Thanks, Ed. I will read over what you have offered, think about it, apply it, and let you know. I have a 17-year-old of whose existence I learned when she was 10, lives with her mother in Washington (state), and two other daughters 9 and 4 in my marriage. Always looking for ways to make the latter relationships better, the former relationship is inherently more problematic and occasion for serious concern.

Time short.

Bruce Nevin
bn@bbn.com

==========

Date: Mon, 16 Sep 1991 10:36:17 PDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AERO.ORG
Subject: PCT

[From Rick Marken (910916)]

David Goldstein mentions some anti-PCT attitudes he has encountered and asks how they could be answered.

Attitude:

> PCT says we must choose self or suffer
>intrinsic error signals. Conclusion: Why fight it, be selfish and
>be happy!

My answer: PCT doesn't say this. It says people control variables relative to their own references for these variables. They could control the perceived happiness of someone else, keeping that variable at a nice high (happy) reference level. Control systems can certainly be selfish (I've met a few that are) but they don't need to be (I've also met several that aren't).

Attitude:

>A related idea: If something that person A does or says has the
>result that person B has bad feelings about, that is the
>responsibility of person B.
>   Conclusion: Say/do what you want
>to other people. If they don't like it, tough! They are causing
>their own bad feelings.

My answer: Well, person A, if you feel that way about it,fine. But if I am person B I will probably change my reference for the amount I want to find myself talking to you.

These attitudes suggest that the people you are running into are interpreting control theory as some kind of license to be selfish. It's not. It's just a model of how people work.

>What sort of anti-PCT attitudes have you encountered? I would be
>interested in hearing them.

The most common "anti" PCT attitudes I have run into are (in no particular order):

1) So what? This comes from people who are looking for rules of behavior that can be used to make their lives and the lives of others better. Unfortunately, there are no such rules (as an understanding of control theory would reveal). The only way to make things better is to learn how people actually work and then work within that context.

2) It's obvious. This comes from people who assume that control theory is about perceptual guidance of behavior or cognition or whatever. These are people who would also say that control theory is old hat and is behind the times. These folks usually just ignore control theory -- they have better things to do.

3) It's wrong. These are the best. There are VERY FEW of these -- though this SHOULD be the attitude of virtually every conventinal psychologist. The most likely member of this group is a behaviorist who actually understands control theory -- at least in a qualitative way. But they figure that reinforcement theory already has it right (which moves them to group 2) or they imagine that it can't really work that way. Control theory needs many more people who think that control theory is WRONG and are actively interested in showing that this is the case. As I said, there are just too few (if any) of these types.

The only time I have encountered anything approaching hostility to control theory is when the listener figures out that control theory is completely inconsistent with the whole experimental/statistical framework on which

psychology is based. Most psychologists really believe in this model. They spend years learning statistics and experimental design. It is the core of the discipline: the basic foundation on which the search for psychological truth has been built. Control theory says -- forget it. When you say that to the people who wrote the texts, taught the courses, labored in the stat classes and paid their dues running hundreds of subjects in complex factorial experiments, you don't get big cheers. Even if you carefully show why conventional statistics/experimental design seems to work but really reveals little if anything about the internal organization of living systems.

So my experience is that control theory has the biggest problems when it comes face to face with faith in the SCIENTIFIC METHOD as articulated in the pages of the exhalted textbooks of statistics and methodology that are the bedrock of ALL (cognitive, behavioral, ecological, etc) psychological science.

Best 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)

==========

Date: Mon, 16 Sep 1991 14:11:30 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: jbjg7967@UXA.CSO.UIUC.EDU
Subject: external/internal

[from Joel Judd]

Further questions on Bill's response to input/intake questions:

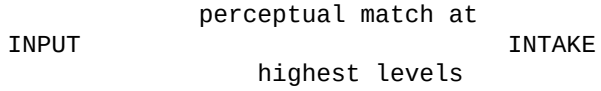
>There are really two questions here: what determines the effective input, and what is the preferred >state of it?

>Which of them become perceptions, and what those perceptions are, depends entirely on the way >your perceptual system is organized.

I guess I was imagining that at the exterior, ALL patterns are the same; the differences appear once the organism starts perceptual transformations. In this sense, INPUT would be the external environmental energy--INTAKE the transformed perceptions. However intake now takes on all kinds of subtle meanings depending upon the level of transformation, thus implying a sort of continuum:

external
environment hierarchical
levels -->
complete < -----||----------> complete,
unambiguous

gobbledegook



Now it's probably not the case that either of the extremes occurs--at least I don't know how to understand what either might mean. But I can imagine what happens in between when, for example, one is plopped down in a foreign country with a language one has not experienced. The patterns of energy making up the L2 impinge on the perceptual system with varying results. Initially, perceptual transformation ends at about the phoneme level, although an occasional syllable may be recognized. Interestingly, sometimes one even thinks one hears a word or a phrase, but it is perceived in a known language. After a little experience, words/phrases begin to be recognized, etc. and onward through the acquisition process. In any case it would seem that in order to talk about intake one must be a little more specific, no?

In a class, you often have the epistemological assumption you mention:
>...that the person receives what you present
and later you say
>Even with some reason to think that the student is perceiving in the right dimensions, there's still >the question of the state of the perception that the student has selected as the target. Reference >levels can indeed color our perceptions...by changing our sense of their value.

This gets DEEP. No pun intended. What does this mean for language learning? I've got to think about it. At first glance it would seem that dealing with GROUPS of learners begins to take on nightmarish proportions.

[...what intake most helps a certain kind of learner in a certain kind of situation]
>This, I think, is closer to the right approach...have you thought of how The Test might be applied...?

The summary statement about the search for optimum intake was meant the way it's meant in the SLA literature. But I think I see where you're coming from. How can a teacher assume that the input he provides is taken by the student as it's meant? By the teacher assuming (a) controlled variable(s) and testing for them. Can the teacher provide input in a way that's likely to maximize the development of intended reference signals? Is that the educational question of interest?

==========

Date: Tue, 17 Sep 1991 10:34:27 +0200
From: Oded Maler <Oded.Maler@IRISA.FR>
Subject: Book problem

Our library tried to contact CSG publishing for obtaining "Living Control Systems" several months ago, and got no response. Could someone do something so that I could base my misunderstanding of PCT on a firmer ground?

Oded Maler
IRISA
Campus de Beaulieu
Rennes 35042
France

==========

Date: Tue, 17 Sep 1991 15:00:32 PDT

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

[From Rick Marken]

Oded Maler says:

>Our library tried to contact CSG publishing for obtaining "Living Control
>Systems" several months ago, and got no response. Could someone do something
>so that I could base my misunderstanding of PCT on a firmer ground?

Try writing to CSG Publishing yourself. Just ask for a copy of "Living
Control Systems" by Powers. The address is

CSG Publishing
Route 1
Box 302
Gravel Switch, KY 40328
USA

I forget what they are charging for the book. I bet they would send you
a copy COD. Maybe someone could post the cost info. Also, you should order
a copy of "Behavior:The control of perception" by Powers from Aldine/DeGruyter
(I don't have their address).

But CSG Publishing, if you send mail to the right place, is very
responsive. Maybe have the library give it another try as well.

Regards

Rick M.

****

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

==========

Date: Wed, 18 Sep 1991 00:55:00 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Dag Forssell <0004742580@MCIMAIL.COM>

Subject: Responsibility

[from Dag Forssell] (for Ed Ford)

Thanks for your call on "Teaching Responsibility". I have been
thinking about it. To me, teaching Responsibility, resolving
conflict within and between individuals, and also performance
appraisals, all follow the same pattern. This more general layout
allows for negotiation, which is not implied in your card. You will
notice that I am heavily influenced by Ford and Soldani.

With reference to your card:

1) EXPLORE THE PERSON'S WORLD: Ask about:

Wants, Actions, Perceptions

(What do you want? What are you doing? Does it work for you?)

You may want to summarize for clarity and rapport.

2) EXPLORE THE OTHER WORLD IN CONFLICT:
(Other goal within person, other person, yourself, family, school, *company*, "society"). (As described by the person!)

Wants, Actions, Perceptions

(What does the company want? What does the company do? What does the General Manager think of this situation?)

If needed, step in and teach about the other world.
You may want to ask the person to summarize.

3) EVALUATION:
Ask person to evaluate the compatibility of 1) & 2).

Is there a conflict?
Are your wants, actions and results aligned with the company?

4) COMMITMENT:

Do You want to work at resolving your problem?

5) PLAN:

What can you change?
What can the company change?
What are your choices?
What can you control?
How can you develop a permanent solution?
How can I help you?
May I show you how to make a plan?

Establish measurable feedback!

Ed, This is an opportunity for me to test my understanding as much as a suggestion to you. Perhaps there are some implications here for the definition of responsibility. (We are not just following rules in this version). I am sure you will edit severely, but hope there may be something here as a stimulus for you to respond to.

Dag Forssell
23903 Via Flamenco
Valencia, Ca 91355-2808
Phone (805) 254-1195 Fax (805) 254-7956
Internet: 0004742580@MCIMAIL.COM

Ed, note phone # for CSG membership list.
==========
Date: Wed, 18 Sep 1991 05:56:43 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Misc replies

[From Bill Powers (910917.1300)]

James Peters (910916) --

Welcome aboard, Jim. Some comments on your comments:

>The trick is finding the appropriate level to stop the inherent infinite
>regression that accompanies both How and Why.

In the "how" direction, the regress can get infinite if you go below the level of functions handling signals and get into neurochemistry, plain chemistry, quantum chemistry, quark chemistry, and so on. But in the upward direction the number of levels is limited by the number that is actually there in the human being. I guess 11.

>How Jimmy returns the ball is also a function of where he thinks the
>opponent will be when the ball is returned which is also a function of
>what Jimmy thinks the opponents overall strategy is. This analysis
>begins to move us up the control hierarchy. It also illustrates that
>How and Why are not independent either.

In the HCT model they're definitely not independent: the higher levels work by controlling perceptions derived from a world made of the lower levels. The only way that temporary independence can be achieved is to persuade the higher levels to hold the reference signals for the lower systems constant for a while.

>In terms of Gary's analysis of the Why question, the direction of the
>regress is apparently up the hierarchy but it doesn't always have to be,
>it could be laterally across the same level of the heirarchy.

Nice thought -- we actually have How, What Else, and Why, where What Else is at the same level. Should have thought of that myself. The hierarchy is already set up so that a given higher-level process employs many lower-order systems of the same (lower) level. If Jimmy wants to win, the "How" question might be answered in terms of tennis strategy, beating the IRS, arguing successfully, or buying low and selling high, depending on the context in which Jimmy is satisfying the general goal of winning. This is a fictional Jimmy, of course -- I don't know if the tennis Jimmy uses other means than tennis to get the sense of winning.

>What we are really doing by asking why is working back through a causal
>chain.

Oops -- not without some careful explanation. The HCT model does NOT employ top-down causation (or bottom-up, either). It's a control-system model. Higher systems don't tell lower ones what to do (although we often talk loosely as if that's the case). They specify what state of a specific perception a lower system is to sense. It's up to the lower system to bring its controlled perception to the specified

reference state. The action or output of each system depends mostly on what disturbances are acting, which in general we take to be unpredictable. If I'm controlling for honesty, I might tell you one day to be more honest and the next to be less honest. Depends on just what degree of honesty I want to perceive, and on your degree of under- or over-compliance as I see it. You can't tell what a control system is doing just by watching its output. Changes in its output tell you mostly about disturbances. You have to trace downstream from the output, in fact all the way back around to the input, to find the stable result of variable output.

>2. My intent is not to diminish the validity of Gary's remarks but to
>point out that they are slightly over-simplified and may obscure some
>complexities in modeling what Jimmy is doing and why. I run into these
>problems all the time in building programs that simulate human reasoning
>...

Some of those complexities may be due to the assumptions and methods that you hint at following this remark. Very few experiments organized around control theory (if any) use statistical methods of the type you mention. See Rick Marken's post of 910916. Experimentation under the control system model is aimed at the characterization of individual behavior. The only reason for using multiple subjects in a single experiment, other than checking for flukes, is to see how variable the individual measures are over a population. We would never average such measures together! What is the average damping coefficient of the human arm control system? Answer: that's not a meaningful question, because the damping coefficient must be appropriate to the build and organization of each control system, if it's stable. Details of organization vary greatly from one person to another.

As you become more familiar with the available CSG literature, you might find yourself in the first two of Rick's categories, but you might be one of those who sees a new level at which to do experiments with human organization. I don't think that statistical studies can hack it in the long run. They have their uses, but once you've seen how control-theoretic experiments go, you'll be spoiled for statistical work. I say that with fingers crossed, because actually nobody is doing systematic research on HCT at the cognitive levels where you work -- this is by way of inviting you to learn the basic principles of HCT and be a pioneer. Doing so will earn you the distrust of your colleagues, difficulties in publishing, and experiments with clear-cut results that you know are right. And friends like us who give you a hard time. You have to weight the costs and benefits yourself.

The question we always ask people who report statistical results is "How many subjects DIDN'T show the effect and how does your hypothesis explain THEIR behavior?" I claim that if you have to use multivariate analysis to show that there was an effect, you haven't got an effect. Real effects stand out like sore thumbs. They aren't the results of causes, but of organization.

My biggest objection to most statistical analyses (I don't know about your analyses) is that almost uniformly they employ a cause-effect model of behavior. We can PROVE that's the wrong model. Organisms produce consistent outcomes by variable means. It's easy to demonstrate this principle in almost any context, at any level. Most experimenters carefully avoid disturbances that might interfere with output, not

realizing that the same outcome would happen anyway. Of course if they did introduce disturbances, and the outcome did repeat, this would completely screw up their experimental paradigms. Maybe that's why they don't do it.

Rick Marken (910916) --

Thanks for the code for the Mind Reader -- I think I'll translate it into C, because my disk is so full that I can't keep Pascal and C on it at the same time, and C is what's there now. It looks doable.

(910918 -- mostly done but for cleaning up bugs)

Bruce Nevin (910916) =--

>Likewise attenuation on cessation: my subjective impression is that this
>fading echo effect is exactly what happens.

In the tracking filter, that output "dc" represents frequency by its magnitude. So if it dies out to zero, it's indicating that the frequency (not the loudness) is falling toward zero, a continuously descending note. That's what I was bothered about. Loudness would come out of another channel that represents total magnitude of sound BEFORE filtering. I'm not going to worry about this now.

Glad to get those remarks of yours to Gary about the history of formants. It's always easier when you know you're starting from scratch (an exaggeration, of course).

And thanks for the comment to Ed Ford. You're among friends.

Rick Marken (910916b) --

A very nice post, full of things I don't have to try to say now. Your characterization of the "opposition," besides being inimitable, is very clear and true-sounding. You've laid out the conditions under which we have to work, I guess.

Joel Judd (910916) --

Yes, you have my point exactly.

>Can the teacher provide input in a way that's likely to maximize the
>development of intended reference signals? Is that the educational
>question of interest?

No to both. Not if you mean one input that will have the desired effect on every individual. The best you can do is to show statistically that it was effective on the average, meaning that for a majority it was overkill and for the rest inadequate. As you say, "it would seem that dealing with GROUPS of learners begins to take on nightmarish proportions."

Don't forget, however, that the individuals will be trying, too. We aren't just pumping inputs into bags. I think that the educational question of interest is how the teacher can go up a level and get the learners to ask for the inputs they need. This means teaching something other than those inputs: it means teaching how to tell when you need information or help and how to guess what kind will do you some good. One

person may need some rote drill, preferring to work out principles independently starting with a set routine that works. Another may want to know the rules and to work out their application independently. Some may prefer to hear an explanation; others to read it; others to have it demonstrated. I think that teaching at this higher level would multiply the teacher's effectiveness, making up somewhat for having to deal with each person as a separate case.

Oded Maler (910917) --

>Our library tried to contact CSG publishing for obtaining "Living
>Control Systems" several months ago, and got no response.

Greg Williams doesn't ignore orders. Try this address:

Gregory Williams
460 Black Lick Road
Gravel Switch, KY 40328
USA
($16.50 US pp).

Others out there trying to guess how control theory can be interpreted to fit what they know: why don't you try this address, too?

Best to all,

Bill P.

==========

Date: Wed, 18 Sep 1991 10:01:55 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: making a pitch

[From: Bruce Nevin (910918 0738)]

Gary Cziko (910913.2130)

>This got me to wondering if it would be possible to just disturb the
>fundamental frequency of a speech signal and feed it back to the speaker to
>see if compensating adjustments were made. For example, some questions in
>American English are marked by a continually rising intonation as in "Are
>you coming home?" (although British English seems to me to put a drop on
>the last syllable). Could we do some type of analog processing in real
>time so that it would come back to the speaker with a steadily falling
>fundamental frequency (sort of like putting reversing prisms in the
>subject's ears)? Of course, the disturbance wouldn't have to be this
>severe, but could vary all the way from no disturbance through a
>disturbance that would "monotone" the sentence to the reverse intonation.

Just changing the fundamental would not do. A change in the fundamental is magnified 5 times in the 5th harmonic (typically within the first formant for some vowels), 10 times in the 10th harmonic, etc. Pitch change would require resynthesizing the signal. Diddling with particular sets of harmonics to shift a formant might actually be easier, given no great pitch change during the affected segment.

One of the things that makes synthesized speech sound "mechanical" is

lack of variations in pitch from period to period that convey affect in natural speech (Lieberman and Blumstein, p. 84). Short-term variations in pitch "from period to period" constitute pretty fine detail! I think only a hierarchical control model can effect this.

The more I think about it, the more I suspect that we should consider the possibility that control concern what harmonics are damped, that the notion of formants is an artifact of history and observational bias, looking for acoustic correlates of articulatory outputs that were presumed to be logically prior. Throw the notion of articulatory targets away, except as backup for situations analogous to navigating around the furniture when somebody turns out the lights, and the question of which harmonics get shut down seems just as obvious a "handle" for perceptual control as the question of which get passed through. You don't do anything special to produce the harmonics within a formant, although what you do does let them pass through. Either perspective is feasible; I'm just suggesting we entertain both.

Bruce Nevin
bn@bbn.com

==========

Date: Wed, 18 Sep 1991 13:29:12 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: tracking code

Bill Powers (910914.0700)

Alas, I have had little time to study your program, and as I said my C is a bit fogbound (even my LISP is creaky). When last I worked with C intensively about 6 years ago, I had only lint and DDT for debugging, and a fairly uninformative compiler, and that was I guess not encouraging. I don't have a C compiler on my PC.

```
>static char ch;          /* static because defined elsewhere, too */
>int dc,x,y,f,t;
>int a,freq,out,damp,input;
```

I remember that it was helpful to write a comment at the beginning of each function that specifies function inputs, outputs, and an "index" of variables. This would specify what each of these variables represents and explicitly state intended mnemonic values of variable names. That way, there's one place to look in case I forget what a or y is at some point, or the relationship between f and freq. Here's what I have culled from comments scattered through your code. I would welcome correction and clarification, as a help to my understanding:

x = Nonmnemonic: value used in oscillator, initialized to 0.

y = Nonmnemonic: oscillator output, initialized to 10000.

f = Frequency: value from control handle, +/- 2048

t = Time: x coordinate of display (time dimension). Advanced incrementally
to max of 60, then reinitialized if x negative and y nonnegative.

a = Nonmnemonic: variable used in integrations

freq = Frequency: sets the center frequency of the filter. Max 4096.
    Set proportional to output signal (freq = dc/10)

out = Output: variable used in integrations. Output of second integrator.

damp = Damping: value used in oscillator.

input = Input: scaled-down output of oscillator (y/100) input to first
    integrator.

dc = D-something correction?: variable used in integrations. The
     corrective output signal that sets the tuning frequency. Follows
     fairly rapid changes in input frequency. The frequency-indicating
     output of the filter. Corresponds to the "envelope" signal. Its
     magnitude indicates the frequency to which the filter is tuned, and
     thus indicates the frequency being tracked. For a constant tracked
     frequency, "dc" is constant (with some ripple). As the frequency
     changes, "dc" also changes. Clearly, if "dc" were really a neural
     signal, the frequency of firing of this signal would indicate the
     frequency of the input signal -- but the output frequency would
     simply be an analog of the input frequency; it wouldn't have to be
     related to the input frequency in any harmonic or other manner.
     Only the output envelope would then matter, and it would indicate
     what the input frequency is without having that frequency. So this
     would be the first abstraction from physical frequency to a
     frequency- indicating FM signal.

ch = character read in from control device. Used by adread to set f?

dt = Device time?: the physical time represented by one iteration of the
    loop. The implicit time-increment, nowhere stated in the code.
    Clock-tick? Implicit in the t++ step, I assume.

        Bruce Nevin
        bn@bbn.com
==========
Date: Wed, 18 Sep 1991 12:55:45 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Varying pitch

[from Gary Cziko 910918.1238]

Bruce Nevin (910918); (also addressed to Bill Powers, Bob Hart)

>Just changing the fundamental would not do. A change in the fundamental
>is magnified 5 times in the 5th harmonic (typically within the first
>formant for some vowels), 10 times in the 10th harmonic, etc. Pitch
>change would require resynthesizing the signal. Diddling with
>particular sets of harmonics to shift a formant might actually be
>easier, given no great pitch change during the affected segment.

I suppose one of the things which suggested that pitch change could be
easily accomplished is that several years ago I saw a not very expensive
audio cassette player which allowed you to vary the speed of playback
without affecting the pitch. I didn't understand how it worked, but

assumed it was pretty simple considering the cost of the unit (someting like $150). So, I figured, if it is easy to change the speed of playback without varying the pitch, why shouldn't it be just as possible to vary the pitch without varying the speed? Perhaps this tape player was more complicated than I thought and perhaps the delay between the tape head reading the the playback was longer than I thought. I need to find out more about this. Perhaps Bob Hart from my campus's language learning lab could give us some clues here (I know you're out there listening, Bob).

>Throw the notion of articulatory
>targets away, except as backup for situations analogous to navigating
>around the furniture when somebody turns out the lights, and the
>question of which harmonics get shut down seems just as obvious a
>"handle" for perceptual control as the question of which get passed
>through.

I wonder what the study of deaf speakers could tell us here. Of course, most deaf people have trouble speaking and perhaps those who have the least trouble have also the least deafness. But I remember hearing an interview a year and a half or so ago on National Public Radio with someone who was supposed to be totally deaf (I don't remember if it was from birth) but who spoke amazingly well. In fact I could hardly hear anything unusual in his speech other than what came over to me as a slight east Indian accent. I believe his last name was Kaiser (spelling?) and he worked (and perhaps still does) as an editor for the Sun-Times in Chicago. I'd been meaning to ask Bill Powers about him for a long time and just never got around to it (Bill also worked at the Sun-Times).

Anyway, wouldn't it be of some interest to disturb the acticulation of deaf people who speak intelligibly (even asking them to do something as simple as keeping their tongue tip touching their bottom teeth) and see if they could adjust for the disturbance. I can't imagine how they could if they couldn't hear what they were saying, so in that sense the experiment seems silly. But wouldn't it be amazing if this fellow Kaiser COULD compensate in spite of being deaf. Big trouble for PCT!

Who can tell us more abou the deaf and hard of hearing and how well they speak? What about people who lose their hearing suddenly vs. those who lose it gradually? I think I'll contact a Elissa Newport who is at Syracuse University who has done lots of work on the deaf and language and see what she can tell us.--Gary

==========
Gary A. Cziko Telephone: (217) 333-4382
University of Illinois FAX: (217) 244-0538
Educational Psychology Internet: g-cziko@uiuc.edu (1st choice)
210 Education Bitnet: cziko@uiucvmd (2nd choice)
1310 South 6th Street
Champaign, Illinois 61820-6990
USA
==========

==========

Date: Wed, 18 Sep 1991 13:47:13 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: sonagraph

I remembered what a sonagraph is. It's the device used by obstetricians

to make a sonagram of the fetus during pregnancy. Ultrasound is another term associated with it.

Bruce Nevin
bn@bbn.com

==========

Date: Wed, 18 Sep 1991 13:40:02 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: jbjg7967@UXA.CSO.UIUC.EDU
Subject: Biblio and teaching

[from Joel Judd]

This may be a good time for Gary or me to send the CSG Bibliography around again. There have been many new subscribers since the last time it went out.

At the beginning of the year a bibliography of publications dealing with CT was prepared and sent out over the network. It was intended to serve as a resource for obtaining classic CT texts and articles, and contains a summary of each work with (theoretically) enough ordering information so that one could get the book/journal from anywhere in the world. Of course, what would the ideal be without a few disturbances?

Also, there is a standing request for the authors represented in the biblio to send in updated information (publishing company changes, price increases) as they become aware of them.

from Bill's response (910917):

>I think the educational question of interest is how the teacher can go up a level and get the learners >to ask for the inputs they need. This means teaching something other than those inputs: it means >teaching how to tell when you need information or help and how to guess what kind will do you >some good.

This sounds just like "learning how to learn," or the old "teach a man how to fish..." saying. There is one published attempt (that I know of) and a scattered bibliography of materials along this train of thought in SLA. The focus of this line of teaching is two-fold: 1) to make students AWARE of their learning preferences and styles--how they learn best; and, 2) get them to feel RESPONSIBLE for their own learning.

I see the first as playing with attention; getting people to see how they learn. One's learning procedures may be so ingrained or unrecognized that this process can be difficult. The second involves setting reference levels for self-evaluation, which in some cases means disturbing reference levels for 'depend on teacher evaluation of performance'-- at the highest level involving the person's CONCEPT of 'learning' itself.

This type of thinking has been partially adopted by many, I think. In the last decade it has become vogue to explain that one's teaching style is "eclectic," meaning that one grasps at any aspect of a particular method which seems to be effective, and integrates them into one's own practices. The part teachers don't get (except perhaps intuitively) is WHY such practice might prove more satisfying than adopting a single method. Understanding a model with controlled perceptions and reference levels can

provide some sense in what might appear to be nonsense in a classroom. The other part not recognized is getting students to "ask for the inputs they need" (again except, I think, among those teachers who seem to do this without recognizing or being able to explain what they are doing).

I think such characterization of education also explains what's been bugging me for a long time. One of the patterns of SLA one sees over time is the rise and fall and then modified acceptibility of methods. The Direct Method, Audio/Lingual, Total Physical Response, Counseling Learning, etc. have gone in and out of fashion and then quietly remain in the background. The fact is, all of them work with SOME people. There are fluent L2 speakers who have some up in an A/L framework, and some who learned with Suggestopedia and so on. Yet because any one of them did not provide the promised panacea, they were discarded in favor of some new method.

Thinking about it now, it might be instructive to gather successful learners from several methods, as well as unsuccessful learners from the same methods, and talk to them about WHY their language ability is as it is. I'll bet that there will appear a list of preferences similar to what Bill posted yesterday (this begins to smack of attempts to match "learner style" with teaching method). The fact is, people CAN find a way to learn. In Peru, I remember coming across a guy who really spoke English well. I asked him where/how he learned. He pulled a rolled up periodical out of his jacket and proudly said, "I read comic books."

==========

Date: Wed, 18 Sep 1991 15:15:24 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: speeding up tape

Gary,

I believe the technology you mention works by eliminating silences.

Bruce
bn@bbn.com

==========

Date: Wed, 18 Sep 1991 15:21:48 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: RYATES@CMSUVMB.BITNET
Subject: Re: Biblio and teaching
In-Reply-To: Message of Wed,
18 Sep 1991 13:40:02 -0500 from <jbjg7967@UXA.CSO.UIUC.EDU>

From Bob Yates

Joel, you are speculating about the rise a fall of certain methods. I am not quite sure of all those reasons, but some of shifting my be due to certain institutional factors. See Alistar Pennycock's article in TESOL Quarterly in 1989 about interested knowledge.

Another term entering L2 teaching with clear control theory assumptions is consciousness raising. See Rutherford, W. and Sharwood Smith, M. (eds) 1988. Grammar and Second Language Teaching. Newbury House for some relevant articles that you should be able to restate in control theory labels.

==========

Date: Wed, 18 Sep 1991 15:29:23 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Maag <SECD001@UNLVM.BITNET>
Subject: Re: sonagraph
In-Reply-To: Message of Wed,
              18 Sep 1991 13:47:13 EDT from <bnevin@CCB.BBN.COM>

Sonogram is used in the early months of pregnancy and is an instrument
that is inserted vaginally. An ultrasound is used latter and is
externally moved over the stomach. Having a baby in three weeks, so I
remember this stuff.
John Maag
==========
Date: Wed, 18 Sep 1991 15:57:49 PDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AERO.ORG
Subject: Experimental/Statistical Psychology

[From Rick Marken (910918)]

Well, no one (except Bill Powers[910917] has responded to my last post trashing
conventional psychology so I'll try again.

Jim Peters writes:

>experiments can be used to test reductionists hypotheses but not
>wholistic ones. Experiments can only be practically run with 3 or 4
>independent variables and first order interactions. More variables
>require too many subjects to be practical and still maintain some
>semblance of statistical power. Experiments are necessary to provide
>solid support for hypotheses (I realize you can only reject hypotheses
>with experiments, but we really don't think of it that way most of the
>time). The answer is a team approach where wholistic models are
>developed through observations of behavior and by incorporating as many
>of the results from existing experiments as possible. These models are
>then used to guide further experimental (reductionist) investigations by
>pointing out interesting potential independent variables and
>interactions. The results of these experiments are then used to enhance
>and improve the model, which acts as an accumulator of results. In this
>complementary way, wholism and reductionism work together to help us
>understand both what and why people do what they do.

Could you give an example of the kind of "wholistic" model you are thinking
of? What kind of experimental investigations test the model? What are the
independent variables and interactions? How do these independent variables
and interactions relate to the model? What is it about "complex behavior"
that you are trying to find out?

I've always wanted to do cognitive type research from a control theory
perspective. Maybe a dialog about what cognitive psychologists want
to know and how they think they can find out will help move me towards
a coherent plan for such research.

Hasta Luego

Rick M.

****

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

==========

Date: Thu, 19 Sep 1991 06:12:14 TZONE
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "James M. Peters" <jp2r+%ANDREW.CMU.EDU@CARNEGIE.BITNET>
Subject: Re: Experimental/Statistical Psychology
In-Reply-To: <01GAQKLHOJVKD7PO2Y@BITNET.CC.CMU.EDU>

Rick Marken (910918) asked me the following:

>Could you give an example of the kind of "holistic" model you are thinking
>of? What kind of experimental investigations test the model? What are the
>independent variables and interactions? How do these independent variables
>and interactions relate to the model? What is it about "complex behavior"
>that you are trying to find out?

>I've always wanted to do cognitive type research from a control theory
>perspective. Maybe a dialogue about what cognitive psychologists want to
>know and how they think they can find out will help move me towards a
>coherent plan for such research.

I don't have the time right now to give Rick the response these insightful questions deserve. Let me do two things, give him a quick and dirty response and refer him to an article of mine which reports a major cognitive modeling project of mine.

The article is "A Cognitive Computational Model of Risk Hypothesis Generation" in The Journal of Accounting Research, 1990 Supplement, pp. 83. The model reviews both financial and other quantitative information (mainly financial statement data) and qualitative information (e.g. the quality of the audited firm's management, whether financial reporting standards or approaches have changed, whether key personnel have turnover, etc.) and identifies accounts that are more risky (i.e. have a greater chance of error). The model is holistic in that it produces evaluation similar to an auditors in a similar way by employing a variety of more basic reasoning processes. For example, it uses four different methods for managing uncertainty and incomplete data: direct assessment (assigns a certainty factor of sorts), data dependency backtracking (does the best it can until something goes wrong and then backs up to the point of conflict and redoes its analysis), decision deferral (sets a subgoal to come back to an issue when it knows it will be getting additional relevant data as an adjunct to it's established problem solving strategy), and default reasoning (takes a shot based on knowledge of business in general or whatever data is has at the time and moves on). Obviously, these strategies are not mutually exclusive. The model has a set of heuristic rules, which I incorporated based on existing empirical research in psychology, theoretical work in AI and extensive research with auditors, to select a given strategy in a given situation.

What a I want to know about human behavior is what information the person accesses with what operators while attempting to achieve what goals. These desires are based on the simple observation that human behavior is adaptive and is a function of the human's goals, the demands of the task and the human's capabilities, which include both declarative knowledge (facts) and procedural knowledge (inferencing processes applied to facts). Therefore, if I want to explain and predict human behavior, I need to know what the human wants to accomplish, what the human knows and what the task requires of the human, or any intelligent agent, to solve.

I hope this helps.

Jim Peters

==========

Date: Thu, 19 Sep 1991 08:03:39 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: deaf speech

[From: Bruce Nevin (910919 0704)]

Gary Cziko (910918.1238)

A deaf man here at BBN clearly controls his articulation for lip-readability. The sounds are quite unintelligible to me and to most others.

He has also a characteristic of voice in common with partially-deaf people that I have known, and that is use of breathy voice. I believe the reason is that this type of laryngeal activity is more perceptible as voice vibration when you can't actually hear the vibration. It is more easily felt in the tissues of the throat. With ordinary voicing, a quietly spoken utterance is not easy to distinguish tactilly and kinesthetically from a loudly spoken one. Breathy voice, I believe, is a way of assuring oneself of being audible to hearing persons when one is deaf.

Ability to control non-acoustic perceptions so as to meet acoustic norms would show only that it can be done not that it normally is done. I would consider it quite a remarkable feat. It would be fascinating to find out how it is done and how it was learned.

Bruce Nevin
bn@bbn.com

==========

Date: Thu, 19 Sep 1991 08:21:00 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: baby

(John Maag 9/18/91 15:29)

>Having a baby in three weeks

Congratulations, John, and much joy to you!

Bruce
bn@bbn.com

==========

Date: Thu, 19 Sep 1991 04:19:00 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Dag Forssell <0004742580@MCIMAIL.COM>

Subject: Control Systems for introduction

(from Dag Forssell) [for Bill P]

Today I put together a number of simplistic control charts to illustrate different applications in our daily lives.

I remember reading about gun sights in "Living Control Systems". To illustrate this, and point out it's importance to the success of the allied war effort would be neat, especially if as I suspect, this is a clear example of a hierarchical control system.

On a battleship, wallowing in the sea, is the gunner (Fire control officer, "aimer") aiming by a coordinate system (polar coordinates relating to the horizon and north) (for heavy guns) or by direct aim (anti-aircraft). Is the anti- aircraft aimer supported on a gyro stabilized platform to make his task easier (destroyer in storm)?

How would this be organized? With direct aim, the angles would be relative the deck, and the CS a simple one. But with gyros involved, we are dealing with either two circuits side by side and a summation of output or with a hierarchical setup. Either would be of interest as we get into the model of our own CS. I suspect you will tell me three CS. Two above summed into one below.

I am sure you have some input for me! Are there other reasonably well known applications that are hierarchical?

The CS theories came out of telephone amplification circuits. I want to portray these as well, to illustrate that even a rapidly changing reference signal is followed faithfully by a CS with enough amplification. This will later lead to the suggestion that a rapidly changing reference signal to the muscles of a concert pianists fingers can be faithfully followed.

My question is: How to portray the input signal and the output signal (graphically, clearly, simply, self evident) so that the faithfulness and amplification are both portrayed.

Got your letter. Thanks for missing lines. Called Newark & will get catalog & Sprague details. Look forward to building an actual servo. Have some ideas on how to arrange it, but they are sure to change. Do you have any suggestions on servo handbooks, perhaps they have answers to my questions above.

Dag Forssell
23903 Via Flamenco
Valencia, Ca 91355-2808
Phone (805) 254-1195 Fax (805) 254-7956

Internet: 0004742580@MCIMAIL.COM

==========

Date: Thu, 19 Sep 1991 08:52:11 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: jbjg7967@UXA.CSO.UIUC.EDU
Subject: SLA methods

Bob Yates (910918)

Thanks for the refs. I believe S. Smith and one other person are the authors of a set of materials called _Learning to Learn English_. Are you familiar with it? It came out in 1989, by Cambridge U. Press. In fact, if you are aware of ANYTHING else that suggests teacher behavior along the lines Bill described yesterday, I'd like to see it.

Re: methods, I'm pretty confident that if there had appeared a method that produced desired effects with 95, or perhaps even 85% of the learners, it would have been adopted immediately, institution or no. The fact is, no language learning curriculum that I know of is based on anything but correlational studies, which studies provide abysmally low correlations between/among hypothesized learner and teaching variables.

==========

Date: Thu, 19 Sep 1991 07:51:30 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: C-program

[From Bill Powers (910919.0700)]

Bruce Nevin (910918) --

Sorry about that, Bruce. I guess I suffer the disease that I object to in other programmers -- impatience with the learner. Now I'll probably go too far the other way, but it's hard to stabilize a control process when the sampling rate is this slow.

I'll talk about the oscillator first.

Let's start with an integrator. The output of an integrator is the continuing sum of its input values, multiplied by a scaling factor that gives the meaning of physical time to the interval between iterations. Integration can become complex on a computer, but in its simplest form one step of an integration looks like this:

o = o + k * i, where o=output and i=input.

This is shorthand for:

```
deltao = k*i;
o = o + deltao;
```

The first line is a differential equation,

do/dt = k*i, or do = (k*i)*dt.

... and the second line integrates it (by one small step)

We use units of time in which dt = 1 (one iteration) and give physical meaning to those units later. So you won't see "dt" in the program. I suppose I should make it explicit.

Let's hook up two integrators in a closed loop, in Basic.

```
10 y = y + k1*x
20 x = x + k2*y
30 print x,y
40 goto 10
```

If k1 and k2 are both positive numbers, this loop will run away no matter what values of x and y you start with, or what values of k1 and k2 (other than zero).

If either k1 or k2 is negative, but not both, and if their product is small enough, the above program will produce a sine-wave if run continuously. The sine-wave's amplitude will be determined by the starting values of x and y. If you start x at 0 and y at 100, you will find x varying as sine(time) y varying as cosine(time), with the amplitude being 100.

The frequency of the sine-wave (not its amplitude) is set by the product of k1 and k2. If you increase the product, the frequency increases as the square root of the product. The amplitude remains set by the initial values of x and y. This relationship is reasonably accurate when k1*k2 is very small, so it takes many iterations to create one complete sine-wave.

If you rewrite the loop so that k1 is used in both equations, then frequency will be proportional to k1. In my oscillator, a control handle is read on each iteration of the loop, and the reading becomes k1. So by moving the handle, you can change the frequency of the oscillator.

Of course this is a crude method of integration, so after a while the sine wave will drift -- it might decay to zero or get larger. Its frequency will stay the same. To make sure the oscillator keeps running without a change in amplitude, the equations are changed a little:

```
05 k1 = (handlereading)
10 y = y - k1*x + d*y
20 x = x + k1*y
30 if y > 100 then y = 100
40 print x,y
50 goto 5
```

Note that both k1 and k2 are now just k1. To make the signs opposite we use a negative sign in line 10. And in line 10, we also add a small fraction of the current magnitude of y to y. I use a value of d of something like 0.0001. This makes the amplitude of the sine-wave tend to grow slowly, so eventually it would become huge.

However, in line 30 the program tests to see if the amplitude of y ever gets greater than 100, and if it does, it resets it to 100. This puts a limit on the growth of the amplitude. The oscillator will come to equilibrium in a state where adding the tiny positive damping term in line 10 will make the amplitude 100.01, and it will get clipped to exactly 100 on each cycle. So now we have a constant-amplitude oscillator

with a frequency adjustable by the control handle.

You can calculate sine-waves by hand if you set the ks to 0.2 or so. Of course you could use 0.001 and take all day.

In the program just above, line 50 loops back to the start, so all this program would do would be to print the amplitudes of x and y. In my program, instead of looping back at 50, the program goes on to do one iteration of the filtering process and display of output, then goes back to the start and does another little step of the oscillator integrations. This is how we get the effect of everything varying simultaneously -- we're doing time-sharing among different processes, each one getting just a little lick and then waiting until all the others have had their lick, too.

A filter is an oscillator just like the above, only with NEGATIVE damping that makes the amplitude tend to die to zero. If you add to either x or y an input from a varying source (on every iteration), then clearly the oscillator will build up to a high amplitude if the input variations arrive in time with the natural frequency of oscillation. The nearer the oscillator is to sustaining its own oscillations (the less the damping), the greater will be the amplitude buildup at the "resonant frequency" and the less the input has to be off the correct frequency to lose the buildup effect. So by varying the damping we can make the filter broadly tuned or narrowly tuned.

The frequency tracker uses the phase shift that occurs when the input frequency differs from the filter's center frequency. This phase shift is converted to a dc signal (dc as opposed to ac, alternating current) which is fed back to adjust the filter frequency (k1 above) to eliminate the phase shift.

Got to send this now -- how you doing so far?

Best

Bill P.

==========

Date: Thu, 19 Sep 1991 08:49:52 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: dc winding down

Bill Powers (910917.1300)

>In the tracking filter, that output "dc" represents frequency by its
>magnitude. So if it dies out to zero, it's indicating that the frequency
>(not the loudness) is falling toward zero, a continuously descending
>note. That's what I was bothered about. Loudness would come out of
>another channel that represents total magnitude of sound BEFORE
>filtering.

Don't you have to represent the amplitude of each harmonic in order to get the damping effect that shows up as formants?

On one view, you have amplitude of glottal pulse or fundamental Af, then decreasing amplitude of harmonics AH1, AH2, . . . , AH40 (or so).

Imposed on these you have damping in the supralaryngeal vocal tract reducing amplitude of harmonics that fall in affected frequency ranges, as represented by the vocal tract transfer function (Lieberman and Blumstein pp. 40f). A heard vowel is the product of these.

I can imagine using dc to lock in 41 ECSs (elemental control systems) to track the relative amplitudes of the fundamental and the first 40 harmonics. These would be the vowel systems. Others would perceive bursts and transients of consonants as interruptions to the stream of vowels and silences under intonation contours.

If dc is used to identify frequencies, it would play only an indexing role in the whole control system for speech. I wouldn't worry about it "winding down" when it encounters silence, since the value of dc in itself need not be passed up the hierarchy as a perception once other ECSs are on track. I would be concerned about response time e.g. shifting from hearing a high voice to a deep one. Also, we presumably don't want it kicking in for every non-speech sound that is heard, only for speech and speech-like sounds. Perhaps both problems could be handled by redundancy--associating a particular remembered value-range for the fundamental with person A, another with person B, together with other characteristics. Pandemonium is certainly an interesting place!

Bruce Nevin
bn@bbn.com

==========

Date: Thu, 19 Sep 1991 09:10:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: TJ0WAH1@NIU.BITNET
Subject: skinwalker skinnerSthat skinwalker Skinner

[from Wayne Hershberger 910919]

My new modem seems to be working fine. My old one was fried last week when our house was struck by lightning. The house is ok, just a few singe marks here and there, but the electrical devices took a beating: TVs, stereo, computer.

(Bill Powers 910914)

>On second thought, there was really no chance of Skinner's
>discovering control theory, after all. He held in his hand the
>fragile little skeleton balancing on its smoothly lubricated
>stack of joints, tugging on its own bones through elastic bands.
>He felt it matching its effort against his effort, pushing the
>platform out from under itself, and saw it remaining still as a
>consequence. And then he let it spring off into space, out of
>his grasp.

Bill, you wax poetic. What a charming allegory.

But beware of that skinwalker fellow Skinner. His tormented spirit now seems to be hurling lightning bolts down upon his critics. [Obviously, I've been reading to many Tony Hillerman murder mysteries replete with Navaho mythology (a skinwalker is a Navaho witch).]

(Gary A. Cziko 910910)

>Could...you give me the page numbers for:
>Goldstein, D. M. (1989). Control theory applied to stress
>management. In Wayne A. Hershberger (Ed.), Volitional action:
>Conation and control. Amsterdam: North-Holland.

You bet: pages 481-491.

Gary, I have just had the pleasure of reading your paper for
Educational Researcher. Smooth, very smooth.

Could you please send me a zipped version of Rick's spreadsheet
program for Lotus 1,2,3? I have the software to unzip it.
Thanks.

Warm regards, Wayne

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

==========

Date: Thu, 19 Sep 1991 10:17:51 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: C program

Thanks for the elaboration. Much of the fault is just not having
sat down to work through the code. I will look at it and at this
explication on the train tonight.

My dad tells me he can see no way of implementing the filter-modify-
remix setup in hardware without considerable expense for components.
Much to my surprise, he said that components cost much more than
does equipment made from those same components. Economies of scale
for the manufacturer. I will see him the last weekend of this month,
and will give him your several messages about implementing these
things in code. He has done some things in BASIC on his little
Commodore 128, and might get interested. As a radio engineer he
has the background in oscillators, FM, mixers, and so on, and might
very well come up with some interesting angles. So your explication
will be useful to him as well, since his programming experience is much
more limited than mine.

Bruce Nevin
bn@bbn.com

==========

Date: Thu, 19 Sep 1991 15:28:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: TJ0WAH1@NIU.BITNET
Subject: suitcase demo

[From Wayne Hershberger 910919]

(Dag Forssell 910910)

>I would like to build a simple control system demo to fit in a
>briefcase and be powered by regular housecurrent.

You might want to consider the control systems used to pilot radio-controlled model aircraft. They are battery powered. You would not need the radio, just a servo and a device to provide the servo's reference input. You can purchase a servo in about any R/C hobby shop for about $25. The servo driver you may need to order special. Ace R/C Inc. [Box 511, 116 W. 19th St. Higginsville, Mo. 64037; telephone: (816) 584-7121] calls their product a "Servo Cycle." It costs about $30. Ace R/C sells their products as kits or as assembled units. If you were to get everything assembled from Ace R/C, I estimate that it would cost you about $85. You would need:

- Ace R/C Bantam servo: $26.45
- Servo Cycle (with connectors for Ace servo): $32.95
- 4.8vdc, 500ma, nicad battery pack: $20.00?
- battery charger for nicads: $8.00?
- Postage $3

****

(Ed Ford)

I got a postcard from Poland requesting a copy of your ABS chapter, "Understanding control theory." Could you please send a copy to:

Dr. Henryk Kulas
Laboratory of Psychology
Academy of Physical Education
ul. Wiejska 1
80-336 Gdansk
Poland

Thanks.

Warm regards, Wayne

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

==========

Date: Wed, 18 Sep 1991 15:16:28 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: UPROBER@BOGECNVE.BITNET
Subject: Request for help on kids mental health project

[From Dick Robertson]
I'm looking for help with a request to evaluate the feasibility and existing resources for a project with the following requirements:
1) Statistics estimating the proportion of lower elementary school aged (1st

through 5th grades) children in this country who indicate need for preventive mental health care;
2) An interactive program for screening kids via computer, in schools where kids have routine access to computers, by which kids could get assistance for mild mental health concerns and through which a school psychologist could be alerted from a kid's performance to emergent needs for special care. needing intervention by a clinician.

I know that there have been a number of computer-counselor programs developed, but I have lost touch with who and where. Also, does anyone know whether any have been developed especially for kids? Is anyone on the net working in this or related area, or know of references to it? Thanks, in advance, Dick.

Department of Psychology, Northeastern Il Univ. 5500 N. St. Louis
Chicago, Il 60625 Ph 312 794 2587 uprober@bogecnve
Home: 5712 Harper Ave. Chicago, IL 60637 (312) 643 8686

==========

Date: Fri, 20 Sep 1991 08:12:07 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: BE PATIENT WITH ME

FILE: CSG-EM12

[From Chuck Tucker 910920.0800 EDT]

Dear CSG'ers,

I have been reading the posts on the NET on a daily basis but I have not been able to hold myself together sufficiently to deal with all of the interesting and exciting ideas that I find; this is still one of the few places that a person can learn about human behavior. I wish I could comment adequately on a number of the threads that have appeared in just the last month but for some I don't know enough to comment and for others I feel that it would require at least a 20 page post. So, instead, I will just mention a few matters that have struck me and report more on them at a later time.

SOCIAL CONTROL or CONTROL FROM THE OUTSIDE

It seems to me that several comments that I made earlier this year on the NET are relevant to the issues of "external control." I stated about "sociocybernetics" that:

> Society, social structure, social class, culture, or group pressure do <<not>> make people do anything.
>
> Personality, socialization, and social background do <<not>> make people do anything. (Rather, these provide resources for action, but determine none of it.)
>
> Social life, by which I mean living and acting together, depends on arrangements people make.
>
> People guide their actions by directions they give themselves.

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

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

Technology does <<not>> make people do anything. (Rather, technology provides resources for action.)

Social norms, rules, values, beliefs, customs, traditions, laws, or social sanctions do <<not>> make people do anything. (Rather, these are devices people use to facilitate living and acting together.)

Genetic inheritance or any other biological factors do <<not>> make people do anything. (Rather, these permit people to do what they do, and, undoubtly permit them to do much that so far they have not done.)

Without making arrangements people are socially incompetent.

People can <<not>> be made to do anything, unless they are literally and directly and physically forced to.

These speak quite directly to the matters of "external control" but I don't believe that those of us who use the word 'control' in the title of the model we use will ever be able to avoid the problem of others interpreting that word as meaning "control by others," "force," "manipulation," "external influence," "environmental cause," unless we clearly point out, as Bill does in his post of 910822 (last page) " . . . control is a technical term: it means stabilization of a variable against arbitrary disturbances (as well as the ability to change the value around which stabilization takes place)" or as he defines it in BCP "CONTROL: Achievement and maintenance of a preselected perceptual state in the controlling system, through actions of the environment that also cancel the effects of disturbances (283)."

But most people do not use this definition of 'control' and sometimes some of us forget and use it in a non-technical sense. All of the literature in sociology, psychology and social psychology that I have examined (I re-examined some of very recently) uses the term 'control' to mean either control by outside forces or forces responding to the violating of norms, rules or laws (this is also found in my dictionary where control comes from the Latin 'contra' meaning "against"). So to have others understand what we are talking about and interested in we will have to preface our remarks with the technical definition of 'control,' or make up other words for 'social control' (like 'influence,' or 'reciprocal influencing' [Simmel stated: "That which constitutes "society" is evidently types of reciprocal influencing. Any collection of human beings whatsoever becomes

"society," not by virtue of the fact that in each of the number there is a life-content which actuates the individual as such, but only when the vitality of these contents attains the form of reciprocal influencing. Only when an influence is exerted, whether immediately or through a third party, from one upon another has society come into existence in place of a mere spatial juxtaposition or temporal contemporaneousness or succession of individuals." in Park and Burgess p. 139]. Or another alternative is to always use the phrase 'perceptual control' and clearly distinguish it from 'social control' or 'reciprocal influencing' but do not use the word 'control' alone. Of course, each of us can give that direction or instruction to him/herself but following it is always a difficulty.

It seems to me that the posts of McClelland (910909), Powers (910910.0700), Nevin (910910 1221), Powers (910911.0700), Powers (910912.0700) and Nevin (910912 1243) should be read as a set with the focus on the issue of how PCT deals with the "SOCIAL." I see a wide area of agreement that language is crucial because it is USED by people for their perceptions, to adjust reference conditions and to even adjust loop gains as well as being crucial in the reorganization process. Roles are a handy illustration of how this is done since a person will evoke a role not only to "control" his/her own action but will ask another to "control" to do similarily, as in "I'm am your Father," "Don't call your Mother "her" she is your Mother," "This is Dr. Tucker speaking," "I said that when I was a member of the administration but now I'm a Judge," ["Here come da Judge."] "He's not Bush, he is President Bush," [not Busch either] "I'm transferring you a call from the President." Now not everyone will act exactly the same when such statements are made but my bet is one would observe very similar actions from the receiver of such statements. We are not robots but we can organize our conduct in ways that are quite predictable to ourself and some others. If you TREAT such statements as self instruction or directions then you should remember that the last person to give an instruction for perception is the person him/herself but surprisingly we often TAKE another's statement and USE it as an instruction for ourself in a way quite similar to what the other would do it it he/she USED the instruction for him/her self and it seems to make for "co-control" or "reciprocal influencing." George Mead used the phrase "taking the attitude [physical position of the body] or role [as illustrated above] of the other" for such co-action within a "social act." Now, what would be very useful it to have some carefully done studies which would illustrate the point.

MUSINGS ON STUDIES OF WORDS SUCH AS ROLES

I have been think about studies which were done by Garfinkel "The Routine Grounds of Everyday Activity" SP (1967) and some of his student with the idea of "breeching." These studies were designed to disturb to see what other were "controlling for" even though Garfinkel and his friends did not clearly recognize that is what they were doing since their theory was a normative and orderly one where they thought they were revealing the "true nature of social life." [No one we know would ever talk about the TRUE MODEL OF BEHAVIOR]. I also think of the "Candid Camera" stunts done by Allen Funk especially those dealing with physical

matter [e,g., asking the gas station attendent to look at a car which he discovers has no motor; a naked lady getting on an elevator and a man saying "I like your outfit."] Recently, I have been looking at Jean Piaget's "clinical method" which appears to me to rely on questions which disturb to find out what children are "controlling for" but I must admit that I have just begun examining this aspect of his Piaget's work. These sorts of "studies" are beginning to form a program which I think might be quite useful for examining questions about the use of language. IF ANYONE HAS COMMENTS ON THESE MATTERS I WOULD BE DELIGHTED TO RECEIVE THEM.

CODA

Most of the discussion of sound makes no sense to me since I don't know the sounds of the letters of the alphabet except for the NAMES of them as in A sounds like A not "short A" or "long A" since I was in school at the time they experimented with "teaching reading by sight" not by phonics. I have to hear a word pronounced to know how it sounds [I still don't know how to say Cariani so I call him "Peter"]. My wife, by the way, was in the same school system but her father drilled her on phonics and she is constantly correcting me (which can be a distrubance) as she does he five year old students. I mention this not to gain some sympathy for my handicap but to point out that any study that is done should get "baseline" information about the abilities of the subjects for speaking and hearing.

I will have some comments on "How and Why" as well as Skinner in a post next week. Have a good weekend.

Best, Chuck

==========

Date: Fri, 20 Sep 1991 08:13:40 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: IGNORE IF YOU HAVE RECIEVED-EXCUSE

SOUTHERN SOCIOLOGICAL SOCIETY

ANNUAL MEETING

APRIL 9-12, 1992

NEW ORLEANS

CALL FOR PAPERS

The theme of this annual meeting is "Will the Center Hold? Linking Sociology to Its Specialities and other Disciplines." One of the questions posed by the President of the SSS that is relevant to this session below is: "How do they utilize the findings and theory from other disciplines and incorporate them into sociology?" I would like to see some papers discussing not only another area (Cybernetics) has influenced sociology or social psychology but also how the reverse is the case or a

challange to the claims that there is any influence either way. Any type of paper from abstract theoretical to refined experimental is appropriate in this session but what I would like to recieve is an abstrast for such a paper or a paper that can be presented in about twelve minutes (about six double spaced page) rather than some paper that will have to be reduced before the meetings. The title of this session is:

CYBERNETICS AND SOCIAL CONTROL

Please send abstracts or papers to me by OCTOBER 15, 1991 by either snail or Email.

Chuck Tucker
Department of Sociology
University of South Carolina
Columbia, SC 29208

BITNET: N050024 AT UNIVSCVM
OFFICES: (803) 777-3123 or 777-6730
HOMES: (803) 254-0136 or 237-9210

==========

Date: Fri, 20 Sep 1991 09:36:25 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: review of Peckham, instructions

[From: Bruce Nevin (910920 0747)]

Some time back, Chuck Tucker sent me a photocopy of the introduction to

Peckham, Morse. 1985. _Romanticism and ideology: collected essays III (?)_. Greenwood, Florida: The Penkevill Publishing Company.

I would like to review that introduction here. Chuck says: "It presents the view of language as instructions; all "signs" are instructions!" There is much in it that I agree with, and much to which I must take exception. Chuck's note says it is a summary of Peckham's previous book:

Peckham, Morse. 198?. _Explanation and power: the control of human behavior_. New York: Seabury Press.

It is understandable that a summary of a book should be very densely written. Possibly because of Peckham's notion of explanation as subsumption in a hierarchy of categories, the language is also abstract, not to say abstruse. These characteristics together with perhaps some lapses of discipline in the control of them make it a bit tricky sorting the wheat from the chaff--as evaluated from a CSG perspective, of course.

By way of preliminaries, Peckham's other books include:

Beyond the tragic vision: the quest for identity in the nineteenth century
Humanistic education for business executives

Man's rage for chaos: biology, behavior, and the arts.
Arts and pornography: an experiment in explanation
Victorian revolutionaries: speculations on some heroes of a culture crisis
The triumph of romanticism: collected essays
Romanticism and behavior: collected essays II
Charles Darwin's _Origin of Species_: a variorum text

-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-

Peckham starts out saying we have to understand meaning in behavioral terms ("the only generally useful comprehension of `meaning' is a behavioral comprehension, the place of meaning in the rest of human behavior"). Immediately, he makes clear that he is operating in a S-R perspective: the meaning of an utterance is for him any response to it.

That response is appropriate in the judgment of someone, hence, all utterance is normative. Any descriptive utterance is also a prescriptive utterance. As I understand it, Peckham means by this that e.g. the word "book" not only "describes" a class of objects, but also prescribes behaviors appropriate to this class.

We arrive at a similar conclusion about norms and prescriptions being packed into the presuppositions of language from a CT perspective, but without having to take behavioral outputs directly as responses to "verbal behavior." A simple declaration "the book is on the table" carries with it presuppositions that prescribe a universe of possible actions that you, the hearer, might take with respect to books, tables, and the relation of one being on the other. Those presuppositions are there because you have learned to use those words in physical and social contexts in which they were descriptively accurate and in which certain kinds of actions or attitudes were taken (or prescribed in even more explicit words and actions) with respect to them. They are presuppositions because I presume that you are familiar with a universe of such remembered experiences, you presume that I am, and each of us metapresumes that the other is making that presumption. By these shared presuppositions we coordinate control of our respective perceptions of our shared environment. Such are among the preconditions for communication. This is what is meant when we say that the meanings of words (as well as their phonemic constituency) must be pre-set in speaker and hearer prior to their active use. Bruner indicates the bootstrapping by which this pre-setting is accomplished.

I judge your behavioral response appropriate or inappropriate not according to the words I use (to which indeed I may not closely attend) but according to the goals I maintain at the moment. It might be a novel response, novel even in kind, and be perfectly appropriate if my goals were indifferent to book and table and your relation to them or if my goals called for a novel response.

In these cases, to be sure, the novel response would have nothing to do with the meanings of the words--but behavioral responses never did! Peckham may be saying that the behavioral responses constitute signs and that the signs, not the behavioral outputs, are the meanings--his notion of `semiotic transformation,' of which more later--but this is problematic for the same reasons (Guthrie's lens). Even when categorized and abstracted to signs, behavioral outputs correspond to meanings only insofar as they correspond to the internally held

reference values which are a better ground for meaning.

I believe that Peckham is saying, here and elsewhere, that all verbal interaction is manipulative. I am sorry that he finds it so--and we are forewarned. More speculation on this later.

Peckham proceeds to identify explanation with categorization. If term A is subsumed in category C, and term B is subsumed in category C, then use of C prescribes that we should respond to both A and B in the same way, and that for our response non-C-like characteristics A or B are irrelevant. Because all (language) terms are categorial and all categories "subsume with neglect" (of irrelevant features), all utterance is fictive.

I agree that categories specify relevance. The intensive definition of a category amounts to a specification of differences that make a difference. They make a difference with respect to goals that we might have. Insofar as they are conventionalized, they prescribe something about goals that we might have or might be expected to have.

I have discussed this previously. There are other and more adequate modes of explanation. One may make the analogy of A and B explicit, under which they may be subsumed under C. One may use an instance of A to model B, or vice versa, and test the model.

Since Peckham does little with the word "explanation" beyond kind of swinging it around in the air, I am not sure how much to make of this inadequacy. It may be that he is defining the term in a limited way as a stepping stone to his notions about manipulation and power. Further on (pp. 3-4), Peckham distinguishes this limited sense of `explanation' from `interpretation':

> Interpretation is a perceptual disengagement of an analogically determined recurrent semiotic pattern from an analogically determined series of semiotic matrices.

This sounds suspiciously close to use of analogy for its explanatory power, though the relation of patterns to matrices may devolve to the relation between terms in the former and respectively subsuming categories in the other. Both mythology and science, he says, are modes of interpretation

> for both are derived not from empirical data (the world of semiotic configurations [patterns?]) but from preceding and less developed explanatory regresses. Science differs from mythology in not resting on a judgment of an explanatory regress as a stable guidance, but in exploiting the instability and non-immanency of all levels of explanation from sign configurations in the natural world to that termination of explanation known as scientific theory. Thus experiment modifies explanation by generating semiotic material (or data) which the current explanation cannot successfully subsume. This is feedback. Science is merely the most complete model of the semiotic hierarchy from configuration to the termination of an explanatory regress, since it depends upon the capacity for randomness of response.

But scientific explanation is not limited to categorial subsumption.

Peckham says that meaning is not immanent in utterances. By this he means that "there is no necessary subsumptive relation" in language. Nonverbal categories depend on the sharing of attributes by subsumed terms. Verbal categories do not depend on the sharing of attributes. This seems to correspond to the familiar observation that linguistic signs are arbitrary (with the marginal exception of onomatopoeia, etc.) Peckham, curiously, says that they are random. From the point of view of nonverbal behavior, he argues, the arbitrariness of verbal behavior has the appearance of randomness. This randomness presents a threat of instability and dissolution. Therefore, says Peckham, explanatory hierarchies are needed to control this eruption of randomness, to control verbal behavior. There is no metaverbal position that transcends verbal behavior, so we use these explanatory hierarchies within language instead. He makes glancing appeal to an evolutionary process at the emergence of language, but without elaborating the point to any coherence. None of this is persuasive to me.

Conventionalized behavior is arbitrary, cut off from its origins in the variable outputs of perceptual control. Instead of reaching and failing to grasp an object, the infant reaches with effortful noises, intending thereby to get help. Instead of reaching with effortful noises, the child points with a conventionalized reduction of the effortful noise, which now has the status of a kind of word of request. But that does not mean it is random. Far from it! Conventionalized behavior has a lot of the variability of goal-seeking behavioral outputs *removed* from it. This is because it is the behavioral output itself which has become a goal, subordinated to the higher-level goal to which it refers. I believe Peckham is simply wrong in characterizing verbal behavior as randomness that must be controlled by his explanatory hierarchies in order for verbal interaction to be possible.

Rather, a conventionalized behavior is a tamed, domesticated representative of the great variety of behavioral outputs that might be undertaken in pursuit of a goal. Rather, it is a representative of that goal, and a way of exteriorizing it for the sake of communicating it. Words and signs then are normative, prescriptive, and yes can be manipulative, precisely because they correspond to goals held by one person that might be shared by another person if communication is successful. In this way we approach a view of signs as instructions without the baggage of power and manipulation that Peckham finds necessary because of his external, S-R perspective.

We are on familiar ground when Peckham says "When a configuration or figure is perceptually distinguished from its ground, it is then a sign. . . . As the world comes into our perceptual field, the world turns into signs." This is the category level, or we might call it the sign level, or the semiotic level. "Response to signs entails the production of [other] signs. Thus the most precise definition of human behavior is `semiotic transformation'." (Looking for an explanation of behavioral outputs, again.) On the category level this is indeed the appearance of the world and the appearance of human behavioral outputs. It is of course not all that is going on. Additional levels of perceptual control may be inexplicable and "random" to one stuck on the category level, and that may give rise to an experience of powerlessness and a manipulative approach to exerting power. But I speculate.

Also problematic here is the "inheritance" of arbitrariness from the category level to all higher levels built upon it. Part of the problem

is that from the category level on up all we seem to have is words. I say "seem to have" because there may be category perceptions, and sequence, program, principle perceptions, which we control without use of words, although we can use words to describe them. This is only to say that the structure of language, which is to a great degree socially standardized (we control for conformity) is not to be identified with the control hierarchy on these higher levels. Endless confusion ensues because all we have to discuss these is our descriptions of them using language. We have to find ways of using language to provide pointers, recipes by which we may come to attend to the perceptions themselves.

From the imputed threat of randomness in verbal behavior, Peckham argues that semiotic behavior *must* be controlled. This is a contest between the intrusion of barbarians, excuse me, randomness and the economic need for interaction. Explanation (categorial hierarchy) controls behavior. Complementary to this, he says, is

> the recognition that all behavior is aggressive, in that the organism has no choice but to struggle to control and exploit the environment, including, for human beings, the centrally important verbal environment.

I leave it to you to determine whether this depiction of life as a struggle to control and exploit the environment is adequate, insofar as it may be apt. On it rests Peckham's notions of explanation as power.

Peckham seems to understand something like reorganization in learning. The brain can generate random responses. Learning is this plus the "selection or validation" of one of these responses. The selecting and validating depends on "channelling" of behavior, which is "the task of any learning situation."

> Channelling depends first upon unreliable remembering, which must be supplemented by the constant reiteration in various semiotic modalities of the same instructions, that is, by cultural redundancy, and redundancy is supplemented by policing, the use of force. Thus meaning is ultimately stabilized by the ultimate sanctions of economic deprivation, imprisonment, the infliction of pain, and execution. In modern societies the first three are constantly used in the socialization of children, and in the past the fourth was available, as it still is in certain less developed societies. But if force fails, there is no recourse; therefore culture (or civilization) has as its principal task the maintenance of behavioral stability by circumventing the use of force. The modes of circumvention are the two basic rhetorical modes of seduction and intimidation. Because culture depends upon [meaning, which is non-immanent and therefore unstable], it is constantly threatened with disintegration. . . . Controls . . . are set over culture. These controls are social institutions, of which five may be distinguished: teaching-learning institutions . . . , value institutions [which maintain the individual's self-esteem] . . . , economic institutions, governmental institutions, and ideological institutions (sciences, the arts, philosophy, scholarship). The structure of the interaction among institutional levels is the structure of explanation, for the verbal behavior within an institution is that of an explanatory regress [categorial hierarchy]. An institution is an explanatory regress. An ideology itself consists of the high and terminating levels of an

> institution . . . . [These are really five institutional functions of every institution.] Cultural history is the gradual emergence of institutions which specialize in one particular kind of institutional control over culture, using the other kinds as subsidiary and subordinate modes of control.

The tension between individual and institution, and between individuals who identify themselvesl with different institutional levels, rests ultimately, it seems, on the "fictive" nature of the category level. It is those features and behavioral outputs of the individual (and the goals or reference values that they represent) which intrude seeming randomness in the tidy but sterile world of social convention and bring about their destruction or their change by people mutually controlling for them. Mutual or coordinated control of shared reference values remains a challenge for Control Theory: first, as to how they become shared, and then as to how they continue to be shared in real-time interactive control. Peckham sheds no light on this, at least not in this introduction, though he has interesting things to say about institutions and people in them and may in fact have more to say in his essays that bears on our issues.

I suggested that signs are domesticated representatives of goals. Conventionalized behavior can come to represent a goal by stylizing the possible behavioral outputs that might be undertaken in pursuit of the goal, exteriorizing it for the sake of communicating it. Initially, a gesture may represent a category of like gestures. Once conventionalized, it may represent unlike gestures, and in the changed renditions of successive generations of people it will become increasingly unlike the behavioral outputs that it represents. One can see this, for example, in the development of writing systems from pictures, as well as in Bruner's account of requesting, inviting, etc. As it comes to represent the inwardly maintained goal its relation to the details of behavioral outputs becomes less relevant. Consequently its relation to outward referents becomes likewise more and more arbitrary. But though the details of behavioral outputs for a word "book," say, have no relation such as mimicry or onomatopoesis to the corresponding goal, that does not mean they are irrelevant or random. On the contrary, the behavioral outputs themselves become a subsidiary focus of perceptual control, or in the case of words the immediate perceptual consequences of behavioral outputs in an acoustic signal, marks on a page or screen, etc.

I said that words and signs are indeed normative, prescriptive, and can be used in manipulative ways, precisely because they correspond to goals held by one person that might come to be shared by another person if communication is successful. I suggested that in this way we can approach a view of signs as instructions without the baggage of power and manipulation that Peckham finds necessary because of his S-R perspective. Signs are instructions, but not in the sense of instructions in a computer program. They are instructions in the sense of directions in a cookbook, perhaps. If the recipe says serves 4 and you are making it for 6 you adapt. You ignore instructions that are not relevant (egg substitute, adjacent recipes, etc.) and you interpolate instructions that are missing according to your own goals and your own construal of means for attaining them. They are instructions only by virtue of relevance to those internal goals and means, and only because we take them up as shortcuts to attaining desired ends (a layer cake, or coordination with others for the sake of some other goal), a substitute

that is faster than arriving at appropriate reference values by trial
and error.

This is a key to social reality, I think: without learned conventions we
would invest so much of our time and energy in trial-and error
calibration of subsidiary goals that we would not have enough remaining
to accomplish the further goals that they are intended to support.
Social conventions then are similar to automatized behaviors in their
function.

In Peckham's world, there is much more of compulsion, threat, and fear
than I have suggested. I believe this is descriptively accurate in that
people in many cultures do in fact invest a great deal of their capacity
for control in institutions as they understand them. The coupling of
sanctions and fear in institutions is indeed one way to effect the
sharing of reference values. The study of the variety of cultures
suggests that it is not the only way.

        Bruce Nevin
        bn@bbn.com
==========
Date: Fri, 20 Sep 1991 09:40:22 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Comments: Resent-From: "Bruce E. Nevin" <bnevin@ccb.bbn.com>
Comments: Originally-From: Wayne Rindone <wrindone@BBN.COM>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: neural modeling software

From
 news.bbn.com!micro-heart-of-gold.mit.edu!wupost!sdd.hp.com!elroy.jpl.nasa.gov!u
 sc!neuro.usc.edu!connor Fri Sep 20 08:48:05 EDT 1991
Article 207 of bionet.neuroscience:
Path:
 news.bbn.com!micro-heart-of-gold.mit.edu!wupost!sdd.hp.com!elroy.jpl.nasa.gov!u
 sc!neuro.usc.edu!connor
>From: connor@neuro.usc.edu (John Connor)
Newsgroups: bionet.neuroscience
Subject: Re: Compartmental models of single neurons.
Keywords: Neural and Brain Modeling
Message-ID: <36041@usc.edu>
Date: 20 Sep 91 08:15:06 GMT
References: <1991Sep18.171232.16646@colorado.edu>
Sender: news@usc.edu
Organization: University of Southern California, Los Angeles, CA
Lines: 42
Nntp-Posting-Host: neuro.usc.edu

In article <...> shah@spot.Colorado.EDU (SAFWAN SHAH) writes:
>The book Neural and Brain Modeling by Ronald J. MacGregor (Academic Press)
>1987, contains excellent discussions and the Fortran code to model single
>neurons. If required I can e-mail the source.
Most of the programs in the MacGregor book are point neuron models
rather than compartmental models. The few compartmental models seem
to depend on hodgkin-huxley parameters and conditions. The following
information described how to get a copy of a somewhat more flexible
neural system compartmental modeling program:

Here's how to get a preliminary copy of NEURON via ftp.

```
ftp sunjwm.neuro.duke.edu (128.109.232.3)
Name : anonymous
Password: your-name
cd pub/neuron
cd nrn-unix  (compressed tar files)
         ( cd nrn-dos for pkzip files in self extracting .exe form)
dir
prompt
binary
mget *
quit
```

NEURON now allows users to add their own membrane mechanisms using
a high level model description language. This part is very new however
and I have not yet exercised it thoroughly.

I am interested in your comments and suggestions about the program.

To help me in maintaining a list of users (assuming you want to
be on such a list) send me an email message of the form:

```
mail hines@neuro.duke.edu
subject: NEURON user
name
email address
real address
```

==========

Date: Fri, 20 Sep 1991 08:14:21 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: CSG Newsletter & Dues

from Ed Ford (910920.0815)

To All Conference Participants: Hugh Petrie and Bill Littlewood are the
only people who have sent copy for the CSG Newsletter. I plan to send
out the CSG Newsletter in early October, as soon as I get the third
quarter edition of Closed Loop from our editor, Greg Williams. Also, a
gentle reminder to those who haven't paid their 1992 dues. Closed Loop
is sent only to paid up members. Dues (now $40) payable to: Control
Systems Group and should be sent to: Mary Powers, 73 Ridge Rd, CR 510,
Durango, CO 81301.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, Arizona 85253 Ph.602 991-4860
==========

Date: Fri, 20 Sep 1991 08:32:24 PDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AERO.ORG
Subject: Spreadsheet

[From Rick Marken (910920)]

David Goldstein--

I received your request for a copy of the spreadsheet. I was unable to send a private reply -- what is your address? Anyway, I don't have a copy available right now. Perhaps Bill Silvert could send you the ascii version. If you have problems, I will be happy to send you a disk via snail mail.

Kent McClellend--

Chuck Tucker just referred to a post of yours on 910909 on social control. I think I missed it because it was posted when the computer here was kaput. If you have a copy could you send it to me. Thanks.

Jim Peters--

Thanks for the reply. Could you send me a reprint of the article you mentioned. I would like to see what you did -- moreover, my wife used to be an auditor and she is interested in it as well (a rare convergence of interests for us -- she usually sticks to Jane Austin while I handle the computer modelling). Maybe we could discuss this on the net (once I have read it) to see if there are differences between your approach and that of a control theorist. Your statement regarding what you want to know about human behavior is certainly compatable with a control theory perspective.

Your stated assumption that

> human behavior is adaptive
>and is a function of the human's goals, the demands of the task and the
>human's capabilities, which include both declarative knowledge (facts) and
>procedural knowledge (inferencing processes applied to facts). Therefore,
>if I want to explain and predict human behavior, I need to know what the
>human wants to accomplish, what the human knows and what the task requires
>of the human, or any intelligent agent, to solve.

seems very compatable with the control theory perspective. What you call "adaptive" I would call "purposeful". Goals, of course, are reference levels for perceptual input variables. The "demands of the task" include contraints on how outputs can influence these input variables (the "output function" in the control model) as well as the effect of disturbances on the input variable. I see "declarative and procedural" knowledge as part of the output function as well. In the hierarchical control model these are the functions that control the appropriate perceptions to produce the higher level goals. So if one of my higher level goals is "getting to work" then my procedural and declarative knowledge consists of the lower level control systems that produce the pereptions ("seated in driver's seat", "motor purring", "405 south") that ultimately produce "being at work".

I would say that control theory would say it makes a great deal more sense to try to find out what the human wants to accomplish (this is the main goal of control theory research) than to find "what the task requires of the human". I'm sure you didn't mean it this way, but asking "what the task reqires of the human" makes it sound like you assume that tasks have purposes as well as the human. There is a way to test whether or not

tasks have requirements -- its the same way you test to see what the human wants to accomplish. Using this test, you will probably find that the task could care less about what gets done.

Hasta Luego

Rick M.

****

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

==========

Date: Fri, 20 Sep 1991 11:01:54 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: UPROBER@BOGECNVE.BITNET
Subject: Thanks Dag

[From Dick Robertson]
(To Dag Forssell)
>I have just read Intro to Modern Psychology, which I bought at conference last year. I find it outstanding!<
Hi, Dag. I read your note to me and Bill about the book yesterday. Thanks, it was very nice. It feels good to learn that someone liked it. I don't get that very often from students. Many of them seem to think it's hard. But I did get some insight on that yesterday when a student who really got converted to CT said, "Your book is harder than the regular intro psych texts because it showed me things I didn't already know; that's why I liked it, because I kept thinking, `I know all this already,' when I read the regular general psych book.
I also liked your idea about the portable demo gadget. I too would like to have one to take along when I go to lecture in Belgium next year. Please let me know how it comes along.

Department of Psychology, Northeastern Il Univ. 5500 N. St. Louis
Chicago, Il 60625 Ph 312 794 2587 uprober@bogecnve
Home: 5712 Harper Ave. Chicago, IL 60637 (312) 643 8686

==========

Date: Fri, 20 Sep 1991 15:21:40 ADT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Bill Silvert <bill@BIOME.BIO.NS.CA>
Subject: Mail server, spreadsheet, etc.

I have set up a mailserver on my system from which you can receive CSG materials (like the Marken spreadsheet) by email or (preferably) by anonymous ftp. For further details send a message containing the line:

help

to mail-server@biome.bio.ns.ca and you will get the server help file.

Bill

--
Bill Silvert at the Bedford Institute of Oceanography
P. O. Box 1006, Dartmouth, Nova Scotia, CANADA B2Y 4A2
InterNet Address: bill@biome.bio.ns.ca
==========
Date: Fri, 20 Sep 1991 15:11:39 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: C code, first take

[From: Bruce Nevin (910920 1446)]

Bill Powers (910914.0700) "Tracking filter; C-code"

> damp = 160; /* damping set to 0.160, equal to a Q of about 6 */

>   a = a - muldiv((input + out + muldiv(damp,a,1000)),freq,2048);

Why not:

```
  damp = 0.160 (or: damp = 160/1000)
  a = a - muldiv((input + out + (damp * a)),freq,2048);
```

Is muldiv faster than * as an operator? Or am I missing something?

Bill Powers (910919.0700) "C-program"

>A filter is an oscillator just like the above, only with NEGATIVE damping
But in the primary oscillator:
>   /* x/1000 is a little NEGATIVE damping to keep oscillations going */

Both have negative damping. What is the distinction?

I understand that in the primary oscillator amplitude gradually grows to a limit, and in the secondary oscillator used as a filter it gradually dies to zero, but I the emphatic NEGATIVE both places doesn't explain how. If x=160 by chance in the primary oscillator, then the damping factor is the same in both, no? The difference is that in the first case damping is added to what would otherwise be the wobbly x value, and a value of 0.160 is relatively substantial. In the second case, the product of 0.160 (damping) times the previous value of a is part of the sum that makes up the first argument of muldiv, so that 16% of a is in effect the damping added to input and output, and this sum multiplied by freq and divided by the center value 2048.

>   The center frequency of this filter is set by "freq." As the scaling
>   is set up, when "freq" = 2048, the center frequency is 1/(2*pi*dt),
>   where dt is the physical time represented by one iteration of this

A bit too obscure magic for me so far. I understand the dynamics of tracking as described in intuitive terms, but the determination of phase, being 90 degrees out of phase, etc. appears to be set up by a relation between constants like 2048 ("as the scaling is set up"), and the formula 1/(2*pi*dt) is I suppose the reciprocal of the familiar formula for the circumference of a circle, 2 pi R (where dt is R) for the sine wave, but the relation to the foregoing is obscure.

You asked how I'm doing so far. This is after about a half hour looking
it over on the train. I'll continue.

Got to run for said train. Have a good weekend, all!

        Bruce Nevin
        bn@bbn.com
==========
Date: Sat, 21 Sep 1991 12:32:24 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Peckham, phase, ftp

[From Bill Powers (910921.0800)]

Bruce, thanks for your review of Peckham. I have very nearly zero
tolerance for material like that; I just can't get through books in which
major red flags pop up in the first couple of sentences. It takes a
special talent (from my humble point of view) to suspend disbelief
thoroughly enough to give the writer a fair hearing. Without people like
you I would never know what many people really have to say. Fortunately,
Mary does this for me, too; she's got it. I feel like a mass of quivering
prejudices.

I'll add some metacomments.

When people make flat statements, I often test them recursively:

>"the meaning of an utterance is for him any response to it."

So if he says "The meaning of an utterance is the response to it," and I
respond "bullshit!", what becomes the meaning of his statement? If the
meaning of such a statement is in the response of others to it, why
bother to explain what the statement means, or justify it? The meaning is
not in further words that the speaker emits (for their meaning is also in
the response to them). In fact, if the meaning is in the response (even
the speaker's), then there is no meaning in anything because you can
never track a term down to its final meaning. You get word->response-
>response->response ..... and never get to anything other than responses.
What does meaning have to do with it? It's responses all the way down.

It seems to me that Zen masters spend a lot of time getting their
students to abandon the idea that meanings are in words. There's a
constant challenge to mechanical expectations: the student expects praise
for a clever answer and gets hit up alongside his head. "The sound of one
hand clapping" is a string of words. Don't let the string run through
your nose.

Well, that's just one sentence and I'm not finished. Think how long it
would take me to read that book.

Next sentence:

>That response is appropriate in the judgment of someone, hence, all
>utterance is normative.

B....! Excuse me. We don't go around judging the appropriateness of sentences, our own or anyone else's. We may be jarred by inappropriateness -- meaning hearing words for which we have a reference level of zero -- but if we had such opinions about all words we wouldn't be able to talk. What are the appropriate words in which to express a conjecture? What is the appropriate angle at which to hold the steering wheel of a car? MY utterances, at the moment, are HIGHLY normative, but that's not because my *language* is feeling normative. It's because the guy using it is.

>A simple declaration "the book is on the table" carries with it
>presuppositions that prescribe a universe of possible actions that you,
>the hearer, might take with respect to books, tables, and the relation
>of one being on the other.

I think you're bending over backward here. Presuppositions are in the receiver, not the transmitter (or rather, the *effective* presuppositions, as far as "understanding" is concerned, are). The speaker launches sentences from a background of presuppositions; the listener gathers them in through an independently-constructed set of presuppositions. It takes a great deal of labor to get these two sets of presuppositions into some semblance of operational similarity, not even to mention actual similarity or identity, even if the two individuals have a lot of time and willingness. Look at what goes on on this net. Then the speaker turns to listener B and we start all over. Of course most speakers just assume that everyone works from the same set of presuppositions, the set of which the speaker is aware (and to paraphrase what you say, the speaker's conjectures about the presuppositions of "the average listener").

Calling these "shared presuppositions" is, I think, an exaggeration. If we really shared presuppositions or any of the other trappings of subjectivity, the world would be very different.

>I believe that Peckham is saying, here and elsewhere, that all verbal
>interaction is manipulative. I am sorry that he finds it so--and we are
>forewarned.

Nicely put. By their theories shall ye know them.

>The intensive definition of a category amounts to a specification of
>differences that make a difference.

Differences, I assume, from things that are not in the category. But I wonder. I know that a customary definition of a category rests on similarities among the things that are in the category -- but "similarity" is a slippery idea itself. Things are similar if they are the same, right? Wrong -- if they were the same we would not say they are similar. Similarity implies differences. I think categories are arbitrary groupings. Dogs are similiar to cats in that they have tails and fleas. Lions are similar to cats in that neither one barks. Categories don't exist; we make them up. Then we attach words to them. Then we prop them up Out There and insist that they are real.

>   Interpretation is a perceptual disengagement of an analogically
<   determined recurrent semiotic pattern from an analogically
>   determined series of semiotic matrices.

Well, what I say is that interpretation is NOT a perceptual disengagement of an analogically determined recurrent semiotic pattern from an analogically determined series of semiotic matrices. But does Peckham pay any attention to me?

>I said that words and signs are indeed normative, prescriptive, and can
>be used in manipulative ways, precisely because they correspond to goals
>held by one person that might come to be shared by another person if
>communication is successful.

Now you're doing it. What *I* say is that words and signs are indeed not normative, prescriptive, (although they can be used in ways intended to be manipulative), precisely because they do not correspond to goals held by one person that might come to be shared by another person if communication is successful, but forever remain merely words and signs. If a word corresponds to a goal, that goal must be that word. Likes are compared. If the goal is "sugar," you will say "sugar" and thus perceive "sugar." On the other hand, if the goal is to taste some sugar, you will surely not utter the word, but sprinkle some sugar on your hamburger. The perceptual meaning evoked by the word or sign is what matters. Isn't it?

If I seem difficult this morning it's because the next thing I have to do is fill out corporate tax returns for the disbanded CSG-Inc, because the IRS says we weren't a not-for-profit corporation and therefore must have been a for-profit corporation all along. All because of those damned books. I'll give 'em profit: 0,0,0,0,0. Excuse me while I kick the cat.

I'm just going to skip a lot of stuff here. This is not my day for dispassionate commentary on Peckham, even filtered through you.

One general thought. A "goal," in HCT, isn't something separate from words and perceptions of other kinds. It's just a perception or it was, or it was synthesized from perceptions. The goal that leads you to say "sugar" isn't "Say 'sugar'", but "sugar." If the goal were "Say sugar" then you would say "Say sugar." A goal is a picture or an image or a sense of a perception as it is to be. If your goal is to stack up a bunch of blocks, the goal may consist of a sense of making stacking movements, or it may consist of a picture of blocks stacked up. In the former case you may succeed at making the stacking movements you imagined, but the blocks fall over before the stack is finished. In the latter case, you may not know what moves to make, so the blocks remain in a pile. You can describe a goal as "I'm trying to help." In that case you will probably do a lot of trying and not much helping. Before a goal can be understood it has to be translated into a target perception, a sense of the outcome as it is to be, without words (unless the goal specifically relates to perception of words). This is why it is important to look behind words that seem to describe goals and ask what perceptual situation they indicate. Only the perceptual meaning, for the individual, can determine what controlled variable is meant and what its reference level will be.

C-code.

"Muldiv" works with 16-bit integer arithmetic which is fast. But integers have a maximum value of +/- 32767. So if I want to multiply a number like 5000 by the ratio 160/1000, I would hope to get 5000 * 160 = 800000, divided by 1000 = 800, an integer. But the intermediate 800000 overflows

an integer variable and leaves a false result of 13568, which divided by 160 gives 84. It could be anything, positive or negative.

What "muldiv" does is to multiply the first two numbers to produce a 32-bit result (this is how the 80x86 microchip does it); it retains that result and divides it by the third number without first reducing the intermediate result to a 16-bit integer. So you can calculate 10000*20000/30000 and get the right answer. Using floating point arithmetic would greatly slow the computations (by a factor of 5, at least).

Negative damping: I switched mental signs between paragraphs. Abject apologies. Damping, I suppose, ought to mean subtracting a little from each cycle so that the oscillations "damp out." So that would be POSITIVE damping. The more damping, the faster the oscillations decline in amplitude. Then the first instance, in the oscillator, is NEGATIVE damping -- a little is added on each cycle, making them grow. The second instance should have been POSITIVE (or normal) damping. M. culpa.

> In the second case, the product of 0.160 (damping) times the previous
>value of a is part of the sum that makes up the first argument of
>muldiv, so that 16% of a is in effect the damping added to input and
>output...

Notice the negative sign on the left; this integrator is accumulating the negative of (input + output + damping). The negative sign applies to the damping term, too.

```
   a = a - muldiv((input + out + muldiv(damp,a,1000)),freq,2048);
         ^
```

In the oscillator, the damping term (I see I used 0.001) is ADDED, the negative sign on the left not applying to it.

```
      x = x - muldiv(y,f,1000) + x/1000;
                               ^
```

If I wanted to be really clear, I would always subtract the damping term but make the sign of the multiplier negative for "negative damping," i.e., exponential growth in amplitude. This, of course, would not confuse anyone but me.

>I understand the dynamics of tracking as described in intuitive terms,
>but the determination of phase, being 90 degrees out of phase, etc.
>appears to be set up by a relation between constants like 2048 ("as the
>scaling is set up"), and the formula 1/(2*pi*dt) is I suppose the
>reciprocal of the familiar formula for the circumference of a circle, 2
>pi R (where dt is R) for the sine wave, but the relation to the
>foregoing is obscure.

Yes, it's obscure. The constant 2*pi gets into frequency measurements all the time. It comes from measuring angles in radians, where a radian is the angle subtended if you lay the radius along the circumference of a circle like a piece of wet spaghetti. There are 2*pi radians in a circle (i.e., in a 360-degree angle). Angles get into it when you talk about sine-waves. A sine-wave fluctuation would be drawn by a point on a rotating wheel projected onto the y-axis. One rotation (2*pi radians of rotation) is one cycle of the sine-wave. If the wheel rotates 1 time per second, which is 2*pi radians per second, you get a frequency of 1 cycle per second. If it rotates f times per second, or 2*pi*f radians per

second, you get a frequency of f cycles per second. In computers, sine and cosine functions require angle arguments in radians, not degrees. One radian is 57.296 degrees. In speaking of phase shifts (rather than computing frequencies) we usually talk in degrees, not radians.

The "scaling" refers to what the meaning of one iteration is. I can sample voice input about 8000 times per second, so my least meaningful unit of time is 1/8000 sec. That is dt. The highest frequency you can have is one point up and the next point down in 1/4000 sec: i.e., a frequency of 4000 per sec (and not much of a sine wave). But don't worry about the scaling too much -- I haven't got it exact anyway.

Phase shifts:

Preliminary note. Phase relationships are not explicitly put into this filter. They emerge from its properties. The same goes for resonance and bandwidth. This is the difference between a basically analog approach and a physicist's approach to problems like this. In effect, I'm manipulating our little simulated reality here at a very low level, and letting the behavior of the system arise as a consequence. I just put integrators and amplifiers together in a certain configuration, and as an ensemble they resonate and show the properties of a tuned filter.

If you put a sine-wave into a band-pass filter, and the input is at the center frequency of the filter, the output wave will be exactly in phase with the input wave: the rises and falls will occur at exactly the same times. Because of resonance effects, the output excursions will probably also be larger than those at the input, but that has nothing to do with the phase at resonance.

If the input wave is at a higher frequency than the resonant frequency of the filter, the output will tend to lag behind variations in the input, because the input is changing faster than the filter naturally wants to oscillate. When the input wave is just passing its positive peak, the output wave will still be rising toward that peak; it will arrive at its peak when the input wave is already descending. The output and input will still have the same frequency, but the peaks and valleys of output will lag by a constant amount of time behind those of the input.

If the input frequency is lower than the center or resonant frequency of the filter, the opposite will happen. The output will start falling before the input has reached its peak. The output will "lead" the input by a constant amount.

Counting one whole cycle of the input as 360 degrees of a circle, we can measure the phase shift (lead or lag) in degrees. The greatest possible lead or lag in a one-stage resonant filter is 90 degrees -- one quarter of a cycle (or circle). If the output is lagging by 90 degrees, it will just be crossing zero going upward when the input has reached its upward peak. This amount of lag would occur only with the input frequency very far above the resonant frequency of the filter.

If the input is a sine wave (always assumed when we talk about phase shifts, leads, and lags), the output will be a phase-shifted sine wave. A sine wave shifted 90 degrees is a cosine wave. If you multiply a sine wave by a cosine wave, you get a wave at twice the frequency, by a

trigonometric identity and also in fact. Its average value will be zero.

If you multiply a sine wave by an in-phase sine wave, you get a sine-squared wave, which is of course always positive except at the zero crossings. Its average value is positive. If you multiply a sine wave by a negative sine wave (180 degrees out of phase), you get a negative average value. So you can see that if we multiply the input signal by a signal that is 90 degrees out of phase with the output of the filter (an intermediate signal that is available in my way of building the filter), the average of the product will be zero when the input is at the resonant frequency. As the input frequency changes away from the resonant frequency, the phase relationship will change away from 90 degrees toward either 0 degrees (in phase) or 180 degrees (the negative of in phase), and the average value of the product will become greater or less than zero.

By smoothing this product of waves, we get an average, or direct-current (by analogy to electronics) or "dc" signal with a magnitude and sign indicating the phase difference. Indirectly and nonlinearly, that also indicates the frequency difference between the input frequency and the center frequency of the filter.

Anything we can do to bring the dc signal back to zero will necessarily mean that the input frequency and the filter's resonant frequency are the same again. We could do this by changing the input frequency (a phase-locked loop) or, as in the present case, by altering the constant that tunes the filter (a tracking filter). In either case, of course, we would use the signal "dc" to effect the changes.

My filter consists of two integrators in a negative feedback loop (a very old method I didn't invent). A pure integrator with a sine-wave input will produce an output lagging by exactly 90 degrees -- a cosine wave. Note that mathematically, the integral of a sine function is a cosine function. This is how I get a signal shifted by 90 degrees from the output of the filter: it is simply the input to the second integrator. I apply the damping in the first integrator (altering its phase shift a little), so we really have a true 90 degree phase shift in the second one.

The signal "dc" is really the integral of the product of the two sine-waves. As long as the phase difference is not exactly 90 degrees, "dc" will keep increasing or decreasing. When the phase difference returns to 90 degrees, "dc" will stop changing and simply hold its value. As it happens, added phase-shifts that occur while frequencies are changing work to stabilize feedback loops using "dc" as an output signal. Lucky me.

This filter, with the integrated value of "dc" determining its center frequency, simply tunes itself to the input wave. The output of the filter is thus in phase with the input, larger than the input because of resonance. So why not just use an amplifier and be done with it?

Because the output of the filter is not the output of this device. The integrated signal "dc" is the output. This signal does not fluctuate at the input frequency. It is simply a steady signal. The magnitude of this signal is related to the frequency of the input wave according to the way it affects the tuning of the filter. If that relationship is linear, the magnitude of "dc" is linearly proportional to the input frequency. Its

magnitude represents the abstract concept of frequency, quantified. It is the analog of frequency without itself having a frequency. The only reason for constructing this tracking filter is to obtain the value of "dc," not to do anything with the waves coming out of the filter. It is the signal "dc" that would be passed on to higher systems (or a sonic spectrograph), indicating to them WHAT frequency is present without BEING that frequency. The higher systems no longer have to deal with frequency itself, but only with an analog of frequency.

Clearly, "dc" will change at the speed that the frequency of the input changes, not at the speed with which the magnitude of the input changes during a cycle of oscillation. This is where the trickiness I spoke of before comes in. If the input is a series of neural spikes at an audio frequency, the signal "dc" would also be a series of spikes, but at a much lower frequency. The "magnitude" of "dc", that is, its average spike rate, would be at some steady value corresponding to the input spike frequency. So we have a spike frequency in and a spike frequency out -- but they analogize entirely different things. The input spikes represent pressure peaks; the output spikes do not. In fact, single output spikes don't represent anything: only their rate of occurrance is meaningful. So even though we are still talking about the same basic physical sort of neural signal, there has been a type change in the meaning of the signal.

The mathematical relationships in my filter describe the real behavior only when there are many dt's in each cycle of input (low frequencies). As input frequency rises, the integrations begin to show their crudity; near the top frequency where there are just two samples per cycle, the relationships are way off. But this just shows up as a nonlinearity in the relationship between "dc" and the actual input frequency. You still get interpretable (by me or higher systems) spectrograms.

I've been toying in the background with some other ideas. Basically, audio inputs always occur at the fundamental frequency. If you look at them in the time domain instead of the frequency domain, you get a waveform that's not a sine wave but some squiggley mess that repeats at the fundamental frequency. A self-tuning filter will lock onto the fundamental with no trouble (I predict), but the relationship of "dc" to changes in frequency should depend on the waveshape. I think that a series of broadly-tuned filters might yield a set of "dc" signals that would show changes in relative amplitudes that depend on the waveshape. It's possible that a higher function receiving these signals might be able to construct weighted sums that would contain reduced information about wave-shape (I wish Martin Taylor weren't away). Maybe this will come to nothing, but I'm going to try it. There are lots of people around who could do this quicker and better, but I'll keep on until someone grabs it and runs.

Bill Silvert (910920) --

Bill, I can ZIP and I can ftp -- I've downloaded stuff from Washington U. successfully. How could I send my demo programs to you for posting on your mailserver? They're ZIPped self-extracting .EXE runnable files now.

You're doing a GREAT thing here for us. Sending even compressed source code can get expensive in time, but mailing disks is also expensive, every way. I'm willing to put these demos in the public domain for personal uses. How do I do this?

Best to all, after all. Bill S. cheered me up.

Bill P.

==========

Date: Sun, 22 Sep 1991 17:41:15 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: From Mary Powers

newsletter, conference reports

[from Mary Powers]

Ed Ford just called for reports from those who attended the CSG conference for inclusion in the upcoming newsletter. I think we were less explicit about this before and at the meeting than we were in previous years. Our aim is to have as many brief (1/2 page) comments from those who attended so that those who could not come can get an idea of what went on. All the following would be welcome in addition to or instead of a report of what you presented: comments on other presentations, any new ideas, questions, problems, approaches, angles you took back home with you.

Up there in the future this will be the only record of what went on, except for hours and hours of untranscibed tapes. Secure your place in the archives!

Send direct to Ed BY OCT. 1! at ATEDF@ASUVM.INRE.ASU.EDU
or 10209 N. 56th St Scottsdale AZ 85253

Ed doesn't write the newsletter, he assembles it. We write it.

For those who did not attend the meeting: the increased dues ($40 regular, $5 students) are to meet the costs of producing Closed Loop AND to accumulate a fund to assist graduate students with the cost of attending meetings.

For Hugh and others who have to plan their schedules far in advance: the next CSG meeting will be July 29-August 2, 1992, at Fort Lewis College, Durango, CO. Accomodations will be available before and after the meeting, and for guests, for those who want to enjoy a 4-Corners vacation.

==========

Date: Mon, 23 Sep 1991 09:40:41 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>

Subject: FYI: two Genetic Algorithms books

***** 16889 0
Received: from LABS-N.BBN.COM by CCB.BBN.COM ; 22 Sep 91 15:29:32 EDT
Received: from KARIBA.BBN.COM by LABS-N.BBN.COM id aa04031; 22 Sep 91 15:20 EDT
To: neural-people@BBN.COM, machine-learning@BBN.COM
Subject: Two new GA books
From: aboulang@BBN.COM
Sender: aboulang@BBN.COM
Reply-to: aboulanger@BBN.COM
Date: Sun, 22 Sep 91 15:14:12 EDT
Source-Info: From (or Sender) name not authenticated.

From: morgan@unix.SRI.COM (Morgan Kaufmann)
Newsgroups: comp.ai,comp.ai.neural-nets,comp.theory,comp.theory.self-org-sys
Subject: Publication announcement for books on genetic algorithms
Keywords: genetic algorithms, book, learning
Date: 21 Sep 91 00:28:08 GMT
Followup-To: poster
Organization: SRI International, Menlo Park, CA

PUBLICATION ANNOUNCEMENT: NEW BOOKS IN GENETIC ALGORITHMS

Morgan Kaufmann Publishers

(Ordering information follows this announcement)

FOUNDATIONS OF GENETIC ALGORITHMS
Edited by Gregory Rawlins (Indiana University)

ISBN 1-55860-170-8 $45.95 U.S. 450 pages

The metaphor underlying genetic algorithms (GAs) is natural selection. To solve learning, design, or optimization tasks, a genetic algorithm maintains a population of ``organisms'' (bit strings) that are modified probabilistically until a solution is reached. A GA is a control structure that adapts to the problem being solved through syntactic operations on bit strings and beneficial changes to parents are combined in their offspring.

Genetic algorithms are becoming an important tool in machine learning research. In many cases, GAs may provide an more advantageous approach to machine learning problems than neural networks have provided. GAs have been applied to problems such as design of semiconductor layout and factory control, and have been used in AI systems and neural networks to model processes of cognition such as language processing and induction. They are the principal heuristic search method of classifier systems and they have been used on NP-hard combinatorial optimization problems. GAs are also studied for insights into natural selection operating on natural populations.

This book is focussed on the foundations of GAs. Although much is known about their basic behavior, there are many aspects of GAs that have not been rigorously defined or studied formally. This book addresses the need for a principled approach to understanding

the foundations of genetic algorithms and classifier systems as a way of enhancing their further development and application.

Each paper presents original research and the majority of the contributions will accessible to anyone with general training in computer science or mathematics. The contributions demonstrate progress on many fronts: investigations on the theoretical limits of finding GA solutions (convergence); a new and practical parallel implementation of GAs as a general AI search strategy; a linkage of GAs to other probabilistic search techniques; studies of population dynamics (for infinite populations); coding and its effects; and understanding and anticipating trends in the GA modification process that may lead to non-optimal solutions (deception). This book is a reference for all of the basic issues in GAs and it should be of interest to a variety of fields including machine learning, neural networks, theory of computation, mathematics, and biology.

CONTENTS:

M. B. McMahon,
McDonnell Douglas Space Systems Company

A Comparative Analysis of Selection Schemes Used in Genetic Algorithms David E. Goldberg,
University of Illinois
Kalyanmoy Deb,
University of Illinois

Conditions for Implicit Parallelism
John J. Grefenstette,
Naval Research Laboratory

A Hierarchical Approach to Learning the Boolean Multiplexer Function
John R. Koza,
Stanford University

Deceptiveness and Genetic Algorithm Dynamics
Gunar E. Liepins,
Oak Ridge National Laboratory
Michael D. Vose,
University of Tennessee

Evolution in Time and Space---The Parallel Genetic Algorithm
Heinz Muhlenbein,
University of Dortmund

Spurious Correlations and Premature Convergence in Genetic Algorithms
J. David Schaffer,
Philips Laboratories
Larry J. Eshelman,
Philips Laboratories
Daniel Offutt,
Philips Laboratories

Variable Default Hierarchy Separation in a Classifier System
Robert E. Smith,
University of Alabama
David E. Goldberg,
University of Illinois

An Analysis of Multi-Point Crossover
William M. Spears,
Naval Research Laboratory
Kenneth A. De Jong,
George Mason University

A Study of Reproduction in Generational and Steady State Genetic Algorithms
Gilbert Syswerda,
Bolt, Beranek and Newman Laboratories

Quasimorphisms or Queasymorphisms? Modeling Finite Automaton Environments
T. H. Westerdale,
University of London

Fundamental Problems of Deception in Genetic Search
L. Darrell Whitley,
Colorado State University

Genetic Algorithms for Real Parameter Optimization
Alden H. Wright,
University of Montana

GENETIC ALGORITHMS: PROCEEDINGS OF THE FOURTH INTERNATIONAL CONFERENCE (1991)

Edited by Richard K. Belew (University of California, San Diego) and Lashon Booker (Mitre Corporation)

ISBN 1-55860-208-9 $36.95 U.S. 650 pages

Genetic Algorithms are a category of computer algorithms suggested by the evolutionary process of natural selection. This volume presents research contributions from the Fourth International Conference on Genetic Algorithms. Authors include leading scientists from academia and industry. Topics of particular interest addressed in this book are Holland's Genetic Algorithm (GA) and Classifier Systems, Rechenberg and Schwefel's ``Evolutionary Strategies,'' machine learning and optimization using these systems, and their relations to other learning paradigms such as connectionist networks.

The contributions are divided into eight sections: Representation and Genetic Operators; GA Techniques and Behavior; Formal Analysis of GAs; Parallel GAs; Classifier Systems and Other Rule-Based Approaches; GAs in Hybrid Methods; GA Applications; Connectionism and Artificial Life. In all, 78 papers are presented. The collected papers are also indexed by taxonomic category, keywords and author.

The work presented in this volume will be an important research tool for those interested in machine learning, artificial intelligence, neural networks, cognitive science, artificial life, biological modelling and related fields.

CONTENTS

Ordering Information:

Shipping: In the U.S. and Canada, please add $3.50 for the first book and $2.50 for each additional for surface shipping; for surface shipments to all other areas, please add $6.50 for the first book and $3.50 for each additional book. Air shipment available outside North America for $30.00 on the first book, and $25.00 on each additional book.

American Express, Master Card, Visa and personal checks drawn on US banks are accepted.

MORGAN KAUFMANN PUBLISHERS, INC.
Department E3
2929 Campus Drive, Suite 260
San Mateo, CA 94403
USA

Phone: (800) 745-7323 (in North America)
(415) 578-9928
Fax: (415) 578-0672
email: morgan@unix.sri.com

==========

Date: Mon, 23 Sep 1991 14:17:35 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: correspondence

[From: Bruce Nevin (910923 1248)]

Bill Powers (910921.0800)

Thanks for the further explication re C, I'll get back to it on the train. I hope this stuff is useful and interesting to more than just me!

I assume Peckham is saying something worthwhile because Chuck is sending me stuff of his to read, so I will see how I do with the two more photocopied excerpts from his writings that I received this weekend.

Chuck, where I am missing the point it would sure help me if you would say what you think he's driving at. And it would help to know what goal of yours you believe Peckham helps you to reach. If it's a construal of all signs as instructions, it might be more productive to put it in your own words. I have an exam re-take to prepare for and dissertation research to inch along, so spare time is not a cheap commodity for me. I can write fast and I can type fast and I try to understand fast because it's in my profession to do so, but I depend on feedback to see how I'm doing same as the rest of us and Peckham is still murky for me.

Bill, I'm not sure how seriously to take your mid-IRS fulminations, but some comments to yours anyway.

"The meaning of an utterance is the response to it" doesn't mean just the verbal response, but your comment on regress is on target anyway.

> never track a term down to its final meaning. You get word->response

> ->response->response ..... and never get to anything other than responses.
> What does meaning have to do with it? It's responses all the way down.

However, that misconstrual does lead you astray farther on:

>>That response is appropriate in the judgment of someone, hence, all
>>utterance is normative.

>B....! Excuse me. We don't go around judging the appropriateness of
>sentences, our own or anyone else's.

We do judge whether someone's response to utterance X (not necessarily *verbal* response!) is consistent with goals we had in mind when we said X. But Peckham is ignoring and oversimplifying a heck of a lot about the use of language (pragmatics). On most occasions if I say "is that the book by Peckham you have there?" and you respond by sighing heavily and picking up some IRS forms and checking the figures with a calculator I would assume you hadn't heard me because your response was "inappropriate" for my utterance. Given your email communication, however, I might construe your nonverbal response as an eloquent comment.

>MY utterances, at the moment, are HIGHLY normative, but
>that's not because my *language* is feeling normative. It's because the
>guy using it is.

When he says "all utterance is normative" instead of "all utterances are normative" I assume he means all acts of uttering are normative acts, that is, acts in which one thereby asserts normative expectation. Meaning, in abbreviatory usage so familiar that it should scarcely require comment, not that the acts are normative but that the people performing the acts are normative in the performing of them. Though Peckham still has the problem of focussing on behavioral outputs, this sort of language doesn't imply that words are agents i.e. control hierarchies.

>>A simple declaration "the book is on the table" carries with it
>>presuppositions that prescribe a universe of possible actions . . .

>I think you're bending over backward here. Presuppositions are in the
>receiver, not the transmitter (or rather, the *effective*
>presuppositions, as far as "understanding" is concerned, are).

How's this: A simple declaration "the book is on the table" carries presuppositions with it in the mind of the speaker and in the mind of the hearer. These presuppositions prescribe a universe of actions that each person regards as both possible and relevant to that declaration. Much of communication is about coming to agreement on an intersection of these two intrapersonal universes of associations and then carrying on with actions (possibly including more talk) relavent to that intersection. Needless to say (but maybe I need to say it--IRS still there) the two processes, coming to agreements and carrying on with respect to those agreements, are typically carried out in parallel, not one concluded and only then the other begun.

>Calling these "shared presuppositions" is, I think, an exaggeration. If
>we really shared presuppositions or any of the other trappings of
>subjectivity, the world would be very different.

There are many presuppositions that we share by virtue of being members of the same culture speaking the same language. There are many, many more that we do not. Neither affirmation need be taken as a denial of the other and I wish you wouldn't.

>>The intensive definition of a category amounts to a specification of
>>differences that make a difference.

>I think categories are arbitrary
>groupings. Dogs are similiar to cats in that they have tails and fleas.
>Lions are similar to cats in that neither one barks. Categories don't
>exist; we make them up. Then we attach words to them. Then we prop them
>up Out There and insist that they are real.

I made no ontological claim. We make up categories for things that have in common some characterictics that make a difference for us, that we care about with respect to some internally maintained goals. In learning to use a language a child learns the normal categories of the culture, learns to care about the same kinds of differences as others in the culture do, for the sake of similar goals which the child has learned in the process to maintain internally. The categories have no physical reality other than this internalized economy. This learning of shared categories and goals, however, is what constitutes social reality.

This is the extent of my ontological claim for categories: they do not exist "out there" as physical realities or even as clusters of traits. They exist "in here" as psychological realities because they are internally maintained control systems and their reference values. And they do exist "out there" as social realities because to some rubbery, tolerable limit of detail they must be shared by the members of a culture and the speakers of its language else there is no language and there is no culture and human intercourse as we know it comes to a halting crawl.

>What *I* say is that words and signs are indeed not
>normative, prescriptive, (although they can be used in ways intended to
>be manipulative), precisely because they do not correspond to goals held
>by one person that might come to be shared by another person if
>communication is successful, but forever remain merely words and signs.
>If a word corresponds to a goal, that goal must be that word. Likes are
>compared. If the goal is "sugar," you will say "sugar" and thus perceive
>"sugar." On the other hand, if the goal is to taste some sugar, you will
>surely not utter the word, but sprinkle some sugar on your hamburger. The
>perceptual meaning evoked by the word or sign is what matters. Isn't it?

I think this may be the nub of it. If there's something about control theory that I'm obdurately not yet getting, help me out. Here goes:

On the one hand, we have words. (Let's forget for now about nonverbal signs and the semiotic value that any behavioral output may have--can of worms! And let's even forget about syntax and intonation. Just words for now.) Hierarchical control of perceptions runs from intensities and sensations in the ear and in the effectors of the vocal tract and larynx, then transitions and configurations in parallel for segment, semisyllable, and syllable structures I would guess (why be chintzy?), morphemes (prefix, stem, suffix, etc.), and finally words. The familiar word "cat." (We trust you didn't really kick the cat!) By the mystery

of associative memory, this word evokes remembered perceptions that may run the full gamut from intensities to system and even paradigm-shift levels of perception if such there be.

A verbal hierarchy and a nonverbal hierarchy. And the only thing they have in common is the fact that associative memory links each to the other.

By associative memory, the word *corresponds* to the associated universe of remembered and imagined experiences.

What is wrong with this picture?

Over to you.

Bruce Nevin
bn@bbn.com

==========

Date: Mon, 23 Sep 1991 16:10:09 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: jbjg7967@UXA.CSO.UIUC.EDU
Subject: Pribram; verbal hierarchy

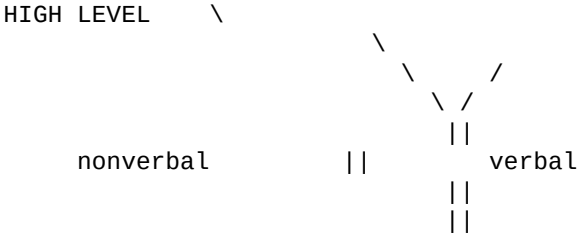
[from Joel Judd]

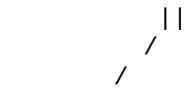
Has anyone picked up and plowed through Pribram's new (1991) new book _Brain and Perception_? Just found it on the new book shelf here.

Bruce (910923) says:

> A verbal and a non-verbal hierarchy. And the only thing they have in common is the fact that the >associative memory links each to the other.

Why two hierarchies? I know this issue comes up in Bill's books, and was also discussed once on the net. I have a problem with seperate hierarchies because I can't conceive of an entire hierarchy for language (is this different from what you mean by "verbal"?). Maybe completeness is not a pre-condition for a seperate hierarchy. But I think that at the initial levels of the hierarchy there's difficulty in saying "this intensity is a verbal one" while that one's not (unless you're the researcher who knows the environmental input is linguistic). And at the higher levels, well, for me that's a major part of the beauty of HCT--there are no verbal correlates to such things as 'family.' Sure, there are words we use when we talk about concepts with others, but the problem is that what you mean by 'peace' is not what I mean; what you mean by 'self-determined' is a little different than what I mean. My experiences differ from yours; that's one reason why we communicate. Does it make sense to visualize the relationship of language to the perceptual hierarchy thusly:





LOW LEVEL

where verbal only corresponds to most of the middle of the hierarchy; not being fundamentally different from any other perception initially, and not having a perceptual counterpart at the highest levels?

==========

Date: Mon, 23 Sep 1991 16:13:53 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: suitcase demo

[from Gary Cziko 910923]

Wayne Hershberger (910919) said replying Dag Forssell (910910)

>You might want to consider the control systems used to pilot
>radio-controlled model aircraft. They are battery powered. You
>would not need the radio, just a servo and a device to provide
>the servo's reference input. You can purchase a servo in about
>any R/C hobby shop for about $25. The servo driver you may need
>to order special.

Wayne, could you tell me what such a system would actually do?

Along these lines, my former neighbor and control systems engineer Petar Kokotovic once told me that one could get for about $100 some sort of control system using magnets that keep an object floating in space. Somehow it could control the distance of the object from the driving electromagnet by varying the stength of the driving electromagnet. I could get more information from him is someone is interested in this.--Gary

==========

Gary A. Cziko Telephone: (217) 333-4382
University of Illinois FAX: (217) 244-0538
Educational Psychology Internet: g-cziko@uiuc.edu (1st choice)
210 Education Bitnet: cziko@uiucvmd (2nd choice)
1310 South 6th Street
Champaign, Illinois 61820-6990
USA

==========

==========

Date: Tue, 24 Sep 1991 06:29:51 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: saturn.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: request for info

To: Dick Robertson
From: David Goldstein
Subject: your request
Date: 09/24/91

Dick,
Sorry, I don't have any information to give you on computer programs

which can be used by children in schools and could be used to
identify those who need some counseling attention.

I think that teachers are in a better position to act as a screening
device. I do have some references along these lines, if you are
interested.

Good luck with your project.
David
==========
Date: Tue, 24 Sep 1991 08:15:27 PDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AERO.ORG
Subject: Re: suitcase demo

[from Rick Marken (910924)]

Gary Cziko Says:

>Kokotovic once told me that one could get for about $100 some sort of
>control system using magnets that keep an object floating in space.
>Somehow it could control the distance of the object from the driving
>electromagnet by varying the stength of the driving electromagnet. I could
>get more information from him is someone is interested in this.--Gary

I'm interested. Yes, please post more info.

Kent McClellend: Thanks for the post on social control. It was indeed one
that apparently fell into the bit bucket while my computer was down. I will
try to formulate an answer to it as soon as possible.

Best Regards

Rick M.

     ****

Richard S. Marken   USMail: 10459 Holman Ave
The Aerospace Corporation   Los Angeles, CA 90024
Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)
==========
Date: Tue, 24 Sep 1991 13:00:29 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: parallel control per convention

[From: Bruce Nevin (910924 1202)]

Joel Judd (Mon, 23 Sep 1991 16:10:09 -0500)

>Why two hierarchies? . . .

>I have a problem with seperate hierarchies because I can't conceive of

>an entire hierarchy for language (is this different from what you mean
>by "verbal"?). . . .

>I think that at the initial levels of the hierarchy there's difficulty
>in saying "this intensity is a verbal one" while that one's not (unless
>you're the researcher who knows the environmental input is linguistic).

>. . . And at the higher levels, . . . My experiences differ from yours;
>that's one reason why we communicate.

Speech can be heard as just noise (just waking up or otherwise not attending to it, cocktail party babble, etc.) But it is equally clear that one can "tune in" and come to attend to it as speech or conversely tune out and cease to do so, sometimes quite abruptly, so that this experience could be just a shift of awareness to include or exclude ongoing parallel control for language. Ideas about subliminal suggestion, experiments with hypnosis, etc. support this view. But language control must not go to a very high level without awareness, since when we do tune in we can't generally recall the conversation from memory. But this may be a function of the relationship between attention and memory, and doesn't by itself disqualify the idea that parallel language control may be going on, out of awareness.

At all the levels below categories, doubtless, each elementary control system (ECS) doesn't know or care whether the inputs it is comparing to its reference signal constitute some aspect of speech or some other kind of noise and/or movement of parts of the vocal tract, air pressure, etc. In that sense, perhaps there are no special-purpose ECSs below category level.

Except that speakers of Swedish easily distinguish y and u- (barred-u with umlaut), high rounded vowels that differ in that one is closer to i and the other to u, and English speakers find them muddy and indistinguishable, and experiments with nonsense syllables and isolated vowels clearly demonstrate that this is not controlled from a higher level by recognition of words/categories. Liberman says (p. 182) that

> the only plausible hypothesis appears to be one that involves either a universal set of neural property detectors that are selectively activated as people are exposed to particular sounds during a "plastic" period, or a completely plastic process that "shapes up" property detectors for sounds used in the language. [I vote for the latter.] In short, vowels seem to be perceived by means of neural acoustic property detectors that respond to particular acoustic signals.

When exposed to synthesized syllables, people hear them as noises or as speech, reliably dependant on whether or not they were told beforehand that it was synthesized speech.

The category level, as I understand the history, was invented largely to account for what is going on in language. I have read no convincing account of perceiving catagories without words and no experimental procedure for testing for control of categories that did not rest upon control of the use of words.

Ambiguity, polysemy, and perverse arbitrariness of words indicates a many-many mapping in the control of words qua words that frequently

does not accord directly with the control of the perceptions to which the words refer. For simple examples, we control the multiple senses of homophones beat/beet, see/sea/C, etc alike below category level though they are very different above; conversely, a suppletive form like went corresponds presumably to the same nonverbal perceptions as go plus the same perceptions as correspond to the -ed of "fried" and the -t sound ending "bopped", and an "irregular" form like "saw" corresponds to the same perceptions as "see" plus those corresponding to that same d/t sound of -ed, and distinct from those corresponding to "saw" the carpenter's tool, and so on and on. The hierarchical control of perceptions involved in the understanding and use of words is organized in many ways differently from the hierarchical control of the things to which they refer and (witness translation to another language) the control of the categories, sequences, programs, principles, system concepts, etc. that we use these and other words to discuss.

>. . . And at the higher levels, . . . My experiences differ from yours;
>that's one reason why we communicate.

This refers to meanings which as I understand it concern nonverbal perceptions. But phonology, morphology, syntax, and even some aspects of semantics, are conventional, and the correspondence of linguistic expressions with nonverbal perceptions is conventional. One way we know they are conventional is that they differ systematically between all the people who use one (dialect of a) language and those who use another. We share most of those conventions, you and I, or we would not be both using English, and we would not be carrying on this email correspondence. We control for conformity with convention in these matters, concurrently with controlling for correspondence with nonverbal perceptions (that correspondence itself being conventional), when we use language.

>Does it make sense to visualize the relationship of
>language to the perceptual hierarchy thusly:

<hourglass diagram>

>where verbal only corresponds to most of the middle of the hierarchy; not
>being fundamentally different from any other perception initially, and not
>having a perceptual counterpart at the highest levels?

A relevant dichotomy is between perceptual control for conformity to convention, where the goal is a particular shape of behavioral outputs, and perceptual control not so constrained. Control of the former sort is almost always subordinate to control of the latter sort: we don't talk just to hear ourselves speak, we don't make a gesture just to shape it in a certain way, unless we are learning a second language, training in a drama class, or the like. As we might expect, bringing it to awareness tends to interfere with performance. (Suggestion maybe for your students: always drill within a speech-with-goal scenario, with just half an ear monitoring performance.)

The "verbal hierarchy" is part of this control for conformity to convention. Control for conformity to convention includes the correspondence of the "verbal hierarchy" to nonverbal perceptual control.

A phonological word "fish" corresponds to several lexical items (noun,

verb, stem of adjective or adverb "fishy," but not part of "fission"). Maybe there is a many-one mapping from nonverbal categories to spoken (or written) "fish," maybe there is a mapping from nonverbal categories to lexical items at a higher point in the verbal hierarchy. I think the latter.

I think on a nonverbal level we may have associations among category perceptions that might be represented graphically as a mesh. Then we have associations of category perceptions with lexical items or in some cases with more complex syntactic constructions (idioms, frozen expressions, cliches, etc.). There are conventional constraints on how lexical items may be linearized so that the mesh relationships may be recovered from word dependencies in a linear string. These presumably have some reflexive effect on how we organize categories in the mesh of perceptions whose control currently concerns us, as does the availability of categories that are familiar and "handy" because of their conventionalization in language. The gain on the control systems controlling for linearization and other aspects of language conventionality may vary, so we get a range from carefully written or spoken prose to passages like the one Martin quoted to us a while back that seem to carry a lot of the parallelism of mesh relations into the string of words, not settling on any single starting point for unravelling the mesh into linear speech.

Wish I could put this more clearly, but "I gotta use words when I talk to you."

Be well,

Bruce

==========

Date: Tue, 24 Sep 1991 13:36:26 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: magnetic repulsion coil

[From: Bruce Nevin (910924 1252)]

Gary Cziko 910923

>control system using magnets that keep an object floating in space.
>Somehow it could control the distance of the object from the driving
>electromagnet by varying the stength of the driving electromagnet.

When I was in high school I made a repulsion coil, essentially half a transformer armature extended some 6" beyond the coils. There were three taps, so I could increase the strength of the magnetic field by turning a control. When I placed a ring of any conductive material over the metal of the armature (sticking up) and turned on the AC power, the continuously extending and collapsing magnetic field induced AC current in the ring. The magnetic field associated with the current in the ring was always opposed to that in the main coil, so the ring floated up off the coil. With enough juice, it shot up into the air a considerable distance, a teenage boy's delight as you might imagine.

It vibrates since at each point the sine wave of AC current passes zero there is no magnetic field in either coil or ring.

Not hard to make, though the coil winding is tedious and you have to be careful not to scratch the insulation coat baked on to the coil wire.

Bruce
bn@bbn.com

==========

Date: Tue, 24 Sep 1991 14:42:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: TJ0WAH1@NIU.BITNET
Subject: servos

[from Wayne Hershberger 910924]

(Gary Cziko 910923)

>Wayne, could you tell me what such a system [the control systems used to pilot radio-controlled model aircraft] would actually do?

These radio control systems employ POSITION servos. Each servo, encased in a small box displacing less than 1 cubic inch, controls the position of a small arm extending from the case. Four servos are customarily used in model aircraft, with the output arm of each servo linked to one of the four flight controls: throttle, rudder, elevator, and ailerons.

If one disassembles one of these position servos one finds three components--surprise, surprise:

1) an effector--a geared electric motor that drives the output arm about its axis of rotation,

2) a position sensor--a potentiometer whose wiper is attached to the output arm's axle of rotation,

and

3) a circuit board comprising electronic components that receive the reference position (signal from a transmitter), compare that reference position with the sensed position, and amplify the error signal to drive the motor.

By wiring electrical meters into the circuit one can see that the servo draws very little current when its arm is idling in the reference position (e.g., 10 ma) but over 100 ma when a load tries to displace the arm.

These position servos are analogous to the kinesthetic loops in Bill's little stick man program.

Warm regards, Wayne

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

==========

Date: Tue, 24 Sep 1991 16:43:46 -0500

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

[from Gary Cziko 910924.1641]

Wayne Hershberger (910924) informed me:

> These radio control systems employ POSITION servos. Each
>servo, encased in a small box displacing less than 1 cubic inch,
>controls the position of a small arm extending from the case. . . .
By wiring electrical meters into the circuit one can see
>that the servo draws very little current when its arm is idling
>in the reference position (e.g., 10 ma) but over 100 ma when a
>load tries to displace the arm.

I get it now. Hell, electrical engineering seems easy once you understand PCT.

Now, are these servos strong enough to interact with a human? That is, could I grab hold of the arm (if only delicately with two fingers) and disturb it and feel it fighting back? For a good demo, it should have enough loop gain and "muscle" so that I can feel it resisting, but not so much so that I can't even budge the arm.

Of course, it would be nice to have it move in more than one dimension to make it seem even more alive.

Perhaps after Bill Powers and Greg Williams get their little man software finished they could make an inexpensive hardware version for physical demos.

--Gary

Gary A. Cziko Telephone: (217) 333-4382
University of Illinois FAX: (217) 244-0538
Educational Psychology Internet: g-cziko@uiuc.edu (1st choice)
210 Education Bitnet: cziko@uiucvmd (2nd choice)
1310 South 6th Street
Champaign, Illinois 61820-6990
USA

Date: Tue, 24 Sep 1991 22:24:51 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Eye Movements

[from Gary Cziko 910924.2200]

Especially for Wayne Hershberger:

I just got home from a talk given by Ted Weyand, a post-doc here in Joe Malpeli's lab on "Cortical Circuits in Eye Movements."

Using cats doing saccades in an "operant conditioning" paradigm, Weyand has

done lots of single-neuron recording of corticotectal cells which connect the frontal cortex, "association" cortex, and visual cortex with the superior colliculus. From what I understood of his talk, it seems that his findings are quite consistent with PCT, with the superior colliculus acting as a comparator.

When I asked him about this, he didn't seem to know anything about control systems. But Joe Malpeni talked to me later and said that there is a large literature on understanding eye movements using control systems. He mentioned in particular the work of David Robinson at Johns Hopkins.

Wayne, if you don't already know of Robinson's work, perhaps you should check it out. If you are familiar with, perhaps you could let us know how close he actually comes to using a control-theory model.--Gary

---

Gary A. Cziko
Educational Psychology
University of Illinois
1310 S. Sixth Street
210 Education Building
Champaign, Illinois 61820-6990
USA

Telephone: (217) 333-4382
Internet: g-cziko@uiuc.edu
Bitnet: cziko@uiucvmd

==========

Date: Wed, 25 Sep 1991 08:00:28 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: ON SKINNER AND SO ON

FILE:CSG-EM13
On Skinner

[FROM CHUCK TUCKER 910925.0800]

Dear CSG'ers,

I too find Skinner's statement (as President of the Eastern Psychological Association) "A Case History in Scientific Method" in the <<The American Psychologist>> 1956 ??:221-233 quite interesting but my use of it is somewhat different than Bill's (910914.0700-"Cumulative Record") delightful post on it [has anyone else noticed how well formulated, humorous and sharply critical Bill's posts are when the subject in Shinner's work?!?]. I use this piece to illustrate the almost accidential ways of "doing science" (Kaplan called this [after Dewey] the "logic-in-use" of scientific inquiry) as Skinner notes:

> Someday we shall be better able to express the distinction between empirical analysis and formal reconstruction, for we shall have an alternative account of the behavior of Man Thinking. Such an account will not only plausibly reconstruct what a particular scientist did in a given case, it will permit us to evaluate practices and, I believe, to teach scientific thinking. Meanwhile we can only fall back on examples (222).

My point is that all we will have are examples and anything that we do with them is a meta-reconstruction (Kaplan's "reconstructed

logic). Skinner failed to see the importance of what he we doing for science - we should not miss the point.

I am surprised that he did not find CT since he clearly recognized that rats had a purpose. He notes:

> I soon found that the constant rate at which the rat stabilized depended upon how hungry it was. Hungry rat, high rate; less hungry rat, lower rate. At that time I was bothered by the practical problem of controlling food deprivation . . . For, whenever it grows slighly hungier, it will work faster, get food faster, and become less hungry, while whenever it grows slightly less hungry, it will respond at a lower rate, get less food and grow hungier (226-227).

He missed the point that the rat will eat until no longer hungry but he did mention purpose then he lost it.

Skinner also notes his impatience with experimental design and deductive theories (227) and even statistics:

> When you have the responsibility of making absolutely sure that a given organism will engage in a given sort of behavior at a given time, you quickly grow impatient with theories of learning. Principles, hypotheses, theorems, statistical proof at the .05 level of significance that behavior at a choice point shows the effect of secondary reinforcement - nothing could be more irrelevant (228).

Then he has a choice statement:

> No one goes to the circus to see the average dog jump through a hoop significantly oftener than untrained dogs raised under the same circumstances or to see an elephant demonstrate a principle of behavior (228).

He provides several examples of studies that were done without statisitics and theory (so he claims).

> In the experimental analysis of behavior we address ourselves to a subject matter which is not only manifestly the behavior of an individual and hence accessible without the usual statistical aids but also "objective" and "actual" without recourse to deductive theory (231).

Certainly you are aware that I am selecting statements from Skinner which I find interesting but I wonder when I read such statements if the author ever read them as a directive for himself. Such as:

> Science is a continuous and often a disorderly and accidental process (232).

> The organism whose behavior is most extensively modified and most completely controlled in research of the sort I have described is the experimenter himself. . . . The subjects we study reinforce us much more effectively than we reinforce

> them. I have been telling you simply how I have been conditioned to behavior (232).

Then he uses Freud to mention the "motivation" of scientists who use "precise experimental designs and "deductive systems" noting that they are more concerned with fame than the subject matter (233). Then he describes his own "motivation" (his word) by using Frazier a character in his novel <<Walden Two>>. Frazier says that he had only one idea in his life " 'Control' expresses it. I think . . . In my early experiemental days it was a frenzied, selfish desire to dominate." Then Skinner notes that Frazier's last remark was ". . . the ony effective technique of control in unslefish." The Skinner transulates 'unselfish' to mean "positive reinforcement to ruin the whole story for me (233). Of course, I can use this to illustrate how even the most famous (perhaps especially) ignore their own evidence and cling to their theories (Kuhn's <<The Structure of scientific Revolution>> and Gleick's <<Chaos>>). I think we whould be very careful that in our excitement for our own models (PCT, HCT, CT or the like) we do not fall into the same trap. Watch out for the claim of the true model.

CODA

There are several ways to call Skinner's notions of stimulus and response and reinforcement into account and one of them is to say:

> I think it is no longer possible - at least it is no longer possible for me - to accept an immanent theory of meaning. Upon this rock I would build my heretical church, that the meaning of a term is the response to the term. One corollary of this is that the notion of immanent reference must likewise be abandoned. It is idle to say that such-and-such an expression refers to so-and so. When we make such statements, what we are doing is saying it ought so to refer, and that if the other party in the discussion does not accept that normative assertion, then either discussion must cease or the normative assertor will use all the social foces under his control, including physical force if he controls that, to make the dissenting party accept that definition or the reference of the term in question. The explanation of this is that if the meaning of a term is the response to that term, then all possible terms are capable of eliciting but a single response, and every individual term is capable of eliciting all possible responses. Now this, of course, is precisely what does not happen, and the fact that it does not happen is responsible for the illusions of immanent meaning and immanent reference. However, if those guardians of remote meta-directions - such as philosophers - would observe what they are doing, instead of just doing it, they could scarcely fail to notice that what they are busy about is the limitation of response to remote explanatory terms; that epistemology, for example, is a normative linguistic undertaking. Social interaction, then, including interaction with oneself as a social dyad, can be defined as the limitation of response (Peckham, 1976 [1973]:372-372).

Of course, one has to notice that Peckham is destroying the very foundation of behaviorism while using its vocabulary yet it is easy (and necessary) to translate the phrase "limitation of response" into "self regulation or control" once this is recognized. Both Mead and Dewey (especially Mead) continued to use the terms 'stimulus' and 'response' while rejecting the notion of immanent meaning or intrinsic behavior producing properties. This bothers his readers to this very day. But Peckham recognized Mead's contribution to science by noting:

> At least as long as fifty years ago George Herbert Mead recognized this, when he said that a scientist can be happy only when he finds his theory to be wrong. Instead of resisting the instabilty of theory - that is, of ultimate explanation - the modern scientist aims at exploiting that instability. That is why, I think, ever since the revolution in physics of seventy years ago, science has made astonishing advances. Looked at from a sufficiently distant, historically sweeping, and comprehensive point of view, human behavior has proceeded by continuously modifying it ultimate level of social management - the centers of power, and the explanatory systems which validate that power. However, that modification has always been resisted, and except for a tiny number of human beings even now is still resisted (Peckham, 1976 [1973]:375-376).

To be continued . . .

THIS IS ONLY A TEST

This life is a test. It is not a real life. Were it a real life it would have been accompanied by instructions as to where to go what to do

---

==========

Date: Wed, 25 Sep 1991 07:57:31 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: SR; Linguistic hierarchy

[From Bill Powers (910925.0700)]

Bruce Nevin (910923)

I'll have a look at the Genetic Algorithm stuff. Obviously it's not "natural" but "unnatural" selection that makes it work, the E. coli effect. Anyway, I can't resist a book that sells for 6 cents a page.

I'm over my IRS tantrum because I finally figured out a convincing way to make a balance sheet come out even on a 1022 form and the returns were mailed yesterday. It all sort of makes sense, but I wouldn't want to do it for a living.

Let's see if I can say more briefly and rationally what I think is wrong with

>These presuppositions prescribe a universe of actions that
>each person regards as both possible and relevant to that declaration.

I think that this amounts to an external observer's generalization about what is going on and not the person-centered view. In other words, it's the old method of analysis and not the model-maker's view. When you hear a sentence, do all the actions "both possible and relevant to that declaration" flash through your mind? I can see claiming that all the associations that are going to prove relevant might pop up somewhere in the brain, in or out of consciousness, but that's a matter of perception, not action. I don't think that *all possible* associations pop up -- that's a generalization across persons or time and therefore doesn't apply to *any* person at *any* time. I think we perceive the sentences of others in the context of what we're already trying to do; I think further that sentences don't call for specific actions any more than any other "stimulus" does. As you said, Peckham's generalizations are generated from the S-R point of view, and I don't believe that view to be tenable.

Chuck Tucker (many posts), how are you going to answer Bruce's challenge about self-instruction? The concept of self-instruction implies to me that we speak instructive sentences to ourselves which we then carry out as if we were responding to an external stimulus (someone else's instructions spoken to us to written on 3x5 cards). But this isn't how we tie bowknots or aim forks at our mouths or love our children or do most other sub- or supra-cognitive things. Even at the cognitive level we manipulate symbols in non-instructive ways: solving equations, describing scenes, making requests, forming sentences.

Control theory is much easier for me to live with than it is for most of you out there. I never had a commitment to any scholarly discipline in the life sciences; I never joined any clubs or formed any loyalties to someone's point of view. But each person who has come into control theory from academia did so dragging a lot of other ideas along, some of them precious and well-worked-out. I don't know of any person who is now a control theorist who doesn't still cling to some secret or not-so-secret security blanket from a former life.

I admire all of you because I can see how difficult it is to dismantle concepts with which control theory clashes; I am grateful to all of you for making such a clear effort to do so despite the pain that must come with the effort from time to time. I am also delighted at the result -- bringing into the field of control theory a deep factual familiarity with the way people work in all sorts of circumstances. The greatest weakness of my theorizing has always been my ignorance of the details; I have always known that eventually the theory would pass into the hands of others more qualified than I in practically every scientific field. This doesn't bother me -- if anything, it is part of the reward. I have been given the chance to contribute to human knowledge despite handicaps of education and temperament that but for luck would have confined me to a rather pedestrian life. That chance is only meaningful if others like those on this net and in the CSG elaborate on my simple ideas and put them into practice in the real world.

If that sounded like a farewell speech, forget it. I'm not about to shut up. I'm trying to say something rather difficult to say, which is that even some of my best friends need to look at the ideas most valuable to

them and ask if they still really make sense in terms of control theory. Maybe I'm just trying tp push a process that's already under way and will only generate resistance be getting impatient. But maybe a little disturbance of the right kind, if this is the right kind, can bring the effort needed to oppose it into awareness, and thus reveal the existence of concepts that are being protected against the inroads of PCT.

Joel Judd (910923) --

I haven't seen the new Pribram book, but will get hold of it (when I say "I will" I mean that Mary is already on the job).

Your interpretation of the relation between the verbal and behavioral hierarchies is getting closer to my view. I'm trying to do without specialized hierarchies altogether; it just offends my sense of parsimony to duplicate functions. Maybe in the end it will turn out that the areas of Broca and Wernicke are specialized for language, but I'm far from convinced of that. Has anyone tried to test for deficits in control behavior that go with the aphasias and apraxias and alexias and so on? Could there have been so much fascination with the verbal deficits that nobody thought to see what ELSE had been interfered with?

My working hypothesis is that there is just one hierarchy. It controls many kinds of perceptions, among them being those associated with language at all levels of organization. Language is just one of the things that this hierarchy can do. Perhaps at the higher levels, from categories on up, what we do IS language -- or can be looked upon as language when the object is communciation and the symbols happen to be those in the conventionalized subsets we use in speech and writing. But there is also drawing and calculating and reasoning and comprehending and generalizing and organizing to think about, activities which may or may not look like language while they're going on, but which use the same sorts of mental capabilities.

I've always hoped that linguists would help to check out this working hypothesis by seeing the operations needed to grasp language as belonging to the same types as the operations needed to do other things at these levels. If there is some capacity we need in order to solve an equation, then this same capacity ought to be evident in the process of constructing a sentence or a meaning.

Time to transmit and get the mail.

Best to all

Bill P.

==========

Date: Wed, 25 Sep 1991 14:29:17 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: FYI: more GA (from cybsys)

You may have already seen this on the cybsys distribution.

Bruce

----BEGINNING OF FORWARDED MESSAGES----
Received: from BBN.COM by CCB.BBN.COM ; 24 Sep 91 14:48:27 EDT
Received: from cunyvm.cuny.edu by BBN.COM id aa29010; 24 Sep 91 14:46 EDT
Received: from BINGVMB by CUNYVM.CUNY.EDU (IBM VM SMTP R1.2.2MX) with BSMTP id
 0437; Tue, 24 Sep 91 14:31:19 EDT
Received: by BINGVMB (Mailer R2.07) id 8634; Tue, 24 Sep 91 14:01:08 ECT
Date: Tue, 24 Sep 1991 13:51:12 EDT
Reply-To: Cybernetics and Systems <CYBSYS-L%BINGVMB.BITNET@cunyvm.cuny.edu>
Sender: Cybernetics and Systems <CYBSYS-L%BINGVMB.BITNET@cunyvm.cuny.edu>
From: CYBSYS-L Moderator
 <cybsys%bingvaxu.cc.binghamton.edu@cunyvm.cuny.edu>
Subject: Evolution Machine Software Available
To: Bruce Nevin <bn@BBN.COM>

Really-Really-From: Joachim Born <born@iir-berlin.adw.dbp.de>
Really-From: alife@cognet.ucla.edu (Artificial Life Digest)

[The following is a cross-post from Artificial Life Research List
Digest Number 062 Tuesday, September 17th 1991. You are encouraged to
subscribe by sending mail to alife-request@cognet.ucla.edu - Moderator]

Announcement
"The Evolution Machine" - v 2.1

We offer the software package "The Evolution Machine". The "Evolution Machine" presents a collection of evolutionary algorithms (Genetic Algorithms and Evolution Strategies) in a common framework.
The "Evolution Machine" includes extensive menu techniques. It runs on PC's with MS-DOS.
A detailed description of the "Evolution Machine" is given by the manual of the "Evolution Machine".The manual can be found on the FTP-server ftp.wtza-berlin.de (141.16.244.4) .The file em-man.ps.Z contains the complete manual.
In this manual, an introduction is given, the handling is fully described and the included algorithms are compared with regard to their performance.
Interested parties can order the code of the "Evolution Machine" free of charge. A request is to send to one of the authors:

Hans-Michael Voigt	Joachim Born
Email: voigt@iir-berlin.adw.dbp.de	Email: born@iir-berlin.adw.dbp.de
Tel: (00372) 674 5958	Tel: (00372) 674 2484

Address:
Institute for Informatics and Computing Techniques
Rudower Chaussee 5
D - 1199 Berlin

----END OF FORWARDED MESSAGES----
==========
Date: Wed, 25 Sep 1991 15:14:47 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: FYI: GA book list (from cybsys)

----BEGINNING OF FORWARDED MESSAGES----
Received: from BBN.COM by CCB.BBN.COM ; 25 Sep 91 14:59:28 EDT
Received: from vmd.cso.uiuc.edu by BBN.COM id aa22649; 25 Sep 91 14:58 EDT

```
Received: from VMD.CSO.UIUC.EDU by vmd.cso.uiuc.edu (IBM VM SMTP V2R1)
   with BSMTP id 5022; Wed, 25 Sep 91 13:58:59 CDT
Received: from VMC.CSO.UIUC.EDU by VMD.CSO.UIUC.EDU (Mailer R2.07) with BSMTP
 id 8176; Wed, 25 Sep 91 13:58:57 CDT
Received: from VMC.CSO.UIUC.EDU by VMC.CSO.UIUC.EDU (Mailer R2.07) with BSMTP
 id 0686; Wed, 25 Sep 91 13:58:04 CDT
Date: Wed, 25 Sep 91 13:58:04 CDT
From: Network Mailer <MAILER%VMC.CSO.UIUC.EDU@vmd.cso.uiuc.edu>
To: @vmd.cso.uiuc.edu:bnevin@ccb.bbn.com
Subject: mail delivery error

Batch SMTP transaction log follows:

220 VMC.CSO.UIUC.EDU Columbia MAILER R2.07 BSMTP service ready.
050 HELO VMD.CSO.UIUC.EDU
250 VMC.CSO.UIUC.EDU Hello VMD.CSO.UIUC.EDU
050 TICK 8172
250 8172 ... that's the ticket.
050 MAIL FROM:<@vmd.cso.uiuc.edu:bnevin@ccb.bbn.com>
250 <@vmd.cso.uiuc.edu:bnevin@ccb.bbn.com>... sender OK.
050 RCPT TO:<csg-l@uiucvmc.cso.uiuc.edu>
250 <csg-l@uiucvmc.cso.uiuc.edu>... recipient OK.
050 DATA
354 Start mail input.  End with <crlf>.<crlf>
554-Mail not delivered to some or all recipients:
554 No such local user: CSG-L
050 QUIT
221 VMC.CSO.UIUC.EDU Columbia MAILER BSMTP service done.

Original message follows:

Received: from VMD.CSO.UIUC.EDU by VMC.CSO.UIUC.EDU (Mailer R2.07) with BSMTP
 id 0684; Wed, 25 Sep 91 13:58:03 CDT
Received: from UIUCVMD by VMD.CSO.UIUC.EDU (Mailer R2.07) with BSMTP id 8172;
 Wed, 25 Sep 91 13:58:41 CDT
Received: from uxc.cso.uiuc.edu by vmd.cso.uiuc.edu (IBM VM SMTP V2R1) with TCP;
   Wed, 25 Sep 91 13:58:36 CDT
Received: from a.cs.uiuc.edu by uxc.cso.uiuc.edu with SMTP id AA02378
  (5.65c/IDA-1.4.4 for <vmc.cso.uiuc.edu!csg-l@uiuc.edu>); Wed, 25 Sep 1991
 13:57:20 -0500
Received: from harvard.UUCP by a.cs.uiuc.edu with UUCP id AA15375
  (5.64+/IDA-1.3.4 for ); Wed, 25 Sep 91 13:50:50 -0500
Message-Id: <9109251850.AA15375@a.cs.uiuc.edu>
Received: by harvard.harvard.edu (5.54/a0.25)
        (for uiucuxc!uiuc.edu!vmc.cso.uiuc.edu!csg-l) id AA13909; Wed, 25 Sep 91
 14:50:11 EDT
Received: from CCB.BBN.COM by BBN.COM id aa21686; 25 Sep 91 14:43 EDT
Date: Wed, 25 Sep 91 14:34:06 EDT
From: "Bruce E. Nevin" <bnevin@ccb.bbn.com>
Subject: FYI: GA book list (from cybsys)
To: csg-l@uiucvmc.cso.uiuc.edu
Cc: bn@ccb.bbn.com


----BEGINNING OF FORWARDED MESSAGES----
Received: from BBN.COM by CCB.BBN.COM ; 24 Sep 91 14:48:19 EDT
Received: from cunyvm.cuny.edu by BBN.COM id aa29000; 24 Sep 91 14:46 EDT
Received: from BINGVMB by CUNYVM.CUNY.EDU (IBM VM SMTP R1.2.2MX) with BSMTP id
```

0298; Tue, 24 Sep 91 14:26:26 EDT
Received: by BINGVMB (Mailer R2.07) id 8039; Tue, 24 Sep 91 13:56:21 ECT
Date: Tue, 24 Sep 1991 13:50:40 EDT
Reply-To: Cybernetics and Systems <CYBSYS-L%BINGVMB.BITNET@cunyvm.cuny.edu>
Sender: Cybernetics and Systems <CYBSYS-L%BINGVMB.BITNET@cunyvm.cuny.edu>
From: CYBSYS-L Moderator
<cybsys%bingvaxu.cc.binghamton.edu@cunyvm.cuny.edu>
Subject: ALife/GA bibliography
To: Bruce Nevin <bn@BBN.COM>

Really-Really-From: todd%galadriel@Forsythe.Stanford.EDU
Really-From: alife@cognet.ucla.edu (Artificial Life Digest)

[The following is a cross-post from Artificial Life Research List
Digest Number 062 Tuesday, September 17th 1991. You are encouraged to
subscribe by sending mail to alife-request@cognet.ucla.edu - Moderator]

By popular demand, we are now distributing our short provisional ALife/GA/
evolution/psychology/other-neat-stuff bibliography to the whole list. As
we've indicated, this bibliography is purposefully short and idiosyncratic; we
have not tried to make it complete, but rather, useful, with pointers to
important works in a variety of areas. We hope it will lead people down
interesting paths as they pursue some of these topics. We do not intend to
keep a complete and definitive *public* list of papers and books in these
areas, but perhaps someone else would like to volunteer on behalf of the ALife
mailing list.... (And we certainly welcome comments and suggestions on things
we've missed and things to include in *potential* future lists!)

enjoy--
Peter Todd
Geoffrey Miller

(this list was originally intended to fit on two sides of a page when printed
sideways in two column, 8-pt type--hence the run-overs when viewed in normal
80-column mode--but there's no guarantees it'll fit that any more....)

****

A Short, Selective, and Provisional GA, Evolution, and Artificial Life Bibliography

prepared by Geoffrey F. Miller (geoffrey@psych.stanford.edu)
and Peter M. Todd (todd@psych.stanford.edu)

References organized by topic; * = highly recommended and topical.
This list is also available by email from the addresses above.

BASIC EVOLUTIONARY THEORY

Dawkins, R. (1986). The blind watchmaker. W.W. Norton.
Dawkins, R. (1982). The extended phenotype. W.H. Freeman.
Dawkins, R. (1976). The selfish gene. Oxford U. Press.
Dupre, J. (Ed.). (1987). The latest on the best: Essays on evolution and optimality. MIT Press.
* Eldredge, N. (1989). Macroevolutionary dynamics: Species, niches, and adaptive peaks. McGraw-Hill.
Futuyama, D.J. (1986). Evolutionary biology, 2nd Edition. Sinauer Associates.
* Richards, R.J. (1987). Darwin and the emergence of evolutionary theories of mind and behavior. U. Chicago Press.
Williams, G.C. (1966). Adaptation and natural selection. Princeton U. Press.

EVOLUTIONARY COMPARATIVE PSYCHOLOGY

Alcock, J. (1989). Animal behavior: An evolutionary approach (4th Ed.). Sinauer Assocociates.
Camhi, J.M. (1984). Neuroethology. Sinauer Associates.
Driver, P.M., & D.A. Humphries. (1988). Protean behavior: The biology of unpredictability. Clarendon Press.
Ewert, J.P. (1980). Neuroethology. Springer-Verlag.
Gould, J.J. (1982). Ethology: The mechanisms and evolution of behavior. W.W. Norton.
Hoyle, G. (1984). The scope of neuroethology. Behavioral and Brain Sciences 7: 367-412.
Krebs, J.R., & Davies, N.B. (Eds.). (1984). Behavioral ecology: An evolutionary approach. Blackwell Scientific.
MacKay, D.G. (1987). The organization of perception and action. Springer-Verlag.
McFarland, D., & A. Houston (1981). Quantitative ethology: The state-space approach. Pitman Books.
Pearce, J.M. (1987). An introduction to animal cognition. Lawrence Erlbaum.
Roitblat, H.L. (1987). Introduction to comparative cognition. W.H. Freeman.
Slater, P.J.B. (1985). An introduction to ethology. Cambridge U. Press.
Stephens, D.W., & J.R. Krebs (1986). Foraging Theory. Princeton U. Press.

EVOLUTION AND MOTIVATION

Colgan, P. (1989). Animal Motivation. Chapman & Hall.
Gallistel, C.R. (1980). The organization of action. Lawrence Erlbaum.
Maes, P. (1990). How to do the right thing. Connection Science 1(3).
Tinbergen, N. (1951). The study of instinct. Oxford U. Press.
Toates, F. (1986). Motivational systems. Cambridge U. Press.
Tolman, E.C. (1932). Purposive behavior in animals and men. Appleton-Century-Crofts.

EVOLUTION AND LEARNING

Belew, R.K. (1990). Evolution, learning, and culture: Computational metaphors for adaptive search. Complex Systems 4: 11-49.
Bolles, R.C., & Beecher, M.D. (Eds.). (1988). Evolution and learning. Lawrence Erlbaum.
Davey, G. (1989). Ecological learning theory. Routledge.
Gallistel, C.R. (1991). The organization of learning. Lawrence Erlbaum.
Gardner, R.A., & B.T. Gardner (1988). Feedforward vs. feedback: An ethological alternative to the law of effect. Behavioral and Brain Sciences 11: 429-493.
* Hinton, G.E., & Nowlan, S.J. (1987). How learning can guide evolution. Complex systems 1: 495-502.
Lee, Y.C. (Ed.). (1988). Evolution, learning, and cognition. World Scientific.
Marler, P., & H.S. Terrace. (Eds.). (1984). The biology of learning. Springer-Verlag.
Maynard Smith, J. (1987). When learning guides evolution. Nature 329: 761-762.
Staddon, J.E.R. (1983). Adaptive behavior and learning. Oxford U. Press.
Todd, P. M., & Miller, G. F. (1991). Exploring adaptive agency II: Simulating the evolution of associative learning. In Meyer and Wilson (Animals to Animats).
Todd, P. M., & Miller, G. F. (1991). Exploring adaptive agency III: Simulating the evolution of habituation and sensitization. In Schwefel and Manner (PPSN).
Also see many other papers in Meyer and Wilson (Animals to Animats) and Schwefel and Manner (PPSN).

EVOLUTIONARY HUMAN PSYCHOLOGY

Cosmides, L. (1989). The logic of social exchange: Has natural selection shaped how humans reason? Cognition 31: 187-276.
Rozin, P. (1976). The evolution of intelligence and access to the cognitive unconscious. In Sprague, J.M., & Epstein, A.N. (Eds.), Progress in psychobiology and physiological psychology. Academic Press.
Shepard, R.N. (1987). Evolution of a mesh between principles of the mind and regularities of the world. In Dupre, J. (Ed.), The latest on the best: Essays on evolution and optimality. MIT Press.
Shepard, R.N. (1989). Internal representation of universal regularities: A challenge for connectionism. In Nadel, L. et al. (Eds.), Neural connections and mental computations. MIT Press.
Tooby, J., & Cosmides, L. (1990). The past explains the present: Emotional adaptations and the structure of the ancestral environment. Ethology and Sociobiology 11: 375-424.
* Tooby, J., Cosmides, L., and Barkow, J. (Eds.). (In Press). The adapted mind. Oxford U. Press.

ARTIFICIAL LIFE

Beer, R.D. (1990). Intelligence as adaptive behavior: An experiment in computational neuroethology. Academic Press.
Braitenberg, V. (1984). Vehicles. MIT Press.
Brooks, R.A. (1987). Intelligence without representations. In Proceedings of the Workshop on Foundations of Intelligence. MIT Press.
Forrest, S.J. (Ed.). (1990). Emergent computation: Self-organizing, collective, and cooperative computing networks. MIT Press.
* Langton, C.L. (1989). Artificial Life. MIT Press. [ALife I]
* Langton, C.L., J.D. Farmer, S. Rasmussen, & C. Taylor (Eds.). (1991). Artificial Life II. Addison-Wesley. [ALife II]
Simon, H.A. (1982). The sciences of the artificial. MIT Press.
Toffoli, T., & N. Margolus (1987). Cellular automata machines. MIT Press.

GENETIC ALGORITHMS

Davis, L. (Ed.). (1987). Genetic algorithms and simulated annealing. Pitman Press.
* Goldberg, D.E. (1989). Genetic algorithms in search, optimization, and machine learning. Addison-Wesley.
Grefenstette, J.J. (1985). Proceedings of the First International Conference on Genetic Algorithms. Lawrence Erlbaum.
Grefenstette, J.J. (1987). Proceedings of the Second International Conference on Genetic Algorithms. Lawrence Erlbaum.
Holland, J. (1975). Adaptation in natural and artificial systems. University of Michigan Press.
Schaffer, J.D. (1989). Proceedings of the Third International Conference on Genetic Algorithms. Morgan Kaufmann. [ICGA-3]

OTHER EVOLUTIONARY MODELING APPROACHES AND METHODS

Axelrod, R. (1984). The evolution of cooperation. Basic Books.
Fogel, L.J., A.J. Owens, & M.J. Walsh (1966). Artificial intelligence through simulated evolution. Wiley.
Kauffman, S., & S. Levin. (1987). Towards a general theory of adaptive walks on rugged landscapes. J. Theoretical Biology 128: 11-45.
Kauffman, S.A. (1990). Origins of order: Self-organization and selection in evolution. Oxford U. Press.
Lendrem, D. (1986). Modelling in behavioral ecology: An introductory text. Timber Press.
Maynard Smith, J. (1982). Evolution and the theory of games. Cambridge U.

Press.
Schwefel, H.-P., & Manner, R. (Eds.). (1991). Parallel problem solving from nature. Springer-Verlag. [PPSN]

GENETIC ALGORITHMS AND NEURAL NETWORKS

Belew, R.K., J. McInerney, & N.N. Schraudolph (1990). Evolving networks: Using the genetic algorithm with connectionist learning. CSE Tech. Rep. CS90-174, UCSD.
Chalmers, D.J. (1990). The evolution of learning: An experiment in genetic connectionism. In D.S. Touretzky et al. (Eds.), Proceedings of the 1990 Connectionist Models Summer School. Morgan Kaufmann.
Cecconi, F., & D. Parisi (1991). Evolving organisms that can reach for objects. In Meyer and Wilson (Animals to Animats).
Hancock, P.J.B., & L.S. Smith (1991). GANNET: Genetic design of a neural net for face recognition. In Schwefel and Manner (PPSN).
Harp, S.A., T. Samad, & A. Guha (1989). Towards the genetic synthesis of neural networks. In Schaffer (ICGA-3).
Heistermann, J. (1991). The application of a genetic approach as an algorithm for neural networks. In Schwefel and Manner (PPSN).
Hoffgen, K.-U., H.P. Siemon, & A. Ultsch, (1991). Genetic improvements in feedforward nets for approximating functions. In Schwefel and Manner (PPSN).
Kitano, H. (1990). Designing neural networks using genetic algorithms with graph generation system. Complex Systems 4: 461-476.
Miller, G.F., P.M. Todd, & S.U. Hedge (1989). Designing neural networks using genetic algorithms. In Shaffer (ICGA-3).
Parisi, D., F. Cecconi, & S. Nolfi (1990). ECONETS: Neural networks that learn in an environment. Network 2: 1-21.
Rudnick, M. (1990). A bibliography of the intersection of genetic search and artificial neural networks. Tech. Rep. CS/E 90-001, Oregon Graduate Institute, University of Oregon.
Stork, D.G., & R. Keesing (In press). Evolution and learning in neural networks.
In D.S. Touretzky (Ed.), Neural Information Processing Systems III.
Weiss, G. (1990). Combining neural and evolutionary learning: Aspects and approaches. Institut fur Informatik Tech. Rep., Technische Universitat Munchen.
Whitley, D.W., & T. Hanson (1989). Optimizing neural networks using faster, more accurate genetic search. In Schaffer (ICGA-3).

EVOLUTIONARY APPROACHES TO ARTIFICIAL LIFE

* Ackley, D.H., & Littman, M.L. (1991). Learning from natural selection in an artificial environment. In Langton et al. (ALife II).
Collins, R.J., & D.R. Jefferson (1991). AntFarm: A progress report. In Langton et al. (ALife II).
Collins, R.J., & D.R. Jefferson (1991). Representations for artificial organisms. In Meyer and Wilson (Animals to Animats).
Harvey, I. (1991). The artificial evolution of behavior. In Meyer and Wilson (Animals to Animats).
Jefferson, D. et al. (1991). The GeneSys System: Evolution as a theme in artificial life. In Langton et al. (ALife II).
Koza, J.R. (1991). Evolution and co-evolution of computer programs to control independently-acting agents. In Meyer and Wilson (Animals to Animats).
* Meyer, J.-A., & S.W. Wilson (Eds.). (1991). Proceedings of the First International Conference on Simulation of Adaptive Behavior: From Animals to Animats. MIT Press/Bradford Books. [Animals to Animats]
Miller, G.F. (1991). The evolution of Protean behavior strategies: An endless arms race between prediction and evasion. Unpublished manuscript, Psychology

Dept., Stanford University.
Paredis, J. (1991). The evolution of behavior: Some experiments. In Meyer and Wilson (Animals to Animats).
Wood, D. (1991). A von Neumann approach to a genotype expression in a neural animat. In Meyer and Wilson (Animals to Animats).

COMPUTATIONAL APPROACHES TO GENERATING ADAPTIVE BEHAVIOR

Agre, P.E., & Chapman, D. (1987). Pengi: An implementation of a theory of activity. In Proceedings of AAAI-87, pp. 268-272.
Booker, L.B. (1988). Classifier systems that learn internal world models. Machine Learning 3: 161-192.
Grossberg, S. (1988). Neural networks and natural intelligence. MIT Press/Bradford Books.
Holland, J., K.J. Holyoak, R.E. Nisbett, & P.R. Thagard. (1986). Induction: Processes on inference, learning, and discovery. MIT Press.
Minsky, M.C. (1986). The society of mind. Simon and Schuster.
Rumelhart, D.E., & McClelland, J.L. (1986). Parallel distributed processing. MIT Press/Bradford Books.
Sutton, R.S. (1990). Integrated architectures for learning, planning, and reacting based on approximating dynamic programming. In Machine Learning: Proceedings of the Seventh International Conference. Morgan Kaufmann.
Wilson, S.W. (1987). Classifier systems and the animat problem. Machine Learning 3(2): 199-228.

THEORETICAL ISSUES

Clark, C.W. (1991). Modelling behavioral adaptations. Behavioral and Brain Sciences 14(1): 85-117. (See also commentary by Miller and Todd.)
Cliff, D.T. (1990). Computational neuroethology: A provisional manifesto. In Meyer and Wilson (Animals to Animats).
Graubard (Ed.). (1988). The artificial intelligence debate: False starts, real foundations. MIT Press.
Harnad, S. (1990). The symbol grounding problem. Physica D 42: 335-346.
Hookway, C. (Ed.). (1984). Minds, machines, and evolution. Cambridge U. Press.
Lloyd, D. (1989). Simple Minds. MIT Press/Bradford Books.
Mangel, N., & Clark, C.W. (1988). Dynamic modeling in behavioral ecology. Princeton U. Press.
* Miller, G. F., & Todd, P. M. (1990). Exploring adaptive agency I: Theory and methods for simulating the evolution of learning. In Touretzky, D.S. et al. (Eds.), Proceedings of the 1990 Connectionist Models Summer School. Morgan Kaufmann.
Schull, J. (1990). Are species intelligent? Behavioral and Brain Sciences 13: 63-108.

SOME RELEVANT JOURNALS

Biology/Ethology: Animal Behavior, Behavior, Evolution, Journal of Mathematical Biology, Journal of Theoretical Biology, Nature.
Psychology: Behavioral and Brain Sciences, Cognition and Emotion, Cognitive Science, Ethology and Sociobiology, Evolution and Cognition, Human Nature, Journal of Comparative Psychology, Psychological Review.
Modeling etc.: Adaptive Behavior (forthcoming), Artificial Life (forthcoming), Complex Systems, Physica D, Neural Computation, Connection Science, Neural Networks.

----END OF FORWARDED MESSAGES----

----END OF FORWARDED MESSAGES----
==========
Date: Wed, 25 Sep 1991 15:53:05 PDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AERO.ORG
Subject: book list (from cybsys)

[From Rick Marken (910925)]

Bruce Nevin -- thanks for forwarding the artificial life booklist.
I noticed they didn't include any PCT books. I guess they aren't
interested in how real life works.

Hasta Luego

Rick M

     ****

Richard S. Marken   USMail: 10459 Holman Ave
The Aerospace Corporation   Los Angeles, CA 90024
Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)
==========
Date: Thu, 26 Sep 1991 07:06:59 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: ALife and RLife

Rick,

I guess they don't know any different. Someone ought to tell them
about modelling living control systems. Someone who is closer to
their work than I am. (Wouldn't take much to be closer than I am.)

        Hasty bagles

        Bruce
        bn@bbn.com
==========
Date: Thu, 26 Sep 1991 07:57:49 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: another opportunity to make CT better known

Here is another opportunity to demonstrate a better way.

If everybody gets the cybsys distribution or if there are other objections
I'll stop doing this when opportunistic postings like this come up.

        Bruce
        bn@bbn.com

***** 4308 0
Received: from BBN.COM by CCB.BBN.COM ; 26 Sep 91 01:17:14 EDT
Received: from cunyvm.cuny.edu by BBN.COM id aa11258; 26 Sep 91 1:16 EDT
Received: from BINGVMB by CUNYVM.CUNY.EDU (IBM VM SMTP R1.2.2MX) with BSMTP id
1986; Thu, 26 Sep 91 01:15:47 EDT
Received: by BINGVMB (Mailer R2.07) id 6611; Thu, 26 Sep 91 01:14:41 ECT
Date: Thu, 26 Sep 1991 01:07:29 EDT
Reply-To: Cybernetics and Systems <CYBSYS-L%BINGVMB.BITNET@cunyvm.cuny.edu>
Sender: Cybernetics and Systems <CYBSYS-L%BINGVMB.BITNET@cunyvm.cuny.edu>
From: CYBSYS-L Moderator
<cybsys%bingvaxu.cc.binghamton.edu@cunyvm.cuny.edu>
Subject: Submission to CYBSYS-L
To: Bruce Nevin <bn@BBN.COM>

Really-From: UIN005@DDOHRZ11
Date: 25 September 1991, 10:45:07 SET

Here is the Call for Papers for PPSN 92 at Brussels
converted by dvi2tty from a LaTeX file.
Yours,
Hans-Paul Schwefel

Call for Papers

PPSN 92

Parallel Problem Solving from Nature

Free University of Brussels, Belgium

28-30 September 1992

The unifying theme of the PPSN-conference is "natural computation", i.e. the de*
*sign, the theoretical and
empirical understanding, and the comparison of algorithms gleaned from nature a*
*s well as their application
to real-world problems in science, technology, etc. Characteristic for natural *
*computation is the metaphorical
use of concepts, principles, and mechanisms explaining natural systems. Example*
*s are genetic algorithms,
evolution strategies, algorithms based on neural networks, immune networks, and*
* so on. A first focus of
the conference is on problem solving in general, and learning and adaptiveness *
*in particular. Since natural
systems usually operate in a massively parallel way, a second focus is on paral*
*lel algorithms and their
implementations.

The conference scope includes but is not limited to the following topics:

- Physical metaphors such as simulated annealing,

  o Biological metaphors such as evolution strategies, genetic algorithms, imm*
 *une networks, classifier sys-
    tems and neural networks insofar problem solving, learning and adaptabilit*
 *y are concerned, and
  o Transfer of other natural metaphors to artificial problem solving.

Objectives of this conference are 1) to bring together scientists and practitio*
 *ners working with these algo-
rithms, 2) to discuss theoretical and empirical results, 3) to compare these al*
 *gorithms, 4) to discuss various
implementations on different parallel computer architectures, 5) to discuss app*
 *lications in science, technology,
administration, etc., and 6) to summarize the state of the art.

For practical reasons, there will be both oral and poster presentations. The wa*
 *y of presentation of a paper
does not say anything about its quality.

Conference Chair: B. Manderick (VUB, Belgium) and H. Bersini (ULB, Belg*
 *ium)

Conference Address: PPSN - p/a D. Roggen - Dienst WEIN - Vrije Universite*
 *it Brussel -
Pleinlaan 2, B-1050 Brussels, Belgium - tel. +32/2/64*
 *1.35.75 -
fax +32/2/641.28.70 - email ppsn@arti.vub.ac.be

Organizing Committee: D. Keymeulen, D. Roggen, P. Spiessens, J. Toreele (al*
 *l VUB)

Program Co-chairpersons: Y. Davidor (Israel) and H.-P. Schwefel (Germany)

Program Committee:

E.M.L. Aarts (The Netherlands)	R.K. Belew (USA)	K.A. de Jong (USA)
J. Decuyper (Belgium)	M. Dorigo (Italy)	W. Ebeling (Germany)
D.E. Goldberg (USA)	M. Gorges-Schleuter (Germany)	J.J. Grefenstette (USA)
A.W.J. Kolen (The Netherlands)	R. M"anner (Germany)	J.-A. Meyer (France)
H. M"uhlenbein (Germany)	F. Varela (France)	H.-M. Voigt (Germany)

Important Dates:
April 1, 1992: Submission of papers (four copies) not exceeding 5000 words to *
 *be sent to the conference
address.
May 15, 1992: Notification of acceptance or rejection.
June 15, 1992: Camera ready revised versions due.
Sept. 28-30, 1992:PPSN-Conference.

The proceedings will be published by Elsevier Publishing Company and will be av*
 *ailable at the time of the
conference.
==========
Date: Thu, 26 Sep 1991 06:58:21 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>

Subject: Language; servos; Skinner

[From Bill Powers (910925.1600)]

Bruce Nevin (910924) --

>Speech can be heard as just noise (just waking up or otherwise not
>attending to it, cocktail party babble, etc.) But it is equally clear
>that one can "tune in" and come to attend to it as speech or conversely
>tune out and cease to do so, sometimes quite abruptly, so that this
>experience could be just a shift of awareness to include or exclude
>ongoing parallel control for language.

The same things are true of other sounds: the clarinets in a symphony, the rush of water in the woods with wind blowing in the trees, the clank that tells you you forgot to take the toolbox out of the back of the car, the rumble that tells you you're too close to the right edge of the road, the pop-fizz that's someone opening a beer in the kitchen, and so on. The same perceptual system that recognizes words by their properties recognizes many other things as well in the domain of sound. Similarly with the visual system -- and even more obviously. Letters and printed words are just one kind of configuration we can see; depending on your occupation, even a minor kind.

>The category level, as I understand the history, was invented largely to
>account for what is going on in language.

This is true. But a word is not a category: it is a pointer to a category. The category itself is a mode of perception.

>I have read no convincing account of perceiving catagories without
>words and no experimental procedure for testing for control of
>categories that did not rest upon control of the use of words.

One kind of example of nonverbal categories can be found in what psychologists have called "transfer" or "stimulus generalization." If you learn to like grapefruit for breakfast, you will accept quite a range of colors, sizes, and tastes as satisfying a taste for grapefruit. If there weren't this capacity to accept different instances of the same taste-sight constellation as being "the same thing," you wouldn't be satisfied until you had exactly duplicated the particular grapefruit you ate before. there are great advantages in categorizing, as well as intellectual traps.

Young children can be very fussy about what they eat -- if it isn't exactly the same as before, in the same glass with the same straw, it isn't chocolate milk. At some point we learn to say "what's the difference?" and accept substitutes. I think that's when we're developing the category level. We begin to perceive in terms of equivalences, which is just another way of saying categories.

>Ambiguity, polysemy, and perverse arbitrariness of words indicates a
>many-many mapping in the control of words qua words that frequently does
>not accord directly with the control of the perceptions to which the
>words refer.

I think you can find parallels. All perceptions, after all, are ambiguous, in that many different combinations of lower-level perceptions

yield the same value of the same higher-level perception, and a given set of lower-level perceptions can give rise to different higher-level perceptions. Look at all the things you consider wearing on your feet under the name of "shoes." Conversely, what you call shoes might not satisfy the maitre de, or even you when you go for a job interview. The same situation can lead to different values of the same perception: the Einstein illusion when the train next to yours starts to move. Think of three-way light switches. Think of driving on the wrong side of the road in England, or trying tighten a wheel-nut on a left-hand threaded stud. Pulling on a door that opens by pushing. Mistaking a coat for yours at a restaurant. Visual metaphors in art. Jokes in music.

Today a magpie was joking with Mary and me. We had been out measuring an easement from the description in our deed, standing around and chattering about it. Suddenly there was this bird going squeek-squawk twerp peep haw haw whistle honk in the tree right above us. I think it was making fun of us. I said hello and it kept coming closer saying haw haw and all sorts of complicated things right back and bouncing up and down. Whatever the message was, it was funny as hell. Something was going on between this brain and the other bird-brain but it wasn't in a linguistic system. We were each using the equipment we had to make some kind of contact. As Mary pointed out, my equipment can be used for talking and eating at the same time, whether it's impolite or not, so it can't be the equipment that makes the difference. Or it can be used to talk to birds. I'll swear it almost said hello. It is probably swearing that I almost said haw haw peep honk.

I think your ideas (in talking about Harris earlier) about plasticity are getting close to a good picture of what's going on. We can organize to control many kinds of perceptions for many purposes. If we wake up in a world where everyone is making mouth noises, we can reorganize to build consistent perceptions on them, and so on up the hierarchy. If we had awakened in a deaf family, we would learn to read gestures and expression in the same way, and build consistent perceptions on that basis. Maybe no two people get organized in exactly or even approximately the same way. There may be many alternative inner organizations that will look operationally the same to another person. Maybe the name of the game is just -- controlling perceptions.

re: magnetic levitation. The ring on the solenoid can't be balanced in space (without touching the sides) without a control system: there's no stable equilibrium. Some sort of external confinement is necessary, or some special shaping of the magnets, or something watching the result and adjusting the fields accordingly.

Wayne Hershberger (910924) --

I've sent for the catalogue. I had always thought those servos were just up-center-down or on-off. There should be all kinds of neat demonstrations we can come up with using a pre-packaged position servo as the core device. You could use two of them to play the rubber-band game in one dimension. Maybe you could make a balsa-wood jointed arm. More toys!

Chuck Tucker (910925) --

You just about have me convinced that Peckham wasn't really an S-R behaviorist. A very informative post. You may yet shame me out of picking

on poor old Skinner. He did what he could do starting from where he started, and sometimes he had his head screwed on right. I guess what I have always resented about behaviorists is that they were fighting a battle with somebody else and kept putting me on the wrong opposing side with nothing-buttisms. If I could just accept that Skinner's battle wasn't against internal direction per se, but only against intervening variables and statistical excuses, I would probably see him differently. I will never like the verbal tricks he played, though. And I don't like his caricature of what science is. Keep at it; I may yet have to let go of this prejudice.

==========

Date: Thu, 26 Sep 1991 12:36:13 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: give dense me some help, Chuck

[From: Bruce Nevin (910926 0704)]

CHUCK TUCKER 910925.0800

I enjoyed the "logic-in-use" treatment of Skinner. Pragmatic old Charles Sanders "purse" Peirce argued for mathematicians to report how they really arrived at their conclusions instead of presenting shiny-bright pyramids of logic in their papers. And Ben Franklin said (quoting from memory): "how wonderful it is to be a rational being, for we can make up reasons for whatever conclusions we have decided upon beforehand." It is my experience that we mostly use our rational powers for rationalization.

In your coda quoting Peckham I confess I do not understand him and I ask you to explain.

What does he mean by an "immanent theory of meaning" and "immanent reference"? You identify it with intrinsic behavior-producing properties of things, in your discussion of Mead and Dewey. If Peckham means by this that words themselves do not mean anything, only the users of them do, then it seems to me he has got halfway to where he wants to go.

He says the meaning of a term is the response to the term. On first glance this seems to be a claim that behavioral outputs (responses) correspond reliably and systematically to words acting as stimuli. But as we know, behavioral outputs correspond reliably and systematically to nothing except that they are variable means for maintaining a perception that was disturbed by some stimuli, perhaps including the words Peckham would single out.

On a CT understanding, meaning is *internal* to the perceiver of meaning. It appears that he lacks means for saying this. As his friend, perhaps you could acquaint him with control theory and then ask him if he would like to restate his claim that the meaning of a term is the response to it.

It seems to me that the meaning of a term has two aspects, internal and external. It is internal to the perceiver by the nature of the perceiver as a control system. It is external to the extent that different perceivers agree about it and have come to agreement not by

nonce conversational process but as a given of the society in which they live and a prerequisite for communication.

It seems to me that the conventional assignment of meaning to a term is learned mostly inductively (perceiver generalizes from multiple experiences) and only in small part by explicit instruction as to definitions. One sort of generalization from multiple experiences is the understanding of meaning from verbal context--viz my clear understanding of the verb to misle, ability to fill in gaps due to noise or omission, etc. This is not entirely the imagining of situations in which the other words apply, as Bill has argued (but that position seems to be opening up), since syntax constrains the kind of word that might be missing, and distributional regularities can enable a machine with no memories of perceptions to do some of this even now.

Peckham says "It is idle to say that such-and-such an expression refers to so-and-so." I agree that instead a person refers to so-and-so and uses the expression as means for doing so. Peckham goes on

> When we make such statements, what we are doing is saying it ought so to refer, and that if the other party in the discussion does not accept that normative assertion, then either discussion must cease or the normative assertor will use all the social foces under his control, including physical force if he controls that, to make the dissenting party accept that definition or the reference of the term in question.

If person A and I are to communicate about some X, we must agree on means for talking about X. Person A uses expression E to refer to X. I use expression E' to refer to X. In some cases we may even continue to use the different terms respectively with no hinderance to communication if we regard them as synonyms. But indeed if we each regard them as synonyms in this context, we are likely each to use them interchangeably. If one or both of us regards them as having different meaning, then we differ in the expectations we might have about X, and this difference is very likely to surface in explicit other words about X. Then the difference is not about definitions of E and E" but about descriptions of X, and we might handle such differences in many various ways. I do not at all understand Peckham's precipitous leap to coercion. He must have some unstated motivation for painting a picture of the world in which coercion is the way in which people reach agreements.

Peckham has said that a statement like "the word keyboard refers to this device and others like it" is a normative statement that it ought so to refer, with an "if you don't like it, lump it or leave!" threat attached. He goes on to explain just why "this" is so. I put "this" in quotes because its antecedent is unclear. Does he mean that the statement is normative? That it entails a threat? Both? His "explanation":

> The explanation of this is that if the meaning of a term is the response to that term, then all possible terms are capable of eliciting but a single response, and every individual term is capable of eliciting all possible responses.

The explanation needs explaining! Unless this abbreviates some prior discussion that the reader is supposed to remember and understand by

reference, this seems to poor me no more than gobbledegook. Then to confuse me even more, he says that this many-one mapping from the universe of all possible responses to each single term and the maximal ambiguity of each single term mapping onto said universe of responses in fact does not happen:

> this, of course, is precisely what does not happen, and the fact that it does not happen is responsible for the illusions of immanent meaning and immanent reference.

Yet in his "explanation" he just claimed this was a consequence of his premise (meaning = responses). I am at a loss to know what he is claiming.

He goes on to inveigh against philosophers and other "guardians of remote meta-directions"--people who articulate the system concepts in a society, I assume--for requiring that people conversing with them about system concepts do so in terms of system concepts. This he calls "the limitation of response," presumably coercive, and says that it is the defining characteristic of social interaction.

It seems to me that verbal response in conversation, including things like exchanges of scholarly papers in journals, is constrained by the need to talk about the same subject matter or else change the subject. A change of topic constitutes a non-sequitur if it is not done properly. "Properly" concerns agreement that discussion of the first topic is ended or interrupted, expectations about taking it up again, agreements as to action dependent on the outcome of discussion, etc. Discussion of a given topic is characterized linguistically by word-sharing among the arguments of operators across periods of the discourse. Repeated words and words classed as equivalent by virtue of being in the environment of repeated words or word classes become the descriptive terms of a grammar of the discourse and a sublanguage grammar for the topic or subject matter. These restrictions are entered into partly in the nature of using language to talk in an extended way about a restricted domain of perceptions (i.e. the restriction is in agreement about a universe of discourse) and partly in the interests of carrying out the social interaction called conversation, whose conventions differ from one one language and one subculture to another. These restrictions are taken on voluntarily and typically in the self-interest of the individual, with no coercion, and may be abandoned without sanction beyond perhaps disconcerting other conversants, occasionaing hurt feelings, and the like. If people are in institutional roles defined in terms of power and control, and a person in a dominant role chooses to exploit this at the expense of others in subordinate roles, that is a distinct pathology, and not inherent in social interactions. Indeed, if coercion is the cultural norm for advising people of norms and persuading them to adhere to them, that too may be seen as a distinct pathology of a particular culture, on a spectrum of cultures such as that suggested by Ruth Benedict in a paper I quoted quite a while back.

You, Chuck, have equated Peckham's "limitation of response" with self regulation or control. In the above context, I can understand socialized limitation of individual response (what a mouthful!) as that particular aspect of self control that concerns conforming to social expectations for the sake of coordinating with the actions of others.

Somebody, I can't remember who but can look it up when I get home,

studied ideology and distinguished psychological characteristics of left, right, and center. On the left, direction and character are seen as coming from within the person, and education is leading inner strength out to performance in the etymological sense of the word (ex- "out" ducere "lead"). On the right, direction and character must be imposed from without on the unformed or even depraved individual, and education is that coercive imposition. His characterization of center made so little impression on me that I don't remember it. But it is my observation that people who grew up in right-wing childhood environments where fear and coercion predominated almost always continue through life in like manner. They have great difficulty entertaining alternative system concepts and principles, and can do so only as part of the replacement of one externalized source of authority by another. They tend to be rigid-minded in Rokeach's sense (_The Open and Closed Mind_, 1965?), that is, unable to entertain an alternative belief-disbelief system even for the nonce. They may not be closed-minded, that is, unable to entertain alternative beliefs within an established belief-disbelief system, but they tend to be. How to communicate with people stuck in this way is a challenge. It may be this challenge that preoccupies Peckham.

Got to quit. Another book due to print tomorrow. Bill, some responses to your latest, which I appreciate, tomorrow.

Bruce Nevin
bn@bbn.com

==========

Date: Thu, 26 Sep 1991 22:16:00 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Dag Forssell <0004742580@MCIMAIL.COM>

Subject: Servo, "Cling to theories", Peer Review, Language, Charts.

[from Dag Forssell 910926]

(Wayne Hershberger 910919, 910924)

Thanks for info on servos. I had in fact looked up and visited a hobby store. Concluded that these servos were stepping motors, (counting pulses) and not suitable. Based on your post, I sent for catalog anyway. The thread I started (proud father) gives me hope that these ready made, inexpensive and certainly compact servos may be suitable after all. I hope the apparent pulses are a feature of the radio transmission logic, and that they can be eliminated in a close coupling with joystick. Battery charger may serve as DC power supply, with or without NI-CAD's to smooth DC current and provide peak power requirements. Another DC power supply may be available. I see no need for battery operation per se. This is exciting, but will take me several months to pursue. There are higher priorities. This is a good example of the power of the net.

(Chuck Tucker 910925.0800)

>I can use this to illustrate how even the most famous (perhaps
>especially) ignore their own evidence and cling to their theories

>(Kuhn's <<The Structure of scientific Revolution>> and Gleick's
><<Chaos>>). I think we should be very careful that in our
>excitement for our own models (PCT, HCT, CT or the like) we do not
>fall into the same trap. Watch out for the claim of the true
>model.

In my first reading of this, I thought that you said that Kuhn clings to his theories, and wanted you to explain. On second reading, I see that you emphasize Kuhn's insight. Agree! In Durango, I expressed my admiration for Karl Popper. Here, I found a balanced discussion - argument between Kuhn and Popper - of truth. Kuhn makes it very clear that we can never view anything except through our paradigm. His objection to Popper (if I understood the brief comment right) was that Poppers requirement for falsifiability is too stringent. You cant make rigorously falsifiable statements since you can only see the world through your present paradigm. Have read This book as well as Kuhn's earlier: "The Copernican Revolution" (1957) and find much useful information here to illustrate the kind of paradigm shift we advocate.

Thanks for snailmail with handouts!

(Linguistics thread)

My new subscription to TECHNOLOGY REVIEW just arrived. (October 1991). Two articles of interest:

1) Peer Review: Treacherous Servant, Disastrous Master.
2) Language Busters. (Proof of universal grammar!).

I have snail mailed copies to Bill today.

(Kent McClelland 910925, direct)

Thanks for note. Will look at your durango draft comments closer. I have written Runkel to buy his book. Looking for Byte articles. Can you please send me the whole thing, not just diagrams. (Your note can be read either way, just trying to play it safe). I have delayed work on my charts to incorporate chapter on paradigms and work on my marketing approach. It strengthens my confidence. You are on my mailing list in a few weeks.

Dag Forssell
23903 Via Flamenco
Valencia, Ca 91355-2808
Phone (805) 254-1195   Fax (805) 254-7956
Internet: 0004742580@MCIMAIL.COM

==========

Date: Fri, 27 Sep 1991 07:56:40 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Meaning; Peckham; servos

[From Bill Powers (910927.0800)]

Bruce Nevin (910926) --
>If person A and I are to communicate about some X, we must agree on
>means for talking about X.

This is the problem I'm having: before we can agree to communicate about X, we must both be aware of some X to communicate about. Putting it this way makes X appear to have an objective existence which we need only note, preparatory to attempting to communicate about it.

But X is an *experience* for each of us and physically exists inside us. This X, I presume, is, for each of us, some function of an extant reality in which we're both embedded -- yet the function is not necessarily the same for both of us. For each of us, X is a *version* of a shared reality, a projection of that reality through our individual perceptual functions (each with unique visual, auditory, etc. properties) onto the screen of inner perception. Whatever it is that we're attempting to communicate about, it exists in the world of inner perception first, and only hypothetically in the world between us.

We need terminology that makes it easier to distinguish between the two implied meanings of X: X' the common reality, or X the apparent reality that comes to us through some unknown set of transformations, some of which may be common to human beings and the rest of which are ideosyncratic.

When you say

>It seems to me that the meaning of a term has two aspects, internal and
>external. It is internal to the perceiver by the nature of the
>perceiver as a control system. It is external to the extent that
>different perceivers agree about it

... you are giving equal standing to the internal meaning, which is directly experienced, and about which we cannot be mistaken, and the external meaning, which is hypothetical for both of us and very difficult to test for similarity. As I keep saying, when people reach agreement, it is hard to be sure that they have in fact agreed: that their individual ideas of what has been agreed upon are the same. Even extended operational testing leaves room for differences of which neither party is aware. The more complex the notion to be agreed upon, the easier it is for each party to have quite a different idea of the "common" concept, yet be unable to discover this fact through questioning and observation of the other's behavior. We can make sense of the same collection of lower-order perceptions (including perceptions of actions and their effects) in many ways; these ways can be equivalent in one context but not in another. How many times, in your progress toward understanding control theory, have you been sure you understood some point, only to have some new context arise in which you realized that your understanding was now inapplicable in some way?

In the Peckham passage you cite:

> The explanation of this is that if the meaning of a term is the response to that term, then all possible terms are capable of eliciting but a single response, and every individual term

is capable of eliciting all possible responses.

.. my impression is that he is trying to show what is *wrong* with the SR interpretation, by reduction ad absurdum. Also in the paragraph

... if the other party in the
discussion does not accept that normative assertion, then
either discussion must cease or the normative assertor will
use all the social foces under his control, including physical
force if he controls that, to make the dissenting party accept
that definition or the reference of the term in question.

... is this, itself, a normative statement? Or is Peckham just reporting how people seem to behave? If Peckham is *recommending* this mode of reaching agreement, I am of course convinced that he is wrong. If he is simply reporting a consequence of treating assertions as normative, I agree: this is the inevitable outcome. Either conversation ceases or conflict results (given resistance to being told what to think).

Dag Forssell (910926) --

>Kuhn makes it very clear that we can never view anything
>except through our paradigm. His objection to Popper (if I
>understood the brief comment right) was that Poppers requirement
>for falsifiability is too stringent.

Popper's requirement has been widely misinterpreted; I've seen people claiming that statistical data is best, because the instances that don't fit the hypothesis prove that the hypothesis is falsifiable. Popper could have avoided this sort of distortion of his idea if he had just said "testable" instead of "falsifiable." I suppose he wanted to emphasize that tests must be of such a nature that they can be failed -- the hypothesis must be used to generated predicted behavior which can then be compared with real behavior, to reveal differences, if any, between what the hypothesis implies and what actually happens. If he had put his thesis in terms of model-building, it probably would have been even clearer -- but I don't know if Popper thought in terms of models and simulations. The hypothetico-deductive method is really model building.

Yes, if those little servos can produce enough output force they will certainly make suitcase demonstrators much easier to achieve. But let's not give up on finding components for designing our own.

Best to all

Bill P.

==========

Date: Fri, 27 Sep 1991 08:28:31 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Re: give dense me some help, Chuck
In-Reply-To: Message of Thu,
26 Sep 1991 12:36:13 EDT from <bnevin@CCB.BBN.COM>

Dear Bruce,

Please read my post this morning "some answers to a question"; it may help. My dictionary report that immanent id another word for "living , remaining, or operating within; inherent" and "in theology, present throughout the universe: said of God". That is the definition that Peckham would use for the term.

I will do better later

Regards, Chuck

==========

Date: Fri, 27 Sep 1991 08:11:02 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: SOME ANSWERS TO A QUESTION

FILE: CSG-EM14

TOPIC: some answers to a question

[FROM CHUCK TUCKER 910927.0800]

Below are several preliminary and quite inadequate answers to a question posed by Bill Powers (910925.0700),i.e.,"Chuck Tucker (many posts), how are you going to answer Bruce's challenge about self-instruction?"

I

I doubt that I can answer this question since Bruce admits that he does not comprehend the materials from Peckham that I have asked him to read and you (Bill) have responded to Bruce's interpretation of Peckham's statements as "Bulls---". Until the Enlightenment meta-directions are deleted from a person's program and replaced with those from a Romantic or Constructionist set of meta-directions there is little hope that understanding, let alone comprehension, will be possible.

II

All I can tell you is that this very difficult to comprehend my point of view because it is a model which is based on the ideas of Charles Sanders Pierce, Arthur F. Bentley, J. R. Kantor, Gregor Bateson, John Dewey, George Herbert Mead, Herbert Blumer, Morse Peckham and Robert L. Stewart. Perhaps it is best that you begin by reading a great deal, and perhaps all, of the works of these men and we can talk some more about these issues and questions.

III

I don't believe I can answer the question since the issue is not understood by him or you (Bill). Ignoring Bruce's posts, you (Bill) write "If the meaning of such a statement is IN the response of other to it, why bother to explain what the statement means, or justify it? (Powers 910921.0800) [emphasis supplied]" You are correct but notice that you transformed Bruce's statement of Peckham's from "meaning is response" to "meaning is IN the response" and those little words 'in the' are where you apparently

get the notion that this is an S-R formulation (I suspect Bruce does also but not sure yet). How about trying this: MEANING AS RESPONSE, does that create enough "error" so you see the difference. Read Peckham's statement in my 910925 post and I hope you will get the point.

IV

I don't believe I can answer the question for you (Bill) since you have altered my statements so I won't even claim them. You (Bill) write: ""The concept of self-instruction implies to me the we speak instructive sentences to ourselves which we then carry out as if we were responding to an external stimulus (someone else's instructions spoken to us to (sic) written on 3X5 cards).(910925.0700)" I would never and have never claimed that "spoken statements to another" or "statements written on 5x8 cards" are EXTERNAL STIMULI when another uses them for instructing him/herself because that is empirically impossible by your own model and mine since we both follow the directions we construct from James and Dewey. Statements can't be used unless and until read or heard and the activities of reading and/or hearing transform "whatever" for the user. Details which deal with incipient beginnings of an act are crucial to my model; don't cut the loop!

V

I don't believe that I can answer the question for you (Bill) since you already have the answer. You write: "But this isn't how we tie bowknots or aim forks at our mouths or love our children or do most other sub- or supra-cognitive things. (910924.0700)" I would at least hope would say "I" instead of "we" since when I tie a bowknot I give the bowtie to my wife and say: "Will you please tie this for me, I can't do it." She has never failed to tie the bowtie for me when I have requested her to do so but interestingly as she ties it she verbally tells me what she is doing as if I might do it sometime for myself. I don't listen very carefully since I don't plan to ever tie my own bowtie. Here we have details again. If CT theory is only applicable to what people already know how to do habitually rather than to how people program themselves then the model has no advantage FOR ME over the simplier, well understood, common sense yet wrong S-R model. I know people who have worked hard to program themselves to tie bowties, learn how to use a fork, and love their children who have to re-tell themselves how to carry out such acts but still don't do them well some of the time. There is nothing that leads to failure like success.

VI

I have already given the answer in the assertive form that is used so frequently found on this NET; I wrote in my post of 910920.0800: "People guide their actions by directions they give themselves." I yet to read an adequate (to me) critique to that statement except one which is simply a counter-assertion (e.g., Powers 910925.0700) that "we" don't do it. If one would like some systematic evidence supporting that statement I refer you to the article "Purposive Collective Action" written by McPhail and

Tucker in the "Control Theory" issue of <<The American Behavioral Scientist>> or to McPhail's <<The Myth of the Madding Crowd>>.

VII

I can not possibly answer any questions about self instructions since I can not tell myself to answer such a question. Some people perhaps have answers to such questions that they can produce habitually just by reading a question and "unloading" the answer for that question by typing on their keyboard without hesitation. I think that it is wonderful to be so well programmed. Is not a question to be answered to be treated as a self-instruction to obtain an answer? Try answering that question without treating it as a self-instruction. I know of no study done with human beings (even those done with programs written by control theorists) that does not involve instructions given to be used by someone. If you know of one, please send my the citation!

VIII

It is extremely difficult to answer the question when someone informs you that may have difficulties understanding their point of view or model since you may have "... formed loyalities to someone's point of view." or you are "... dragging a lot of other ideas along, some of them precious and well-worked-out." or "... cling to some secret or not-so-secret security blanket from a former life." and then in the next paragraph expresses gratitude for having ".. been given the chance to contribute to human knowledge despite handicaps of education and temperament that but for luck would have confined me to a rather pedestrian life."(Powers 910925.0700) What do I do with that when my belief is that the model that is being put forward simply adds some interesting details and some, hopefully convincing, rhetoric to the model that I was using before I even became aware of it and further I find, occasionally, epistemological statements which for me contradict the model yet are unrecognized as such by its author. What I do is to ignore such difficulties and go on because my higher purpose or goal or meta-direction is: "I have a deep respect for everyone that I have come to know in the Control Systems Group and I believe that I can learn more from my interaction with them than anyone that I know." I even use that meta-direction to override (yes, I do this too) instructions from my dear friend and colleague to terminate all interaction. The lessons are that you give yourself the last instruction but you may not follow it and there does not have to be agreement to perfrom even highly coordinated and complicated social transactions.

CODA

All of the above answers were written in the spirit of my highest purpose and should not be interpreted as a criticism of anyone, especially Bill or Bruce. Someday I will formulate more adequate answers to the question but of course you will be the judge of that for yourself.

To be continued . . .

Best regards,

Chuck

==========

Date: Fri, 27 Sep 1991 10:37:04 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: distinct hierarchies of control

[From: Bruce Nevin (9109)]

Bill Powers (910925.0700)

I said:

>>These presuppositions prescribe a universe of actions that
>>each person regards as both possible and relevant to that declaration.

What I was trying to get at is, just as you said, that

>all the associations that [might] prove relevant might pop up somewhere
>in the brain, in or out of consciousness

This universe of associations includes remembered and imagined perceptions of actions, both ones own and others'.

>I don't think that *all possible* associations pop up --

Nor do I, that was the intent behind the clause about the person regarding them as relevant. The problem with my formulating it this way is that it entails or implies that the person is assessing relevance and winnowing out some associations. Rather, I think that associative memory constitutes "relevance," that something is "relevant" by the mere fact of it popping up by association. In any case, no generalization across persons or time was intended, beyond that implicit in the person's remembering or imagining other persons and times.

>I'm trying to do without specialized hierarchies altogether

Are you saying that an elementary control system for the sequence b-o-o-k (or the phonological equivalent thereof) is concurrently used for other perceptions? That the program that says "where you would say go + -ed, say went instead and where someone says went understand go + -ed" is concurrently used for something like tying your shoes? Help me to understand the disturbance that you're resisting here. I don't think anyone is claiming that language requires levels of perceptual control that are not required for nonverbal perceptions. Is that the rub?

Bill Powers (910925.1600)

>a word is not a category: it is a pointer to a
>category. The category itself is a mode of perception.

I didn't think I had said anything to contradict this.

As I understand it, a word is a perception at least at the sequence level and for morphologically complex words the program level must be

involved. So you have hierarchical control to the sequence or program level, such that all the elementary control systems involved in recognizing or producing a particular word are just concerned with aspects of language constituting that word. (I'll talk about the connection to nonverbal perceptions in a minute.)

This control hierarchy (just the elementary hierarchical control systems involved in linguistic aspects of controlling some particular single word) includes categories in several parallel hierarchies (features, segments, semisyllables, syllables, stress group or prosodic word, etc.) Those are all category perceptions (and sequences thereof) constituting the word, with a control hierarchy down to intensity under each category perception.

Then you have something (associative memory, we say) constituting the "pointing" from the word to several nonverbal category perceptions (several because the word is ambiguous). On the nonverbal side you also have hierarchical control up to the category level for each of these category perceptions.

I believe that none of the individual elementary control systems involved in the verbal hierarchy for the word are also concurrently involved in perception of the word, except perhaps intensity level if the word refers to a category whose perception includes a sound aspect, and possibly sensation level. Thus the verbal and nonverbal hierarchies, viewed as links from level to level through identified elementary control systems, are distinct.

Question to you: are these "specialized hierarchies" of the sort that you are trying to avoid? Or did you mean something else by that phrase? If so, what did you mean by it?

>>Ambiguity, polysemy, and perverse arbitrariness of words indicates a
>>many-many mapping in the control of words qua words that frequently does
>>not accord directly with the control of the perceptions to which the
>>words refer.

>I think you can find parallels. All perceptions, after all, are
>ambiguous, in that many different combinations of lower-level perceptions
>yield the same value of the same higher-level perception, and a given set
>of lower-level perceptions can give rise to different higher-level
>perceptions. Look at all the things you consider wearing on your feet
>under the name of "shoes."

Yes, I believe that everything from at least category level up is ambiguous (Peckham's "subsumption with neglect").

I am not claiming that the many-many mapping in control of words is unique to language. I am claiming that where there is a particular many-many mapping in the language realm, there typically is not a *corresponding* many-many mapping in the nonverbal realm to which we refer when we use the words, and conversely.

> There are many-many mappings (ambiguities, degeneracies) in the
> hierarchical control of all perceptual inputs, verbal and nonverbal.
>
> Associative memory links perceptions of words (verbal) with other
> remembered and imagined perceptions (both verbal and nonverbal).

These associative links do not map the ambiguities of one (did he say beet or beat?) onto the ambiguities of the other (is that juice or blood?). Words are not iconic.

Thus, the control hierarchies for words (defining "hierarchy" as above) necessarily are distinct from those for other perceptions.

This corresponds to the claim that the associative links from words to their referents (which are perceptions too), as indeed the control of words and their syntax, are learned as arbitrary, language-specific, conventional, social facts.

The learning of social facts like the conventions of language involves nothing more than perceptual control in a social setting where the learner has an internal goal (perhaps intrinsic) of creating and maintaining interpersonal relationships. (Autism looks to me like a lapse of this reference value.) No change to Control Theory is required to accomodate this.

There are questions about the intermediate levels of the hierarchy, but what else is new--I can't imagine it is the possibility of change here that you are resisting, and anyhow I don't know enough to propose any such changes.

Bill Powers (910925.0700)

I wasn't aware of issuing Chuck a "challenge about self-instruction," I was just trying to understand what the heck Peckham is saying. I still don't know what they mean by signs being instructions. Once in a knowledge representation course I was given the exercise of specifying detailed knowledge a robot would need to do what I do getting up and going to work in the morning, in the manner of Roger Schank's frames. I took it to be a reductio ad absurdam of the enterprise. With CT I now can say more explicitly why.

==========

Date: Fri, 27 Sep 1991 08:26:53 PDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AERO.ORG
Subject: APS Symposium

[From Rick Marken (910927)]

Boy, work has really gotten in the way of posting to the net. I do want to thank those of you who are able to do so for providing some nice reading for me each morning.

I received a call for proposals for the American Psychological Society meeting which will be held in San Diego from June 20 - 22, 1992. The APS is the breakaway "scientifically" oriented group from the American Psycological Association. I believe that Tom Bourbon and Bill Williams gave poster sessions at one of the APS meetings. The journal of the APS (Psychological Science) had the good judgment to publish one of my papers. So, I was wondering, would anyone out there like to do a sympsium with me at the meeting? I know it's expensive and all but maybe some of you big time academics (like Tom Bourbon, Wayne Hershberger, Gary Cziko, et al) could get some bucks from your institutions to go to beautiful, downtown

San Diego. Actually, I have a place where we could stay down there for free if you can get the air fare and conference registration.

I know that APS is just a month before the CSG meeting. I just thought that APS might provide a nice forum for sharing our crazy ideas with a wider audience.

If you can do it, please let me know ASAP (before the end of October) so I can prepare the proposal (for a symposium). I need at at least two volunteers in order to qualify for a symposium.

Hasty Bagels?

Rick M

****

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

==========

Date: Fri, 27 Sep 1991 11:38:45 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: Teach. Responsibility & Dr. Corey reply

from Ed Ford (910927.1140)

For those therapists and others who might be interested, I have been trying to develop a system of counseling based primarily on perceptual control theory or, to put it another way, a system for teaching others how to effectively control for what they want, to achieve satisfaction through effective use of their own living control system. I have just revised my counseling card (3X5 inches) from which I teaching counseling techniques in my counseling classes at Arizona State Univ. as well as in workshops I give at schools, corrections, and elsewhere. If my card is sufficiently practical, my students can take this card with them and use it as a guide when they are working with others. I've even used it in the business or work place arena.

Although my counseling origins are reality therapy based, I think I have gone far beyond that. I've tried to create a card that would be used if you were viewing a person as a control theorist might do, viewing your client as a "living control system." I see the three highest levels as the primary areas with which a person must deal, since I believe this is where most conflicts reside. System Concepts are values or beliefs; principles would be standards, criteria, rules, etc.; and program level is where people would make choices. The method involves teaching others how to deal with their own world through asking them questions. You get them to explore their world of wants and perceptions. Next, compare or evaluate these worlds. Third, get them to make a commitment or set a reference signal to work at reducing the error. Finally, teach them to create a plan effective enough to reduce the error. My job as counselor/therapist/manager is to teach

them how to think this all through, and how to make a plan involving the most efficient and effective feedback system for reducing error. The following is how the card looks. If any of you want some of these 3X5 cards, send me your address and I'll send you some.

TEACHING RESPONSIBILITY
based on perceptual control theory
by Edward E. Ford

Responsibility - the willingness and ability of people to follow standards and rules and ultimately to set their own, without infringing on the rights of others.

To Access Them - The more they perceive you as someone they trust and who cares, the more effective will be your ability to work with them.

ASK - KEEP QUESTIONING THEM - don't tell them what you think; repeat your question if they don't answer it; ignore excuses & don't ask why; be non-judgemental; be specific & stay focused; ask what they want that's causing how they feel.

1. Exploration - Wants

WHAT DO YOU WANT? WHAT ARE YOUR PRIORITIES?

- Perceptions

WHAT ARE YOU DOING? WHAT ARE YOUR STANDARDS?

WHAT IS THE RULE? WHAT ARE YOU TRYING TO CHANGE?

WHAT IS INTERFERING WITH WHAT YOU WANT TO CHANGE?

2. Evaluation - Comparing wants and perceptions

IS WHAT YOU'RE DOING GETTING YOU WHAT YOU WANT?

IS IT POSSIBLE TO GET OR CHANGE WHAT YOU WANT?

IS WHAT YOU'RE DOING AGAINST THE RULES OR AGAINST YOUR BELIEFS, VALUES, PRIORITIES, OR STANDARDS?

3. Commitment -

DO YOU WANT TO WORK AT RESOLVING YOUR PROBLEM?

4. Plan -

TEACH THEM HOW TO CREATE A PLAN

a. establish specific area of needed improvement (keep plan small to assure success)
b. set a measurable goal for needed feedback
c. have them think through then explain in detail how they're going to achieve their measurable goal
d. set up a chart which shows progress in time increments (hourly, daily, or weekly, etc.)

I recently posted a letter (910913) that I sent to Dr. Gerald Corey who
is the author of Theory and Practice of Counseling and Psychotherapy.
In the book, I quoted him as saying "Although the ideas of control
theory are not original with Glasser, most of the recent work on this
new theory and how it can be applied to systems is based on his
observations, which are summarized in his 1985, Control Theory."
I then gave him a lengthy explanation on the Control Systems Group and
what we are all about.

I just received a handwritten reply in which he said that "you have a
good point and I do want to be accurate. If you have a suggestion for
how I might re-word or re-phrase the comment..., I'd appreciate that."
He closed by thanking me for sending him the info on our group.

I plan to answer him but would first appreicate any input regarding how
a new phrasing might be worded.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, Arizona 85253 Ph.602 991-4860
==========

Date: Fri, 27 Sep 1991 12:46:43 PDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AERO.ORG
Subject: Dr. Corey reply

[From Rick Marken (910927b)]

Ed Ford (910927.1140) asks:

>I recently posted a letter (910913) that I sent to Dr. Gerald Corey who
>is the author of Theory and Practice of Counseling and Psychotherapy.
>In the book, I quoted him as saying "Although the ideas of control
>theory are not original with Glasser, most of the recent work on this
>new theory and how it can be applied to systems is based on his
>observations, which are summarized in his 1985, Control Theory."

>I plan to answer him but would first appreicate any input regarding how
>a new phrasing might be worded.

Here's my proposed rewording:

"Although the control theory model of living systems was developed by
William T. Powers, Glasser claims it as his own although he doesn't
understand it. Most of the recent work on this new theory (that is not
just arm waving and misconception) has been done by Powers and his
colleagues (Madman Marken, Badboy Bourbon, Ragman Runkel,
Hotblood Hershberger, Fastfingers Ford, Tzar Cziko, Wildman Williams, etc)."

Regards

Rick M.

****

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

==========

Date: Fri, 27 Sep 1991 15:07:46 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: jbjg7967@UXA.CSO.UIUC.EDU
Subject: msu paper

[from Joel Judd]

This is a draft of a paper to be given to a SLA audience next week. Before the CT veterans have a heart attack over the glowing generalities presented here, remember I was given twenty minutes to tell how CT relates to SLA. Any suggestions on how to get the point across more forcefully and quickly through other examples/demos would be appreciated (as would corrections of inaccuracies). Sorry two calculations are missing; they are both abysmally low, I just left my calculator at home.
(we'll see if x-attachments works)

(This file must be converted with BinHex 4.0)

<Garbage Deleted> 9/17/99 MSA

==========

Date: Sat, 28 Sep 1991 08:25:14 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Misc: possible resend

[From Bill Powers (910925.1600)]

I found this apparently unsent. Sending it again[?].

Bruce Nevin (910924) --

>Speech can be heard as just noise (just waking up or otherwise not
>attending to it, cocktail party babble, etc.) But it is equally clear
>that one can "tune in" and come to attend to it as speech or conversely
>tune out and cease to do so, sometimes quite abruptly, so that this
>experience could be just a shift of awareness to include or exclude
>ongoing parallel control for language.

The same things are true of other sounds: the clarinets in a symphony, the rush of water in the woods with wind blowing in the trees, the clank that tells you you forgot to take the toolbox out of the back of the car, the rumble that tells you you're too close to the right edge of the road, the pop-fizz that's someone opening a beer in the kitchen, and so on. The same perceptual system that recognizes words by their properties recognizes many other things as well in the domain of sound. Similarly with the visual system -- and even more obviously. Letters and printed words are just one kind of configuration we can see; depending on your occupation, even a minor kind.

>The category level, as I understand the history, was invented largely to

>account for what is going on in language.

This is true. But a word is not a category: it is a pointer to a category. The category itself is a mode of perception.

>I have read no convincing account of perceiving catagories without
>words and no experimental procedure for testing for control of
>categories that did not rest upon control of the use of words.

One kind of example of nonverbal categories can be found in what psychologists have called "transfer" or "stimulus generalization." If you learn to like grapefruit for breakfast, you will accept quite a range of colors, sizes, and tastes as satisfying a taste for grapefruit. If there weren't this capacity to accept different instances of the same taste-sight constellation as being "the same thing," you wouldn't be satisfied until you had exactly duplicated the particular grapefruit you ate before. there are great advantages in categorizing, as well as intellectual traps.

Young children can be very fussy about what they eat -- if it isn't exactly the same as before, in the same glass with the same straw, it isn't chocolate milk. At some point we learn to say "what's the difference?" and accept substitutes. I think that's when we're developing the category level. We begin to perceive in terms of equivalences, which is just another way of saying categories.

>Ambiguity, polysemy, and perverse arbitrariness of words indicates a
>many-many mapping in the control of words qua words that frequently does
>not accord directly with the control of the perceptions to which the
>words refer.

I think you can find parallels. All perceptions, after all, are ambiguous, in that many different combinations of lower-level perceptions yield the same value of the same higher-level perception, and a given set of lower-level perceptions can give rise to different higher-level perceptions. Look at all the things you consider wearing on your feet under the name of "shoes." Conversely, what you call shoes might not satisfy the maitre de, or even you when you go for a job interview. The same situation can lead to different values of the same perception: the Einstein illusion when the train next to yours starts to move. Think of three-way light switches. Think of driving on the wrong side of the road in England, or trying tighten a wheel-nut on a left-hand threaded stud. Pulling on a door that opens by pushing. Mistaking a coat for yours at a restaurant. Visual metaphors in art. Jokes in music.

Today a magpie was joking with Mary and me. We had been out measuring an easement from the description in our deed, standing around and chattering about it. Suddenly there was this bird going squeek-squawk twerp peep haw haw whistle honk in the tree right above us. I think it was making fun of us. I said hello and it kept coming closer saying haw haw and all sorts of complicated things right back and bouncing up and down. Whatever the message was, it was funny as hell. Something was going on between this brain and the other bird-brain but it wasn't in a linguistic system. We were each using the equipment we had to make some kind of contact. As Mary pointed out, my equipment can be used for talking and eating at the same time, whether it's impolite or not, so it can't be the equipment that makes the difference. Or it can be used to talk to birds. I'll swear it almost said hello. It is probably swearing that I almost said haw haw

peep honk.

I think your ideas (in talking about Harris earlier) about plasticity are getting close to a good picture of what's going on. We can organize to control many kinds of perceptions for many purposes. If we wake up in a world where everyone is making mouth noises, we can reorganize to build consistent perceptions on them, and so on up the hierarchy. If we had awakened in a deaf family, we would learn to read gestures and expression in the same way, and build consistent perceptions on that basis. Maybe no two people get organized in exactly or even approximately the same way. There may be many alternative inner organizations that will look operationally the same to another person. Maybe the name of the game is just -- controlling perceptions.

re: magnetic levitation. The ring on the solenoid can't be balanced in space (without touching the sides) without a control system: there's no stable equilibrium. Some sort of external confinement is necessary, or some special shaping of the magnets, or something watching the result and adjusting the fields accordingly.

Wayne Hershberger (910924) --

I've sent for the catalogue. I had always thought those servos were just up-center-down or on-off. There should be all kinds of neat demonstrations we can come up with using a pre-packaged position servo as the core device. You could use two of them to play the rubber-band game in one dimension. Maybe you could make a balsa-wood jointed arm. More toys!

Chuck Tucker (910925) --

You just about have me convinced that Peckham wasn't really an S-R behaviorist. A very informative post. You may yet shame me out of picking on poor old Skinner. He did what he could do starting from where he started, and sometimes he had his head screwed on right. I guess what I have always resented about behaviorists is that they were fighting a battle with somebody else and kept putting me on the wrong opposing side with nothing-buttisms. If I could just accept that Skinner's battle wasn't against internal direction per se, but only against intervening variables and statistical excuses, I would probably see him differently. I will never like the verbal tricks he played, though. And I don't like his caricature of what science is. Keep at it; I may yet have to let go of this prejudice.

==========

Date: Sat, 28 Sep 1991 09:53:18 PDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AERO.ORG
Subject: Carver & Scheier

[From Rick Marken (910928)]

I received a copy of a letter to Phil Runkel from a professor at the U of Iowa. He praised Runkel's book to the skies (I think correctly -- for those interested, the book is
Runkel, P. (1990) Casting nets and testing specimens. New York: Praeger.)
but he also asked a question "Why didn't Runkel mention Carver

and Scheier?" The fellow from Iowa is a social psychologist and he said that when social psychologists hear "control theory" they think of Carver & Scheier.

I presume that Phil sent me the letter so that I could tell him (Phil) how to answer this question. I have some thoughts -- but I'm not really an expert on Carver & Scheier. But I think its time we CSG types delt with this phenomenon. Here are two mainstream psychologists who seem to have gotten real excited about control theory. They even wrote a big book on it -- and they publish many articles; they seem to have a major research program going. They refer to Powers all the time; Powers PCT model is ostensibly the basis of their work. Yet, there is virtually no contact between them and any members of the CSG -- even the social psychologists (like McPhail). I don't even have a copy of their book. So what gives?

I did start reading their book (which was published in the early 1980s, I believe). I seem to recall that the introductory chapter (which described the basic control model) was reasonably competent. But think went quickly downhill (from my perspective) when they started getting into their own research -- about self image or something. It's like two different book in one. They don't seem to understand the modeling approach to science -- though they do seem to understand the control model reasonably well.

Does anyone else have any ideas on this. It's really quite a weird phenomenon. They certainly have a right to do their work and say that they are control theorists. They seem to be happy with their own research effort -- or I imagine they would have stopped by now. But why no contact with CSG? What do the other conventional psychologists think of them? Is it worth it to look at what they do and try to deal with it from a CST perspective? Is it best to just ignore it (probably the best approach)?

I remember feeling very board while reading their book. It's like reading a lot of conventional psychological research. You know it's off the mark, but it's hard to say why because you tend to frame the answer in the context of the assumptions under which the research was done. Control theory ideas often seem sort of orthogonal to what conventional psychologists are up to.

Hasta Luego

Rick M.
marken@aerospace.aero.org

==========

Date: Sat, 28 Sep 1991 14:42:47 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: jbjg7967@UXA.CSO.UIUC.EDU
Subject: missing numbers

For anyone interested, the missing calculations from the Lambert et al. (1963) criticism in my paper are .82 for the uselessness quotient and an effect size of .162 on a before and after comparison of the same group using a measure of authoritarianism. The t-test the authors performed was statistically significant ($p < .03$).

==========
Date: Sat, 28 Sep 1991 16:25:39 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: jbjg7967@UXA.CSO.UIUC.EDU
Subject: the end

Sorry to rewrite in pieces. The end of the SLA paper from the middle of the next-to-last paragraph has been revised so far to read like this, correcting some mistakes in the characterization of hierarchical functioning:

...This two-dimensional presentation of the hierarchy is not meant to imply that its functioning is uni- or even bi-dimensional, or that it is either a "bottom-up" or "top-down" model. Reference signals for each level's control loops originate in the outputs of higher levels, with the perceptual signals of each level (except the first, which can receive input from the external environment) offering perceptual input for higher levels.

One of the most important implications of such a model for understanding behavior is that by positing reference levels which look for a perceptual match, the system is not searching for the "appropriate" behavior. Instead, it is looking for the results of behavior to see if those results contribute to perceptions more closely matching the internal reference. In other words, goals are not requesting the system to perform specific behaviors--goals are checking the outcomes of any behavior to see if it satisfies the goal. As the saying goes: "There are many ways to skin a cat." Another no less important insight is that only the lowest level perceptions are received directly from the external environment. All subsequent levels of the hierarchy deal with perceptions derived from an "environment" made up of the organism itself. The symphony I hear, the novel I read, indeed the world I perceive is very much one of my own making. The implications for an understanding of language stemming just from this one fact would warrant an entire book.

By no means is this model completely understood or explicated. Some of its crucial aspects are still poorly understood--attention, for example. But it is at present the only model, based on actual neural systems, that offers an explanation for the why and how of behavior, and leads to predictions of future behavior which stem from the functioning of the model.

It is currently possible to convincingly demonstrate control at lower levels of the hierarchy; there can be little doubt regarding the existence of some type of feedback loop operating at these levels of perception. It remains to be demonstrated that these same principles hold all the way up the hierarchy, although in the field of counseling those who have integrated control theory into their work are convinced of its usefulness (Goldstein 1991; Ford 1989). The point I wish to make at this time is this. For several decades now the conception of language learning has been one of linear, cause and effect processes, with a corresponding research paradigm. If, as appears to be the case, behavior is governed by an individual's internally generated goals, if this behavior is characterized by a feedback loop in which the perceptual input is controlled, and we accept that each individual can have different goals and different ways of achieving those goals, then we are in desperate need of a different theory of behavior, and corresponding research paradigm to help us develop both a useful understanding of SLA and truly effective learning environments. I believe

that control theory offers the best current alternative.
==========
Date: Sat, 28 Sep 1991 17:09:42 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Peckham; language; APS; Glasser

[From Bill Powers (910928)] --

Chuck Tucker (910927.0800) --

You say:

> All I can tell you is that [it is] very difficult to comprehend my point of view because it is a model which is based on the ideas of [list] Perhaps it is best that you begin by reading a great deal, and perhaps all, of the works of these men and we can talk some more about these issues and questions.

No fair. If I were to do the same thing, I'd say that before you can understand me you have to learn electronics, integral calculus, analogue computing, and engineering control theory (citing authors), then read everything I have thought important *and interpret it all just the way I did,* understanding what I understood, misunderstanding what I misunderstood, adding to it the same ideas I did, filtering it all through my peculiar slants on life, and so on.

Maybe I can learn at least part of your model if you can explain to me what you mean.

Meaning as response.

Here is the passage from Peckham to which you referred, I believe:

> ... if the meaning of a term is the response to that term, then all possible terms are capable of eliciting but a single response, and every individual term is capable of eliciting all possible responses. Now this, of course, is precisely what does not happen, and the fact that it does not happen is responsible for the illusions of immanent meaning and immanent reference. However, if those guardians of remote meta-directions - such as philosophers - would observe what they are doing, instead of just doing it, they could scarcely fail to notice that what they are busy about is the limitation of response to remote explanatory terms; that epistemology, for example, is a normative linguistic undertaking. Social interaction, then, including interaction with oneself as a social dyad, can be defined as the limitation of response (Peckham, 1976

Side note: the second meaning of immanent, in my dictionary, is "Philos. (of a mental act) taking place within the mind of the subject and having no effect outside it", which seems the opposite of the theological (third) usage.

If I understand the import correctly this time (it does not appear, after

all, to be a reductio ad absurdum argument), Peckham appears to be saying that all terms potentially elicit all possible responses, and all possible terms could elicit any given response. This premise being taken as a fact, it follows that all specific actions consist of deleting unwanted responses to a given term and limiting responses so they do not follow from all possible terms.

Peckham's logic is clear: His premise is contradicted by observation ("Now this, of course, is precisely what does not happen ..."), so to explain why not all terms are observed to elicit all responses, he introduces a process he calls "limitation of response." If there were a limitation of response, this would explain why all terms do NOT elicit all responses even though basically they do.

If I instruct you "stand up," and you stand up, then in Peckham's view, as I understand it, the instruction potentially elicits all possible responses (such as falling down or shouting "Herbert!"), but your response is limited (by your internal properties?) to just the one response of standing up. The impetus to stand up comes from the instruction; it's just that the impetus to do all other possible things has been made ineffective by the limitation-of-response process, whatever it is that does the limiting.

Before I comment further on this, is my understanding of what Peckham is saying correct?

Bruce Nevin (910927) --

>This universe of associations includes remembered and imagined
>perceptions of actions, both ones own and others'.

This way of putting it, if you're implying a *social* universe and not just a universe unique to each individual, implies an observer who is privy to every individual's inner associations. This observer, then, can form a concept of a universe of associations that transcends the individual -- but that universe exists in no other individual, unless that individual is also privy to everyone's inner associations. Therefore no property of interaction that comes out of the community of associations can be made part of a model of the (non-omniscient) individual. The observer who is effectively omniscient can deduce properties of the society from knowing all the inner associations (and the rest of the individual organizations). These would be emergent properties, but they would not exist in any single person (just as a molecule has no temperature or pressure).

So "relevance" may mean "relevance to a given person," or it may mean "relevance somewhere within the society." The problem posed by the latter meaning is the same one posed by statistical knowledge: what does it tell you about the next person you meet? This is the trap in looking for universal rules for any human behavior.

>In any case, no generalization across persons or time was intended,
>beyond that implicit in the person's remembering or imagining other
>persons and times.

Accepted.

>>I'm trying to do without specialized hierarchies altogether

>Are you saying that an elementary control system for the sequence
> b-o-o-k (or the phonological equivalent thereof) is concurrently used
>for other perceptions?

Yes and no. To hear the event "book" as a word requires having a level capable of reorganizing into specialized event-perceivers. At that same level, we also see the downward travel of a ball, contact, and upward travel as a unitary bounce. This is another specialized event-perceiver. If my definitions of the levels are correct (and there's no decisive reason to think they are), the same level of processes, the same substrate of basic functions, is used in either case. There wouldn't be any need to have these two events detected in physically separate parts of the brain (although accidents of wiring might see to it that this happens). To recognize an event of any kind, there must be certain kinds of processes common to all event-recognition, whether the events pertain to words, sights, smells, or anything else. When we find out what those processes are, we will be able to construct event-perceivers -- not just linguistic event-perceivers, but any kind. And I presume that we will find the necessary components for constructing such perceivers in particular volumes of the brain.

When I say I don't want specialized hierarchies, I mean that I don't want to have one hierarchy organized in terms of transitions, configurations, events, and so on, and another one organized in terms of consonants, phonemes, vowels/morphemes, etc.. I would like to see the linguistic terms simply as examples of larger classes of perceptions that occur at those same levels. I suspect that this is really your view, too. The same processes that allow us to filter out one speaker at a cocktail party let us pick the clarinets out of the symphony, hammer on one nail in a whole row of nails, pick out the sound of a rushing stream from the sounds of a birds and a storm sweeping through the forest, and see triangles instead of hexagons in an array of dots. The underlying functions, I claim, are not specialized for language or anything else: language and the other things are examples of what a brain can do when equipped with the capacity to form perceptual functions of all these different classes, and control the results.

Once reorganization has had its way, of course, there will be specialized input functions for many recurrent things, including "book." These perceivers will each interpret all inputs in terms of the particular perception that results from the associated transform; the pandemonium model. The "book" perceiver will hear everything as "book" -- to some degree. So the "book" perceiver would not be used for any other perception. But it is simply one example of the types of perception that can come to be recognized at that level, and it's the level, not the instance of it, that I claim is general.

>As I understand it, a word is a perception at least at the sequence
>level and for morphologically complex words the program level must be
>involved.

I'd say "event", once the word has come to be heard or seen as a unitary space-time object. When we analyze perceptions we tend to bring in higher levels of consideration than we actually use. We can analyze an intensity signal into a "sequence" of impulses, by paying attention from that point

of view. But the signal doesn't *represent* sequenceness.

All that's needed is to create a unique signal when a word is heard or read; from then on, only the signal is processed by higher levels, and it has no component parts. The signal says THAT the word has occurred, but is not that word. So the event level suffices to say that a word or simple phrase has occurred.

The higher levels don't receive words; they receive signals, which are all like, saying that a particular word has been perceived at a lower level. If you could trace circuits at the higher levels, you wouldn't find any words there. We hear words in sentences because we're aware at many levels at once. When you strip away lower-level perceptions, what you get are "ideas," the sense of the meaning without the sense of configurations or events. A higher level can't tell if the category signal it gets came from a word or from the nonverbal perceptions that are also members of that category. The signal just says "a member of this category is present." It doesn't say which member. It doesn't even say which category; it's just the category that this signal, rather than that one, stands for.

[This model needs a way for a system to select which inputs it is going to use. The only way to do this now is pretty clumsy.]

> These associative links do not map the ambiguities of one (did he say >beet or beat?) onto the ambiguities of the other (is that juice or >blood?). Words are not iconic.

Neither are other perceptions. Should I call that "blood" or "ketchup?" Different things look the same, or sound the same, or smell the same, or feel the same in some respects, yet differ in their associations. In spoken language there's no difference between beet and beat. They're the same word. The difference is in the context. In a pandemonium model, ambiguity means perceiving several things at once with about equal weight.

Memory association isn't the only way to account for the interchangeability of verbal and non-verbal perceptions. I think that in general, perceptual functions are underdetermined; a given state of a perceptual signal can be created by many different combinations of inputs. A verbal perception is just a perception; it can be an input to any kind of perceptual function, even a nonverbal one. So you could have a perception built on a word and/or a set of visual or kinesthetic or tactile perceptions -- either kind could evoke the same perception. To the receiving perceptual function, the inputs are just inputs: there's no difference between a verbal input and a nonverbal one. There are only signals of various and varying magnitudes. A sequence-recognizer (as mentioned) can't tell whether a given category signal arose because of a verbal or a nonverbal member of that category being present. It gets only the category-present signal. So different perceptions can be "associated" without having anything to do with each other, save that they are inputs to the same perceptual function.

An idea. When we say "John hit Jim," we recognize this sentence as gramatically correct. When we SEE John hit Jim, we also see this act as "grammatically correct." The same sense of fit to the expected properties of perception occurs. If we say "John hit the nail on the thumb," something jars and it sounds funny; if we SEE John, in the act of hitting

the nail, hit himself on the thumb, we also get the same sense of the unexpected, with a bit of humor there. At some level it doesn't matter whether you say it or see it. It's the same experience, give or take some background details that the words leave out.

Maybe (very maybe) the role of words and symbols as verbal things begins to fade out fairly low in the hierarchy -- Joel Judd may have had this same idea a few days ago. The words create an imagined story by creating perceptions as if the real events were present. From there on up, the brain treats the perceptual signals as if they had originated in the world where they usually originate. The higher-level perceptual processes, in other words, are the same ones that would apply to any perception, not special linguistic processes. The correctness of the sentence at these higher levels is judged according to the relationships among the evoked perceptions -- do they make sense in terms of our understanding of the world?

There are modes of discourse where this bright idea wouldn't seem to apply. But maybe if we looked a little harder ...

>Thus, the control hierarchies for words (defining "hierarchy" as
>above) necessarily are distinct from those for other perceptions. This
>corresponds to the claim that the associative links from words to their
>referents (which are perceptions too), as indeed the control of words
>and their syntax, are learned as arbitrary, language-specific,
>conventional, social facts.

The verbal hierarchy, in terms of your definition, is indeed separate. That is, you could trace interpretative processes up that hierarchy, avoiding all non-verbal branches. But the perceptual functions taking place in that hierarchy are of the same types as you would find by following nonverbal branches, too -- or such is my hunch. Controlling for a particular sequence of words uses the same kinds of operations as controlling for a sequence of hand signals, or a sequence of written marks, or a sequence of bets, or a sequence of knot-tying manipulations. It requires the basic capacity to perceive in terms of sequential order, and convert errors into successive changes in lower-order reference signals.

>The learning of social facts like the conventions of language involves
>nothing more than perceptual control in a social setting where the
>learner has an internal goal (perhaps intrinsic) of creating and
>maintaining interpersonal relationships. (Autism looks to me like a
>lapse of this reference value.) No change to Control Theory is required
>to accomodate this.

Why don't we just say that we agree and get on with it? I'm feeling uncomfortably far from the level where control theory is solid. Could we start trying to think up some demonstrations that show language behavior as control of input? Even very simple ones? I'd like to feel that I can touch bottom once in a while.

Rick Marken (910927) -- APS symposium: I'll talk it over with Mary. Her sister and brother-in-law live there and we haven't seen them for a long time -- we could stay with them. This might be a very nice reason to take a trip.

Ed Ford (910927) -- Your system seems eminently teachable. As to Corey, my inclination would not be to downgrade Glasser, but just to point out that there are people from many disciplines interested in control theory who do not necessarily endorse Glasser's interpretation of it. And of course you should advise Corey not to claim that Glasser is doing "research" in this field: he is applying what he knows, in the context of Reality Therapy. That's OK with me. Rick Marken's way would be more fun, but let's leave all doors open.

==========

Date: Sat, 28 Sep 1991 17:19:36 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Bin Hex

[From bill powers (910928.1700)]

Joel Judd --

I love "the end" but I would really like to see "the beginning." This BinHex language you SLA types speak is really hard to read. Or perhaps is there a program that will run on a PC that can decipher it? If so, I ain't got it. Would appreciate a copy in real words!

Best

Bill P.

==========

Date: Sat, 28 Sep 1991 21:30:12 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: RYATES@CMSUVMB.BITNET
Subject: Re: the end
In-Reply-To: Message of Sat,
28 Sep 1991 16:25:39 -0500 from <jbjg7967@UXA.CSO.UIUC.EDU>

Joel:

   If I understand your conclusion correctly, then control theory predicts that L2 language learning should be dramatically different from individual to individual. Each has different goals and perceptions.

Are you familiar with Long (1990), The least a second language acquisition theory needs to explain. TESOL Quarterly, 24, 649-666? This should become a classic in the field. Long reviews an immense amount of research in L2 acquisition and draws 8 implications for any theory of SLA. The first implication is of interest:

> Common patterns of development in different kinds of learners under diverse conditions of exposure means that a theory that says nothing about universals in language and cognition is incomplete or, if considered complete, inadequate.

Can the application of control theory to second language acquisition predict common patterns of development across different types of learners under different types of exposure?

Bob Yates

==========

Date: Sat, 28 Sep 1991 22:06:33 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: RYATES@CMSUVMB.BITNET
Subject: Re: Peckham; language; APS; Glasser
In-Reply-To: Message of Sat,
28 Sep 1991 17:09:42 -0600 from <powersd@TRAMP.COLORADO.EDU>

Bill writes:

Controlling for a particular sequence of words uses the same kinds of operations as controlling for a sequence of hand signals, or of written marks or a sequence of bets or a sequence of knot-tying manipulations.

I don't think so. Consider two classic examples from Chomsky.

1) Flying planes can be dangerous.

(1) is ambiguous because there exist two different underlying structures. Where is there similar kinds of ambiguity in bets and knot-tying? (I want to avoid handsignals and written marks because they could be natural languages.)

It would appear that the following two sentences have exactly that same structure. Those of us taught sentence diagramming would diagram them the same.

2) John is easy to please.
3) John is eager to please.

Notice that (2) can be restated as

4) It is easy to please John.

However, (3) can not be restated as

5) It is eager to please John.

(4) and (5) show that (2) and (3) have different underlying structures. I don't know of similar cases for betting or knot-tying. Sentences are more than a surface sequence of words.

Bob Yates

==========

Date: Sun, 29 Sep 1991 12:03:13 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: psy_delprato@EMUNIX.EMICH.EDU
Subject: RE: Carver & Scheier

[FROM: Dennis Delprato]
A few views:
1. For several reasons, thinkers exhibit a strong tendency toward insularity. One way this is manifested is that they avoid views

and ways of going about things that do not neatly fit in with
their interests of the moment. This principle would apply bi-
directionally in the case of Carver & Scheier and control theorists
such as Rick Marken & Tom Bourbon who are concerned with "doing it
right." This is a tough problem because the likes of Rick and Tom
are best not diverted by all sorts of possibly related activity.

2. Sometimes it is best for productivity to not go too far too fast.
Although Carver & Scheier might in tgghe
the long run accomplish significantly more if they were to more
fully adopt control theory principles, they likely would be forced
to go a long time without grants, e.g. The result is that they are
doing something creative AND SAFE as far as their professional
situations are concerned. This point obviously cites "one of
the several reasons" alluded to in no. 1.

3. Control theory with all the bells and whistles is not applicable
to Carver & Scheier's research questions. I don't agree with this,
but someone could argue in behalf of this position.

4. Full-blown control theory is difficult to apply to questions
of interest in personality and social psychology. I agree with
this but suspect that the difficulty is more a matter of limitations
of the researchers and conventional personality & social psychology
than of control theory per se.

In general, and I suppose this goes back to no. 1 above, we do not
seem to appreciate the importance of integrative efforts. There
seems to be a tendency to associate apparent/surface uniqueness
with positively evaluated creativity. Thus, I can imagine someone
like Carver seeking to do their own thing (so it might appear)
instead of "merely" doing in personality & social what has already
been done in--ugh--motor skills. How could this be an accomplishment?

Finally, I agree with Rick in that I get bored quickly upon examining
Carver & Scheier's papers. Yet I do admire what they have accomplished.
I think they have taken some interesting and important steps. The
situation is much the same as with the feedback functions work of
some of the so-called molar behavior analysts. There's gold for
the right graduate students to mine in them thar hills. Intellectual
movement takes place v-e-r-y   s-l-o-w-l-y.

==========

Date: Sun, 29 Sep 1991 12:45:33 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Self & other instruction

[From Bill Powers (910929.1000)]

Chuck Tucker (910927.0800) --

While waiting for your reply to my post of 092928, I might as well get
another facet of the discussion into the pipeline:

> ... I wrote in my post [you said] of
> 910920.0800: "People guide their actions by directions they give
> themselves." I yet to read an adequate (to me) critique to that

> statement except one which is simply a counter-assertion (e.g., Powers 910925.0700) that "we" don't do it.

Let's compare the statement in question with the CT point of view.

First, under CT, people don't "guide their actions" unless the actions themselves are the controlled variable (as in dancing, ASL signing, gymnastics) rather than some indirect consequence of the actions (tightening a screw, answering the telephone, steering a car down the road). Either the action or its consequence can be controlled only insofar as it is a perceptual input that is recognized and represented internally as a signal, an analog of the controlled variable. Unrepresented aspects of the action or its consequence are not controlled -- i.e., are not resistant to disturbance. The only exception to that rule occurs when changes in a controlled variable necessarily and systematically entail changes in other variables: variations in elbow and wrist position in space entail variations in the position of the midpoint of the forearm. Disturbances of that midpoint necessarily disturb the positions of elbow, wrist, or both and hence will give rise to corrective action if elbow and wrist position are under control. The corrective action will stabilize the midpoint if it stabilizes elbow and wrist position.

Referring to "instructions people give themselves" might mean internal reference signals, or it might mean speaking and hearing oneself in actuality or in imagination, or it might mean physically writing/recording instructions which one then reads/replays. You say

> Statements can't be used unless and until read or heard and the activities of reading and/or hearing transform "whatever" for the user.

This narrows the choices: it suggests to me a claim that before a statement can be used it must exist as a physical input to the senses (that is what reading and/or hearing means to me). This means, under the CT model, that heard or read instructions are not themselves reference signals: they first must become perceptual signals rising through the hierarchy.

Under CT, perceptual signals do not cause actions. The first reason is that they must be compared with an inner reference in order for the system to know whether the state being perceived is too little, just right, or too much, calling for action that will increase the amount of the perception, no action, or the opposite sense of action. The second reason is that external independent disturbances can tend to alter the perceptual signal and thus create error; radical changes in action can occur without any substantial change in the perceptual signal (they prevent such changes) and without any change in the reference signal. So whatever fate befalls instructions as they are being perceived, the action that accompanies or follows them might be of any degree or direction whatsoever, depending on what external disturbances are acting at the same time. If the instruction is "Lift that suitcase onto the bed," the amount of muscle action that takes place depends almost entirely on what is in the suitcase (which the lifter does not need to know in advance). If two people hear this instruction and attempt to carry it out together, their actions may vary individually from pushing down to pulling up, depending on how the other person is acting.

Reference signals, I have postulated, may be selected from memory recordings of perceptual signals. A past state of a perception is picked out as the standard against which present-time perception is compared. The difference or error drives behavior that makes the present-time perception change (first creating it if necessary) until it matches the reference-perception, the state of the same perception at some past time. If the perceptual signal that was stored was a perception of an instruction statement, say "Lift that suitcase onto the bed," that statement now becomes the reference against which the current state of the same perception is to be compared. The current perception will match the reference signal when it is "Lift that suitcase onto the bed." The result will be that one will hear/read again the instruction that was heard/read previously. This could be accomplished by picking out the card on which the matching instruction was written and reading it again, by saying the instruction to oneself again, or by asking someone "What was that instruction you gave us this morning in the bedroom?" In any case, what will be accomplished is to perceive the instruction again -- NOT TO CARRY IT OUT.

"Carrying out" an instruction means creating in perception a situation that corresponds to the meaning of the instruction, not recreating the words of the instruction. The instruction to "lift" something must result not in experiencing the word "lift" again, but in experiencing the sensations of effort, the sense of position (visually and kinesthetically) and the sense of motion in all relevant modalities. These perceptions are not words, but are derived from sensory receptors of many kinds. In order for all of the relevant perceptions to be brought to the states corresponding to "lifting," the control systems involved must receive reference signals of the same type as the perceptions. A sense of effort must be controlled relative to a signal standing for a particular amount of effort. Kinesthetic information must be compared against kinesthetic information; visual information must be compared against visual information. In each of these channels, furthermore, reference signals of the appropriate type must appear at several levels of organization, as per the HCT model.

We are left, then, with a gap between the incoming instructions, which are perceptions of words, and the nonverbal reference signals that ultimately arise and specify the perceptual outcomes of actions. Somewhere in the brain, the words must be translated into their non-verbal meanings, those meanings being the target states of nonverbal perceptions and ultimately the states of the controlled perceptions themselves.

This translation process, as various people have conjectured on this net, may arise through memory associations at many levels. They may also arise, as I suggested just yesterday, from the fact that all neural signals are alike, so that a nonverbal interpretation can be given to signals arising from word-events or from non-verbal events, equally well. Whatever the mechanism, the words must be translated into nonverbal perceptions before they can have any significance relative to actions (other than saying words). At least we can be sure that the translation of words and sentences into workable reference signals is not a simple input-output process.

Note that this analysis requires us to talk about the meanings of words prior to reaching the output stage, and long before the outputs are turned into actions that an external observer might be able to see.

Furthermore, in most cases the external observer see the actions but must try to infer what they are intended to accomplish, in order to infer meaning. And finally, in order to infer the *correct* meaning that the recipient has given a set of instructions, the observer must use his own perceptual apparatus, and deduce the actual controlled variables (as opposed to the variables the observer had in mind when issuing the instructions).

To this point I have simply tried to analyze what must happen, according to the CT model, when instructions are given and the recipient carries out what purports to be the meaning of the instructions. This certainly can and does occur, but there is no guarantee that simply giving a person instructions will result in any action at all, much less the intended one. Obeying instructions is a choice that a person makes, and the basis of this choice is quite likely to be found in systems of higher level than the levels at which we make and follow verbal rules. My response to the instruction to lift the suitcase onto the bed could be "I'm not your servant -- do it yourself." Or I could simply ignore you and go on watching television. Instructions have no force without permission from the recipient -- without the recipient understanding and playing the game of instructor and instructee.

The fact that human beings *can* follow instructions (more or less) is no indication that following instructions is a basic mode of behavior. A person can duck an oncoming hardball without issuing any verbal self-instructions to duck: all that is required is a zero reference level for a particular visual situation. By the time one has said or thought "Look out, that ball is aimed at my head," one would have been beaned. An animal without language can do the same thing. I claim that very little of behavior actually follows from verbal instruction -- in fact, that sort of behavior is most likely to arise in a social situation in which it is understood that someone gives instruction and someone else tries to follow it.

The concept of self-instruction, of course, can be broadened to include non-verbal reference signals generated in the course of higher-level control behavior. In that case, however, there would be no way to support the generalization that reference signals must be heard or read as literal instructions. They might be so treated (given suitable machinery to translate words into non-verbal reference signals), but they are not required to be treated that way in order to explain normal behavior. The CT model offers the generalized concept of a reference signal; the idea of self-instruction, particularly when tied closely to verbalizations, does not cover most of the cases to which the idea of a reference signal applies.

The idea of self-instruction has another problem in this context, which is that under the CT model, a control system does not specify its own reference signals. It receives them from higher systems, or from genetic specifications (not in words), or from the blind variations of reorganization. The system receiving a reference signal is never the same as the system issuing the reference signal. The very concept of "self" in a hierarchical control-system model begins to show fine structure that is normally overlooked in informal discourse. Whatever issues an instruction as a reference signal is never the system that receives the instruction. So self-instruction can be taken only in a broad and vague sense; its meaning can't be taken literally. Self-instruction never in fact occurs. The method of levels, in the field of psychotherapy, is based on this

idea.

Finally. Behind the idea of instructions that are read or heard, instructions that "guide actions," is the old S-R interpretation of behavioral organization. The instructions are the input, and the actions the corresponding output. This concept makes no sense when one realizes that actions which create the same consequences over and over do not themselves necessarily repeat.

Under simple and carefully-restricted conditions, one can tell people exactly what to DO -- i.e., what actions to produce -- and the result will be a repeatable consequence. This can even be assumed to be a logical first step in teaching control. But a control system can't be acquired on this basis alone. One must then allow natural disturbances to arise (or create artificial ones), which give the SAME actions DIFFERENT outcomes. Only then can the learner realize that it is the outcome, not the action, that is the point. The learner can then discover how to VARY the actions so as to produce the SAME outcome. This can't be taught through instructions concerning actions, because the instructions would have to be infinitely variable, according to all disturbances that have occurred, are occurring, or might occur. When true control is learned, rather than simply going through the moves, there is no need to anticipate disturbances or remember past instances of disturbances. Neither is there any need to plan in advance what actions will be taken for every imagineable disturbance. In fact the actions that accomplish control are no longer important; all that matters is noting and opposing the error between the intended and actual outcomes. In most situations (other than those involving intellectual levels) there isn't even any need for conscious analysis. Learning the relationship between error and action that is necessary for control is the most natural learning there is.

Chuck, I am pleased that my disturbance has resulted in your asking for a more adequate critique of the statement "People guide their actions by directions they give themselves." I hope that this rather wordy "response" gives you some added bases for comparing CT with "the model that I was using before I even became aware of [CT]". The language in which I chose to refer to the possibility that people adapt control theory to what they knew before may have been provocative, but you know that I am your friend and admirer, and would not say anything, even speaking in general, intended to put you down. Quite the opposite. I am hoping that you as well as others may seen an opportunity to go up a level.

Best regards,

Bill P.

==========

Date: Sun, 29 Sep 1991 16:25:03 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Turning corners can be dangerous

[From Bill Powers (910929.1400)]

Bob Yates (910928) --

"Flying planes could be dangerous" is ambiguous in meaning, but it is still a sequence of words. Saying "flying planes could dangerous be" would be a violation (perhaps intentional, perhaps by mistake) of normal ordering, and could be perceived and corrected, or let stand for poetic purposes. The sequence "Flying could planes be dangerous" would need at least a couple of question marks to be acceptable for any purpose. My point is that the sequential ordering of elements is perceivable and controllable, whatever the resulting meaning.

There are more levels above perception of sequence in HCT: "programs" are certainly germane (bringing in rules and logic), and "principles" may be also, bringing in heuristics and generalizations. In other words, I agree with you that structure at higher levels is necessary to resolve ambiguities in word-sequences (or perceive that they are unresolvable -- she told Mary she had damaged her car). Higher levels of processing are probably necessary to decipher any sentence that isn't a simple denotation or that requires substitutions for place-holders.

As I understand the Chomskyian (there's a conflict! Chomskian? Chomskyan?) approach, there is deep structure of a very general sort that constrains the next level, surface structure. This is basically a top-down model, isn't it? That is, I understand this to mean that the deep structure is generated first, and out of it comes the more specific (and variable) surface structure, and out of that comes utterance.

Control theory doesn't say anything directly about what these structures should be. But it does suggest a different approach to the levels of structure. Rather than working from the top down in the "generative" or "elaborative" direction, try working upward in the "convergent" direction. We do this all the time: given a selection of words, construct a sequence from them; given the sequence, construct possible logical groupings and implications from it. This is the direction in which one must work to parse an utterance or sentence. Given the surface structure, perceive in it a more general deep structure. This works in the direction from more variability to less.

From the modeling standpoint there are tremendous advantages to working in the upward direction. The higher representation is a true function of the lower; that is, given a set of lower-level elements, the higher representation is a function of the lower-level elements, the function having only one value at a time. To get a different value from the same elements, you must apply a different function. But because of the many-to-one nature of the upward transformations, there can be many equivalent sets of the lower elements that yield the same value of a given higher-level function. In the upward direction, paraphrases are no problem at all: they are simply alternate sets of lower-level perceptions that yield the same higher-level perception. Thus the expression "9,1" is a paraphrase of "6,4" in terms of a function that takes these values as arguments and adds them (producing the same output value, 10, in either case). Working downward, you could say "Ten -- in other words, 6 + 4" or "Ten -- in other words, 9 + 1". But going the other way, downward, there is an indeterminacy -- what decides WHICH example is to be chosen? This problem doesn't arise going in the upward direction, especially not when feedback loops are involved.

The same two sets of elements would not be paraphrastic (?) with respect to a higher-level function that subtracted its first argument from the

second. In general, it is the form of the higher-level perceptual function that determines what alternative sets of input values are equivalent in the sense of producing the same higher-level signal.

Given a function, correcting an error in its value can be done by routing the error signal (as a reference signal) to the lower-level systems that provide each argument of the function -- each input to it. The sign attached to each branch of the routing just has to be appropriate to the effect of the related element on the value of the function -- mathematically, the partial derivative of the function with respect to each input element. You want negative feedback around each loop. If the changes in reference signals are incremental, then it isn't important to weight the reference signals sent to the lower systems (because it isn't important what mix of the lower elements is present as long as the error is corrected). The changes will continue until ANY set of element-values is found that will satisfy the higher reference setting. It's hard to translate this into terms of symbol-manipulation, but not, I think, impossible.

When the higher-level signal represents (I speak loosely here) the "meaning" of a set of input words, it doesn't matter which input set is chosen as long as the reference-meaning is satisfied. By the same token, if a second higher-level system is looking at the same inputs through a different function, it can also satisfy its reference-meaning if the set of inputs that might satisfy it overlaps the set that would satisfy the first system. So when two higher-order systems are controlling for meaning in a common set of lower-order words, it may often be possible to satisfy them both, by choosing suitable paraphrases on each side -- i.e., sets of lower-order words that, passed through different input functions, can satisfy two meanings at once. Suppose I want to say that (1) Mary and I are going shopping, and (2) Mary and I want to buy a new lawnmower (or at least Mary does). That's easy: Mary and I are going shopping for a lawnmower. One sentence conveys both the going shopping and the item wanted. Finding this sentence is guided by both higher-level requirements: to say where we're going and to say what we want. If only one requirement existed there would be many more ways of expressing either one. But when both have to be satisfied, the range of possible sentences that would express both meanings is greatly reduced.

Now the task is not one of generating a sentence blindly on the basis of some general higher-level command, but one of selecting words (and orderings and the rest) that, when perceived by the higher system, constitute valid instances of the higher structure. The "selecting" is the output part, the conversion of error to changes in lower-level variables. This concept may be harder to grasp than the lineal command structure at least implied by Chomsky's system, but I think it works better, eliminating the problem of ambiguity in the downward command chain. Chomsky once told me in response to an enquiry that he did NOT consider deep and surface structure to be models of brain levels, but thought of them strictly as analytic abstractions. Now perhaps we can see why: as a working model, the "generative" concept won't work. It can't handle the ambiguities.

We also can see how parallel processing comes into the picture. Many higher-level systems at once can be demanding different kinds of meaning or obedience to different rules at the same time. They can jointly manipulate reference signals at lower levels until the resulting sentence is perceived by each higher-level system as a valid instance of the kind

of perception it monitors.

These suggestions don't challenge anyone's view of what the deep structure looks like. That's really a matter for empirical study, not for prediction from control theory. Without knowing much about the details of Chomskyian sentence analysis, I have a hunch that it would be easier to work upward than downward, and that the results would be less equivocal. You can write programs to find the deep structure in given sentences (at least those in which extended nonverbal experience doesn't carry essential information). But you can't write a program that, given a deep structure, can generate the particular way someone says something.

With respect to your two sentences:

John is easy to please, and
John is eager to please.

It helps to drop down a level and just look at the bunches of words, as if, once heard, they are held in short-term memory for a while. Given the words, a person can construct various sequences from them in addition to the one actually heard. I think it's illuminating (somewhat) to do this with the "inverted" forms of the sentences, which you say shows the existence of a deeper structure:

It is easy to please John, and
It is eager to please John.

Try scattering the chunks around like this:

"It" is clearly a chunk that holds a place for a value to be filled in later. From the other chunks we can construct

is easy to please John	is eager to please John
to please John is easy	to please John is eager

In the set of chunks on the left, it's clearly "to please John" that is the best candidate for filling in "It." If we substitute "to please John" for "It", we get meaning: to please John is easy. The other possibility, "is easy to please John" doesn't give us any subject of discourse -- no meaning.

On the right, we find that neither chunk can substitute for "It". Eagerness, from our experience, is a state associated with an agent of some kind. It can't successfully modify "to please John," because we can't say that a thing called "to please John" can have the characteristic of eagerness. This is just a matter of experience, knowing what can happen and what can't. So we conclude that "It," on the right, has to refer to something beside the other chunks on the right -- something from a larger context. And with just a little searching, we find an acceptable interpretation. John is a hunter, and someone is remarking about John's Golden Retriever. It is eager to please John. You

can't leave an "it" floating about unmatched to anything. That's a program-type rule.

What I've done here is what I think we often do with sentences that we hear. We don't just perceive the given sequence of words or the given spatial associations. We retain the elements of the sentence (however we are accustomed to chunking them) and try out our own sequences to see if they suggest meanings. If the original sentence leads directly to an unambiguous interpretation we don't bother, but when there is ambiguity or apparent lack of meaning, we start playing anagrams and try to find an ordering that will make sense. This slows down our comprehension (real or imagined), of course. Reading long complex sentences full of rarer forms like gerunds and word order inversions is a task that's mentally fatiguing and conducive to the creation of invented understandings that may have nothing to do with what the author intended. Got that?

If an American hears a German say in English, "John Mary hit," the American may come up with an image of John hitting Mary instead of the image the German was trying to describe. This can only be done by resequencing the elements into a more familiar order, while still satisfying the expectation that the first noun is the subject.

One last remark out of the depths of my ignorance. When simple experience with the world determines the correctness of a sentence or usage, I don't think it is worth while to look for a linguistic rule to explain the preferences. We could say "turning corners can be dangerous" without any fear of ambiguity. The reason is not that "turning" can't be associated with an activity of a person as "flying" can, because it can: turning pancakes, turning cars. It's because we have seldom if ever experienced a corner doing the turning. Turning is not something corners do, but flying is something that airplanes do. In another universe the opposite could be true: "Watch out for that corner!" could have quite a different meaning.

The only reason for which "flying airplanes can be dangerous" is ambiguous is that we know from experience that airplanes fly and that people fly airplanes ("fly" of course is really two different words, one meaning the way an airplane moves and the other meaning a skill). These different meanings have nothing to do with linguistic rules or principles, or with language at all. Truly linguistic principles should be such that they can be illustrated by abstract symbols substituted for specific words: W1, W2 ... W999999, and so on. If a linguistic principle depends on knowing what each word means, then we should be suspicious of it, because word-meaning is a chance of experience and not a universal principle.

Nice way to spend a Sunday afternoon.

Best

Bill P.

==========

Date: Mon, 30 Sep 1991 10:43:30 PDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AERO.ORG
Subject: Carver & Scheier

[From Rick Marken (910930)]

Dennis Delprato: Nice to hear from you. It's been a long time. I appreciate your thoughts on Carver and Scheier. I agree with many of your points. It is true that people tend to become isolated from points of view that do not seem relevant to their interests. But I do believe that CSG types have gone more than the extra mile in our attempts to understand and challenge opposing points of view (reinforcement type theories, attractor models of behavior, etc etc). It is the opposing points of view, I think, that have made the least effort to try to understand the control model. This is just a general observation -- not particularly relevant to Carver/Scheier.

With respect to Carver & Scheier, you say:

> Yet I do admire what they[Carver & Scheier] have accomplished.
>I think they have taken some interesting and important steps.

Could you explain what these are? What are they doing? What have they accomplished? I'd really would like to know. I don't like to think that I am insulating myself from work done by others because it's not "politically correct". Your statements imply that this is what I am doing -- trying to maintain the "orthodox" control theory position. I think I'm just trying to understand human nature in the context of a control system model of behavioral orgaization. I'm happy to change my mind based on evidence and I'm happy to consider changes in the model (we've had many threads over the last year where we actually have changed the model--adding transport lags, for instance-- or considered it -- when Bill suggested having the model control error rather than perception). So if Carver and Scheier really are doing some worthwhile work then I think we should definitely discuss it on the net. What could be better use of this medium than to discuss research on control theory with which many of us are not familiar.

My guess, however, is that I have ignored Carver and Scheier, not because they violated some orthodox CSG ritual or because they are not working on something I find interesting but because they are doing stuff (like most conventional psychological research) which is basically useless. I bet that looking for useful tests of the control model in their research would be like trying to find such tests in one or another of the arcane studies of operant conditioning that I am sure you are familiar with. There are operant conditioning studies that are suggestive and useful for control theorists. But there are far more which, because they are based on the wrong assumptions, are just too muddled to be of use to a control theorist.

Anyway, I certainly could be wrong; maybe Carver and Scheier have done some good work. Let's forget the generalities and talk about the work itself. What controlled variables are they studying? How do they measure control? How do they track the reference state of the controlled variable? Are their results in the range of accuracy that makes sense for modeling (ie -- correlations > .99)?

Curious minds want to know.

Regards

Rick M.

****

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

==========

Date: Mon, 30 Sep 1991 13:14:50 PDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AERO.ORG
Subject: social control

[From Rick Marken (910930b)]

Here is a long delayed reply to Kent McClelland's post on social control.

>My question is this: Under what conditions can two (or more) independent
>control systems, working in parallel in the same environment, be modeled as a
>single system? How much discrepancy in reference levels, disturbances,
>system gains, speed of response, and the like are possible before the outside
>observer would need to posit two (or more) control systems at work instead of
>one in order to model their joint behavior? How would you devise a test for
>whether two independent simultaneously operating control systems had the same
>or different reference levels?

>I suspect that if we could specify the conditions under which independent
>control systems can "cooperate" to produce behavior indistinguishable from
>one "super" control system, we would have made a start toward resolving the
>"social control" issues discussed in August.

I think I need some more information, now that I look this over. It may be that a tracking task with two people can be modeled with one control system. But why do it if there are really two? As I recall, in one of Tom's demos, one person controls one cursor and another person controls another, possibly relative to each other but not necessarily. The social part comes from the fact that each person affects their own as well as the other person's input. I don't see how this particular task can be modelled with a single control system; there are two degrees of freedom to be controlled (the two cursor positions) which implies two control systems. Is there another specific situation you were thinking of?

Many of Tom's demos show that two control systems can act cooperatively even if that is not their goal. This is what happens in the case above. You could also have two people control the difference between two cursors -- now you could run into conflict if there is a difference in the reference for what this difference should be. If you set it up so that both systems are affecting the input variable in the same way, then you probably could model this situation with a single control system -- and the accuracy of the model's match with behavior would depend on the closeness of the two references, the relative gains of the systems and all the other stuff you mentioned. I don't see why one would do this, especially when you know that there are two physically different systems working on the task -- and you know how they are connected to the input variables. Are you thinking that social control has something to do with the degree to which two actually seperate systems control systems act as though they were one? If so, then

your notion of social control differs from mine. I think of social control as something that controls the interactions between two or more people. An interaction is behavior (actions) on the part of two or more people that influence variables that are controlled by one, both or all of the people. A social controller would be something external to the people involved that controls this interaction in some way.

I think what Tom is trying to show (rather beautifully, I think) is that "interactive" control requires no external social controller. The appearance of social control (as I use the term) emerges out of the non-conflicted interation of multiple control systems. Another nice illustration of the "emergent" nature of social control is the "crowd" demo -- of Powers, McPhail and Tucker. Here, complex,coordinated social behaviors emerge out of the mutual interation of many control systems.

I think the best way to get at this "social control" issue is to define precisely what it is. Perhaps we could agree on one of Tom's demos as a prototype example of social control and then see what's actually going on-- and whether there is any evidence that there is more going on than interaction between two or more control systems controlling their own input (and, in doing so, adjusting to the effects of other control systems).

Best Regards

Rick M.

****

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

==========

Date: Mon, 30 Sep 1991 13:28:00 PDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: marken@AERO.ORG
Subject: goodby sociology

[From Rick Marken (910930c)]

Just one quick addendum to my "social control" post:

If it turns out that there really is no such think as "social control" as conceptualized by sociologists (and other social scientists) this does not mean the end of sociology -- not by a long shot. Control systems do interact; they are social. So this is what sociologists will study -- the phenomena that result from the interaction of multiple control systems.

So don't worry, Kent, there is still plenty (possibly even more) to do in a PCT based sociology.

Hasta Luego

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)

==========

Date: Mon, 30 Sep 1991 17:15:21 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: jbjg7967@UXA.CSO.UIUC.EDU
Subject: once and for all

[from Joel Judd]

OK, OK. So I committed a computer pho paw. Here's the paper, shortened actually, in a form everyone (who wants to) can receive:

SECOND LANGUAGE ACQUISITION AS THE CONTROL OF PERCEPTION

In this paper I would like to make four points: a) the phenomena of behavior is one of control of perception; b) that there exists a model of behavior which explains both HOW and WHY we behave as we do; c) that the research methodology implied by this model is NOT the typical one employed by the social sciences and SLA in particular, together with an example of typical SLA research; and, d) perceptual control loops of the kind proposed by the model are arranged hierarchically.

I need a volunteer for a harmless demonstration. [what follows is the rubberband demo--a volunteer is asked to hook one finger into a loop and I hook one of mine into the other. I tell teh volunteer to move as he sees fit, avoiding exaggerated or extreme movements, and when he knows what I am doing, to stop]

What do you observe? (responses) You will notice that as long as the rubberband was taut, I moved my finger in a coordinated fashion along with the volunteer's. If you think I was simply mirroring his movements, what would you say if I covered up his hand, or used a rubberband with three loops? One could run a correlation on the movements of our two fingers, and find a coefficient somwhere in the neighborhood of .95, or even better. This is an incredible correlation for the behavioral sciences. But what does it tell you about me? It tells you that our finger movements correlated. Somehow that knowledge is not very satisfying. Why was I moving the rubberband as I did? What if I tell you that I was trying to keep the knot over a spot on the board?Now what do you know? Does that change your perspective on this experiment? Now you know WHY I behaved as I did why, as long as the game was on, I counteracted moves made by the volunteer. Did the volunteer CAUSE me to move as I did? Well yes, and no. Yes in the sense that he caused disturbances to my goal of keeping the knot over the mark. No in the sense that if 'keep the knot over the mark' was not my goal, his movements would not have required any action on my part. I was concerned with maintaining a particular relationship between the knot and the mark on the board. He made it difficult for me to achieve that goal, so I had to DO something to overcome the disturbances.

The principles I would like to emphasize from this demonstration at this

time are three. First, I had a purpose in playing the game. My purpose was to maintain a close match between my goal ('keep knot over mark') and what I perceived with respect to that goal (through vision). My behavior was purposeful. Second, while the behavior had a purpose, it was not controlled, it was only incidental. It was the result of a comparison between my goal and my perceptions. If my perception was 'knot over the mark,' then little behavior was required; if the perception was 'knot far from mark,' marked behavior resulted. But what I was controlling is the third principle: perceptual inputs. What I wanted was to perceive the knot over the mark. My observed behavior was only one of several ways I could have achieved the desired perception (I could move just my finger, my whole body, or even the chalkboard itself). I did not concentrate on the volunteer's finger movements, or my own. What I did was to check my actual perceptions agianst what I WANTED to perceive.

Given such an interpretation of events, what can we predict? We can say that I will do what is necessary and possible for me to do in order to maintain my internal reference or goal. You cannot predict what EXACT physical behavior I am going to exhibit; you can predict (knowing my goal) that I will do SOMETHING to maintain that goal in`the face of disturbances (SHOW DIAGRAM 1--control loop from McCLelland 1991).

We saw in the demonstration how the observation of my behavior did not lead you to understand WHY I was doing what I did or perhaps you hazarded a correct guess). It was obvious that I was moving in concert with the volunteer, but even noting this (and correlating an extremely high correlation) you learned nothing about my purposes. There are only two ways in which you could find that out. One is to ask me. Of course, I can lie or mislead you, but it is possible to find out one's goals by asking what they are. How often this possibility is overlooked in the social sciences. The other, "purer" way to determine goals is to hypothesize what they are, then apply systematic disturbances to the organism and see if it tries to overcome it--to obtain the goal even though unpredictable obstacles threaten to prevent its attainment. This description of behavior is known as perceptual control theory (Powers 1973; 1989).

In diagram 1 we see the key functions of a perctual control loop. There is an internal reference signal which specifies what the system wants to perceive, the goal. This signal is fed into a comparator where it is compared with environmental input composed of one's own behaviors coupled with environmental disturbances. Discrepancy between the two signals results in an error. This is what drives behavior in an attempt to reduce the error. The system is "error-driven." The RESULTS of the actions, not the ACTIONS themselves, are what we note as feedback. Our perceptions of the environment again are compared with references and around the circle we continue. There is no beginning or end. THe notion of cause-effect is relative to what part of the loop one os referring to. Only by cutting the loop can we speak of such a relationship. It makes no sense to speak of the significance of bahevioral outputs in and of themselves. What is important is how the system perceives such behavior and other disturbances, and how these relate to goals. Without knowledge of teh system's goal(s), observable behavior can be variable and even arbitrary. The solution to the problem of understanding variable behavior (or individual variation in behavior) is to determine the goal(s) of the system(s) under evaluation.

It has been mentioned already that the research paradigm inplied by control theory is The Test, a way of determining internal goals. This is in contrast to traditional statistical methods based on sampling, proportions,

and percentages. J.G. Taylor (1950) tried to warn that a reliance on statistical method and experimental design would not take the place of sound theory and modelling, "The kinds of answers we have a right to expect from [psychologists] are not statements of porbabilities relating to isolated sets of phenomena, from which we can deduce nothing concenring any other phenomena, but general laws that are applicable throughout the whole field of psyhcology" (p.108). If we believe universal laws are those things which apply to everone, then statistics are not going to help us know what they are for any specific person, "How then are we to discover its [the mind's] laws? Clearly not by statistical methods, since these do not enable us to calculate exactly the results to be expected in an individual case" (p.109). Unfortunately few have heeded such warnings.

Rather, in SLA we find a preponderance of the following type of study (Lambert, Gardner, Barik, Tunstall 1963). In this study the authors compared two groups of native English-speaking French students in a six-week intensive French course. Previous work suggested to Gardner and Lambert that two types of motivation were at work in SLA. In an effort to determine more precisely the type of attitude most effective in SLA, they administered a battery of seven tests to the volunteer students at the institute. Corerleation matrices for the two groups on these seven measures, factor analyses of these correlations, and t-tests made up their statistical measures. The authors wished to find out about the "...attitudinal and cognitive correlates of L2 learning" (p.358). What could they base their conclusions on? Subjects were selected non-randomly, and the ability divisions used by the institute were adopted by the researchers. Large numbers were available: "around 89" for the beginning group and "around 103" for the advanced group. Of the 56 correlations reported, only 5 were .4 or higher, most (49) were .2 or less.

Upon close examination, one wonders how any confidence could be placed in the authors' conclusions. No generalizability can be made since no attempt was made at randomization. As Runkel (1990) forcefully points out, this is a key assumption one is forced to make if employing group statistical measures. Others include the central limit theorem, as well as the interchangeability of subjects. Methods employing these assumptions, calculating means and providing proportions and percentages, are called by Runkel methods of "relative frequency." The important result of using such methods is that they obviate saying anything about a particular INDIVIDUAL. More importantly, they cannot tell us what an individual's GOALS are. They force researchers to act as if subjects were only behaving according to the variables of interest to reasearcher; if not, they are often considered to be acting in "error."

The highest correlation in this study was between a measure of authoritarianism and a measure of attitude towards things French (Francophilia). It was reported as .58. Consider what this tells us. Only a third (.34) of the shared variance between these two measures is accounted for. Consider a measure of the "coefficient of alienation" for this same coefficient. Taking the square root of 1 minus r squared, we obtain a K of .82. This tells us that using the authoritarian measure as predictor of the Francophilia score is not very helpful. The correlation is 82% USELESS for making predictions about an individual subject's score on the Francophilia measure. Consider a third measure that can be applied to comparison of two means, as in the case of t-tests. One statistically significant before and after change was on the same authoritarian measure for the beginning group. Overlooking the fact they ran multiple t-tests within groups, let us look at the effect size for the above mentioned change. It is .162. WIth an

effect size such as this, and assuming normal distribution of the scores, the distribution curves obtained would be virtually overlapping. In other words, as a measure of the difference in standard deviation units before and after the language courses, the effect size tells us that for any given student the likelihood that his score falls into the non-overlapping area of the curves is too small to even mention.

The authors were not trying to deceive in this study. Rather, the limitations inherent in the statistical methods they used require them to make assumptions about their subjects and limit what they can conclude about them. By violating necessary requirements for the use of inferential measures, they are not able to speak of individual students, only GROUPS of them. Assuming that any of the "attitude patterns" resulting from the factor analyses are in fact important in SLA, it is impossible to know if any of their subjects ACTUALLY EXHIBITED such a pattern. Even forgetting about the subjects as real people, the numbers themselves are not very noteworthy.

It is evident that the authors struggle with these issues throughout the paper, as they walk a line between what they WANT to say and what they CAN say about their groups. Their theoretical hypothesis is made explicit at the outset, "... an individual successfully acquiring a second language gradually adopts various features of behavior which characterize another linguistic and as is often the case, another cultural group" (p.358). During the body of the paper they use the plural terms studentS, subjectS, and groupS in talking about the students. But towards the end they lapse back into the singular:

> "It is only with the advanced students that favorable attitudes towards France correlate highly with anomie. One can interpret these findings to mean that as a student progresses in specialization of French..." (p.367)

Their methodology does not permit such speculation, either for individuals or for the population at large.

This study I use, to repeat, not as a personal attack on the authors or their devotion to understanding SLA. It is simply typical of many research experiments in the published literature. Others include Selinker (1969), Dulay and Burt (1974), Schumann (1985) and more recently VanPatten (1990). This "scientific method" is perpetrated in part because that is the insitutionalized way to get degrees, publish articles, and obtain employment. The other reason is that people seem to believe that behavior really works in a linear, input-ouput fashion, and that if we can only determine the optimum configuration of inputs for a given learning situation, we can be assured of the desired outcome. It is against this view of learning that I argue.

The importance of recognizing control of perception and goal-driven behavior has been demonstrated. A way of testing to find out what one's goals are has been explained. My claim today is that these same principles which apply to behavior in general apply to a particular form of behavior--language. This parsimonious view of the control of perception is advocated by Powers (CSGnet 9/25/91) and other modellers. Language forms a particular instantiation of perceptual control, not a seperate "module" or special cognitive scheme.

In actuality, many perceptual control loops operate at each of the hypothesized eleven levels of perception (DIAGRAM 2--from Living Control Systems). Previously, a single perceptual loop was shown. Here we can see how numerous loops interact to provide ever more sophisticated perceptions of our world, and ever more intricate ways of interacting with it. This two dimensional...

HERE CONNECTS WITH "THE END' POST FROM THE OTHER DAY

Comments?