CSG_9108

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Date: Thu, 1 Aug 1991 06:36:42 -0600
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Linguistics, Misc

[From Bill Powers (910731.0800)]

Bruce Nevin (910730) --

"Keeping track of frequencies" gives me a problem because it implies a perceptual function that perceives frequencies of usage. The method of computing frequencies isn't the difficulty. The difficulty is in how information like that could be obtained in present time by one behaving system rather than by a linguist with vast records of how a lot of people use words.

The nearest I can come to something like what you want (within the present model, of course, which could be lacking something vital) is the sequence level. This does have the property that the elements of the next lower order will be categories, and I believe that words are symbols for categories rather than unique perceptions. It also provides for perceiving word-sequences like "now is the time" as unitary perceptions. This applies rather directly to Harris' partial orderings. It also applies to familiar sequences of nonverbal control actions, to which we refer with phrases like "Push down on the clutch then turn the key in the ignition" or "Clutch down, turn key". Given a set of recognizeable/controllable sequences like these, the next level -- programs -- could use them as elements of if-then rule-structures. The rules can apply to nonverbal sequences or to verbal ones, directly. Once you get to the sequence level, it doesn't matter whether the elements are words or nonwords.

Another possibility is in an area I've only very lightly touched on: addressing properties of memory. I've speculated about associative memory and the idea that any perception can be used as the address of any other perception stored "with" it, whatever "with" means. When many such address linkages exist for a given addressing perception, associative memory gives you back ALL of the contents that are addressed -- and this could be a whole loud chorus of associations or just an elusive sense that something goes with this address. You might logically DEDUCE frequency of association from that, although that isn't what you'd experience.

The problem with the associative-addressing idea is that I'm assuming distributed memory -- memory in every control system at every level, instead of just some big pool that anybody can send addresses to. You only get a tiny little bit of memory in any one place, and only one KIND of memory. This means that the control hierarchy has to be involved in creating addresses and detecting responses from memory in many systems at once -- it can't just be a pure memory phenomenon. The whole perceptual system is involved, too. In some ways this is an advantage because it limits the associations to the right subsystems and the right contexts. But trying to model this sort of thing is just a great big mess. It's too

complicated for me. Also I don't know enough about the properties of human memory. That's why I haven't done anything with this -- my poor brain just runs out of steam. This needs someone with a bigger boiler.

I don't have any problem with accepting that there ARE preferred word-orderings and pairings and so on. But this only tells me that a linguist can sit down with records of word-usage by many people and come up with relative frequencies of usage. It doesn't tell me that any individual system does this. I've proposed that some constructions are rare because the classes of nonverbal experiences to which they refer are rare. That's something an individual could know about. Of course I realize that this works the other way, too: word-usage affects what we are likely to think about (but not, I claim, as strongly). My point is that the mechanism that underlies the phenomenon of frequency-distributions doesn't have to have anything to do with frequency-distributions.

With regard to "pounding with an axe," my own comprehension of that sentence involves a step of imagining trying to do that. Pounding isn't chopping or cutting, so to keep from doing those things I turn the axe-head 90 degrees and pound with the flat part; also I pound with the end of the handle (making nasty dents in it). Or as you say, I use a single-bit axe. If you had said "pound with a glerfic", I would imagine pounding with something sort of hard and blunt and of indefinite shape, assuming that glerfics are in fact suited for pounding and not constructed of Jell-O. So the association for me isn't made directly from word to word. If you had said "pound with a string" I don't think I would end up associating "pound" and "string" at all -- no matter how many times I heard it. You can't pound with a string, and I'd sooner take my own experience as authority on than that anyone's words.

Words are perceptions, too, and I suppose one could listen to a foreign language and gradually realize that some argle-bargles occur quite often. There's that frequency perception, all right. Is that just learning to perceive a pattern? Is that why we say it occurs "quite often?" Or, taking "quite often" more literally, does perceiving the pattern cause a lot of jingles of previous occurrances down the time-dimension of memory? It seems to me that the time-dimension is about the only one there can be for a single localized memory store, in a model organized like mine. I don't know. We're talking about some pretty obscure properties of memory here. We don't have any experiments that will let us distinguish the niceness of one explanation from that of another.

Maybe if we use a more realistic neural model, with all the input functions of a given level bunched up together in a nucleus so they can interact, memory phenomena would also look better. At least then you'd have something more like a common pool of memory, at least within a given level or part of a level.

>Surely your objection, Bill, to the notion of neural systems
>"calculating frequencies and probabilities" rested on more than a
>semantic nicety?

I sure hope so. How do you tell?

Joel Judd (910730) --

Yes, you bring up a point again that others have mentioned (Martin Taylor comes to mind): what people actually say is pretty messy -- even in their native language. I'm thinking of Mayor Daley in Chicago some years back, and his son, now Mayor, who seems to have inherited the inability to create a spoken sentence, and President Bush. A lot of people, especially in off-the-cuff conversation, just seem to be pouring out a lot of words as they come to mind and getting the general idea across in no particular order -- trying to run a serial system in parallel mode, maybe. A listener who thinks in more ordered sentences automatically straightens out the syntax, missing references, wrong references, out-of-order fragments, incorrect words, and so on. And when that listener is asked what the other person said, the result is grammatical and makes sense (perhaps not the sense that the speaker was trying to get across, but sense).

Mary came across something pertinent, which I've persuaded her to write up: separate post.

Joe Lubin (910730) --

>How does one go about getting the LittleMan arm simulation? And all
>writings specifically pertaining to it?

The disk is in the mail. All the writeups that currently exist about it are on the disk ready to be printed with a TYPE command. I'm sending you the version without physical dynamics. You can get a copy of version 2 from me at the meeting -- it's not self-evidently runnable yet, and adjusting the parameters requires using a separate program with no instructions. I'll show you how to use it.

In the version you'll get, it's assumed that control systems exist that can bring the arm to a position where the angle-perception signals match the reference signals. So the system you'll see just has to generate the appropriate reference signals, and it gets back the same reference signals as perceptual signals -- imagination mode. But the arm does assume the specified angles, and the ray-tracing for binocular vision is legitimate.

When you come to the meeting, I'll also give you the source code (Turbo Pascal 5.5) if you're interested in that level of detail.

Bruce Nevin (910730b) --

>For avoiding the pitfall of confusing perceptions with descriptions of
>those perceptions, it might be helpful to identify programs, sequences,
>and categories that are not so readily identified with language, and to
>work with them as examples.

You know, I think you're right.

>You (Bill) mentioned programs involved in control of language itself.

How about taking some more or less lengthy sentence and analyzing it as a program (or as something that requires program-like ability)? A program

is a series of choice-points joining sequences (sequences have no branches). You go along the sentence (uncovering it, as it were, one word at a time) until you arrive at a place where a choice appears: IF such and such was the meaning, the next sequence will go one way; if another meaning, a different sequence will be next. Mary and Sam wanted [his,her] car washed. IF you mean that it's Mary's car THEN you say "her" XOR if you mean that it's Sam's, "his." It takes a program to do that. Would it be possible to construct a block diagram of the program that's needed to construct a particular sentence?

We're (CSG modelers) working on nonverbal higher-level control demonstrations, but we're hung up on the problem of how to prove that it's a higher-level task. Maybe we should just go ahead and set up some putative higher-level control tasks to see what they look like. This gets into some demanding programming, though, because you need a richer screen (a few bars of light moving up and down or sideways aren't really enough).

I like "linguistic emperialism." I take it that this was not a typo, but means imperialistic use of empiricism.

Gary Cziko (910730) --

>For some reason that escapes me completely, we've had about 15 new
>subscribers come aboard CSGnet over the past two days, many of them from
>outside the USA.

I suspect Cliff Joslyn's influence; he just came back from talking about control theory at an international Cybernetics conference (which he helped organize) in Brussells.

Welcome from me, too. When you sign up, please put your last name first in the listings. I have a program that sorts the lists by name, but right now I have to edit by hand to fix entries that go first name, last name. I'm sure you're all shocked and horrified by this.

>The problem of what can be predicted and what cannot from a PCT
>perspective is both intriguing and troubling for me.

You have to ask the purpose for which you wish to make predictions. Ponder Phil Runkel's book some more. CT is fundamentally a theory of individual organization. You get to statistical predictions for populations in a different way. First you study enough individuals to find how their control parameters are distributed. Knowing that, you can predict how a population of "similar" (oops) individuals will do the same sort of control task. You will also know better than to speak of the "average way of controlling in this task." Nobody controls that way.

If you have ways of measuring individual control parameters, wouldn't it usually be unnecessary to go through the population-study route? When you study populations, you get characteristics of the population, but you don't learning anything about an individual except perhaps the outer limits of variation within which this person might be found -- unless the person happens to be from a different population and your criteria for population membership just didn't happen to pick that up.

Wanna start THAT thread up again? Anyway, Bruce Nevin just gave the right answer.

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Date: Thu, 1 Aug 1991 07:09:18 -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: Woman Dr.; Illusion of control

[from Mary Powers]

Birdwhistell says: "when we take a tape recording and turn it over to a secretary for typing...her belief that most human beings...speak in complete sentences...leads to a situation in which she hears this kind of material on the tape. By actual count, even skilled secretaries working from unstructured interviews make about one mistake every five words. Lest we get some ideas that this is a disease which is peculiarly secretarial, let me add that our experience, utilizing the ears of some of the best linguists in America, has shown that even these experts, when working with shapes of material larger than a word or simple syntactic sentence, give us records with errors every ten to fifteen words."

So perhaps the following is a perfectly normal *spoken* sentence:

Is, um, the woman who, er, in the room, uh, is a doctor

On choice:

To me choice means looking over what's available in the environment and picking that which best reduces error. Choosing goes on constantly, and we usually only notice it when conflict creates a glitch - "I can't make up my mind". That's why restaurants put Surf and Turf on the menu.

On the illusion of control (Polly Brown)

Having control and having the illusion of control are probably the same thing subjectively. The real issue is the mind-set of the experimenter. If he thinks he is giving people the feeling of control, then he thinks that feeling is being externally caused by his actions - true enough if he has enough power over his subjects to be able to prevent them from controlling. When he stops controlling their freedom to control, then is he "giving" it to them - or is he simply getting out of their way?

What's going on in CT terms is that the subjects had been deprived of control - there was a discrepancy between their actual and desired amount of control - and they felt happy when they were able to reduce the error. They may not even have been conscious that this was an error, and may have given up trying to do something about it by setting their reference levels for control very low - expressed as passivity and apathy. Given the

opportunity to control, they had to be encouraged to do so at
first - to raise their reference levels once again and to risk
feeling errors they had become accustomed to believing they could
not reduce.
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Date: Thu, 1 Aug 1991 11:02:37 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: o tell me where is fancy bred, part 27

[From Bruce Nevin (910731.0712)]

Bill Powers (910731.0800)

Look, it's all your fault I'm lagging on my current deadline. I wish
you'd stop controlling my behavior like this!

>"Keeping track of frequencies" gives me a problem because it implies a
>perceptual function that perceives frequencies of usage.
>   The difficulty is in how
>information like that could be obtained in present time by one behaving
>system rather than by a linguist with vast records of how a lot of people
>use words.
. . .
>I've proposed that some constructions are rare because
>the classes of nonverbal experiences to which they refer are rare.

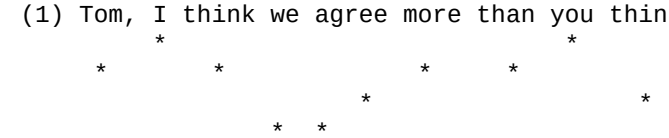
Bill, how does the control system know that the classes of nonverbal
experiences are rare? Whatever your answer, why does it not apply
equally to verbal experiences?

>My point is that the mechanism
>that underlies the phenomenon of frequency-distributions doesn't have to
>have anything to do with frequency-distributions.

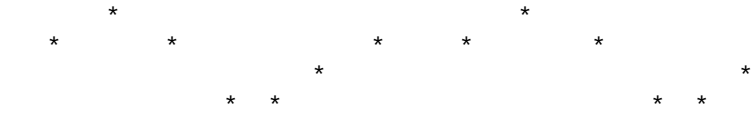
I have no problem with that. All I need is a basis for people's
expectation that "we agree" is so likely in the following sentence (1)
(taken from Closed Loop #1, 8th page) that I can unhesitatingly
construe it as being present in zeroed form and reconstruct (2) as a
more explicit form of it:

  (1) Tom, I think we agree more than you think.
  (2) Tom, I think we agree more than you think we agree.

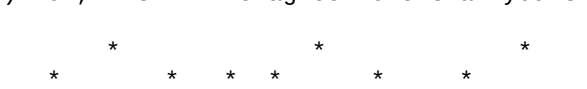
In this particular case, if I heard it spoken the reconstruction would
be furthered by the presence of the intonation for (2) clipped off at
"think", rather than the intonation for (3), as represented by rising
and falling marks below:



(2) Tom, I think we agree more than you think we agree.



(3) Tom, I think we agree more than you think.



But in written form, this intonation is part of what is reconstructed, based on the hearer's judgement as to what is likely.

[The final pitch of "think" in (1) is that of "agree" in (3), so that it is a kind of elision or foreshortening rather than a clipping off. Sentence (3) would be insulting to Tom, of course, which adds another of imagination/likelihood.]

[Intonation contour upon entry of operator on arguments, and reductions of contours together with other reductions, was part of the "other" parallel control that I omitted in my brief sketch yesterday. Aside: how hard is it to model coordination of parallel control? Some meta-perception required?]

You want to derive the verbal likelihoods from the nonverbal. Are the nonverbal perceptions privileged in some way admitting of likelihood judgements precluded to verbal perceptions?

I am not at all arguing that language packs along a baggage-train of frequency-counts, you see. Frequency as a measure of likelihood was just a means for Harris to test and verify his model, not a psychological claim. Having established the model, we can look for other and superior means for testing it, and no longer worry about frequency counts.

>I don't have any problem with accepting that there ARE preferred word-
>orderings and pairings and so on. But this only tells me that a linguist
>can sit down with records of word-usage by many people and come up with
>relative frequencies of usage. It doesn't tell me that any individual
>system does this.

No: the linguist can sit down with people and ask, for pairs of utterances, whether they are different as to their likelihood. Differences found for a pair in form A must be preserved for the corresponding pair in form B (or at most collapsed, predictably, with certain operators) or else A and B are not transforms. This was the criterion for transformational analysis. It is no longer needed insofar as we accept that the work of analysis has been done. I know Harris talks about frequencies in _Language and Information_, but that is only because he doesn't have a disciplined and principled way to talk about perception, memory, and imagination--he doesn't have CT.

I think that the likelihood of the words falls out from the likelihood of the whole complex of perception--real-time, remembered, and imagined, verbal as well as non-verbal. What Malinowski groped for with his talk about "context of situation".

The nonverbal perception of pounding with string is much harder to imagine than the nonverbal perception of pounding with a hammer or something of category "hammer". Why? (If the only tool you have is string, every problem is bound to look like a package.)

>I believe that words are symbols for
>categories rather than unique perceptions.

What is happening when I refer to a perception on some other level?

* a specific intensity ("yes, the second one is definitely brighter" "Ow! That's too loud!")
* a perception (yellow)
* a configuration (lemon)
* a transition (turning)
* an event (a toss)
* a relationship (material for lemonade)
* a sequence (steps to make lemonade)
* a program (what to do if no sugar)
* a principle (have contingency plans)
* a system (making things with Sarah helps our marriage)
* a conversion/paradigm-shift (this new definition of quality time makes sense)

Categories mapped onto each of these? You may be right, that all the "meta"-ness of language is in a capacity of the category level to apply to any perception whatever, but that feels like shifting the problem under a different rug.

It seems to me that levels 1-6 are constitutively hierarchical, that is, each builds upon the prior in a directly constructive way (sensations are made up of intensities, configurations are made up of sensations, transitions are made up of configurations, events are made up of transitions, relationships are made up of events). Level 7, category, seems to escape this progression, as noted. Then, arguably, levels 8 and up are a constitutive hierarchy above the category level, as we have been discussing.

I think that associative memory/imagination links are the basis for the fuzzy semantic classes of words as well as of the well-defined operator and argument classes, and I tried to suggest how this might work in a recent post. I am sorry that memory is such a murk in the model. You may need to allow watery, non-graph-theoretic things like neuropeptides and feeling-tones to flow about the circuitry after all. A great source of wildcards for reorganization! Should I look up some references?

>If you had said "pound with a glerfic", I would imagine pounding
>with something sort of hard and blunt and of indefinite shape, assuming
>that glerfics are in fact suited for pounding and not constructed of
>Jell-O. So the association for me isn't made directly from word to word.

You would know that a glerfic must be hammer-like (a) because you remember situations when "pound" was used appropriately and inappropriately, (b) because "pound" is associated with a fuzzy semantic class of words so typified by "hammer" that you might even use that as a classifier word, (c) both, inextricably. I vote for (c). I agree with you, you see, that verbal perceptions are controlled in and by the identical perceptual control hierarchy as all other perceptions. There are almost surely at least partially distinct areas of associative memory, however.

>The nearest I can come to something like what you want (within the
>present model, of course, which could be lacking something vital) is the
>sequence level.

>rules can apply to nonverbal sequences or to verbal ones, directly. Once
>you get to the sequence level, it doesn't matter whether the elements are
>words or nonwords.

>. . . gradually realize that some argle-bargles occur quite often.
>There's that frequency perception, all right. Is that just learning to
>perceive a pattern? Is that why we say it occurs "quite often?"

This seems to be a suggestion that (long-term) associative memory is the process of acquiring a sequence-level control system. I ask again: are we growing these neural connections when we learn? When we establish a long-term memory? I am ignorant.

>How about taking some more or less lengthy sentence and analyzing it as a
>program (or as something that requires program-like ability)? A program
>is a series of choice-points joining sequences (sequences have no
>branches). You go along the sentence (uncovering it, as it were, one word
>at a time) until you arrive at a place where a choice appears:
>. . . Would it
>be possible to construct a block diagram of the program that's needed to
>construct a particular sentence?

I refer you again to Stephen Johnson's dissertation. He has done just this in a Harrisian framework, using Prolog. I can send you a copy if you want. I don't have the program, but he might be willing to share that.

>means imperialistic use of empiricism.

No, just a typo. I don't think I'm an empiricist, certainly not in the much derogated sense of logical positivism. But I like puns too.

Be well,

Bruce Nevin
bn@bbn.com

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Date: Thu, 1 Aug 1991 08:53:18 -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: CSG meeting mac

[From Rick Marken (910801)]

Just a quick note to let those of you attending the CSG meeting know that I apparently will be able to get a hold of a portable Mac to bring to the meeting. So if anyone besides me has Mac demos or experiments or whatever to share at the meeting, please bring them.

Gary Cziko -- the chapter looks great. I skimmed it last night. It took me several years to reach the level of understanding of PCT that you evidence after only, what, one or two years. Well written too.

Hasta Luego

Rick M.

****

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Date: Thu, 1 Aug 1991 13:23:33 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 <silvert@BIOME.BIO.NS.CA>
Subject: Ecological applications

I'm a new subscriber who has just decided to drop out for the present -- my interests are really peripheral to those of most of the active correspondents in the group, but I thought that I would post a short message about my interests in case anyone out there is interested in corresponding.

I'm a marine ecologist interested in applications of control theory to the understanding of marine ecosystems as well as to modelling the fishery and other anthropogenic factors. Currently I'm interested in strategies of optimal foraging with a mixed food resource, and I'm interested as well in the evolution of alternate control strategies in cases where the optimal control trajectory bifurcates. For example, at some levels of food availability the value of the objective function (generally some measure of growth efficiency) is the same whether an organism expends a lot of energy in greedy feeding or conserves energy by becoming an ambush predator.

If anyone is interested in any of these topics I would be pleased to hear from you.

Bill

--
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Date: Thu, 1 Aug 1991 14:18:35 EDT
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Self-organization

[Martin Taylor--910801:1410]
(Marken 910730)--
> I guess I don't see "self-organized" systems as a necessary
>intervening step between non-living physico-chemical systems and controlling
>(living) ones. I am guessing that these kinds of dissipative systems don't
>have any special status in the life/non-life continuum. They are just another
>type of system (like the solar system) with interacting cause-effect
>components that produce interesting phenomena. I am betting that the big
>evolutionary step (from life to non-life) occured when chemicals became
>organized as closed loop negative feedback systems.

I agree with the last sentence. My point is that this step is much easier to achieve when the complex system within which it happens has settled into some reasonably deep attractor basin, so that it has a reasonable chance of persisting a long time. The evolution of (a) a negative feedback control loop responsive to a reference variable, and (b) a technique for making such loops, is likely to happen only in a reasonably stable organized structure, and such structures occur only in dissipative flows (a rock is not such a structure, but a standing wave in a river is one).

I agree that self-organized structures are not stable against prolonged disturbances, except that there might be a continuous transformation of the attractor structure. It is the evolution of the control system that permits such stability. And I am betting that the "big evolutionary step" occurred within the self-organized dissipative flow of solar and geo-gravitational energy into space.

Martin Taylor
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Date: Thu, 1 Aug 1991 14:14:32 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: categories here, categories there . . .

[From: Bruce Nevin (910801.1311)]

Here's an observation that may bear on the role of categorization:

Conventionalized things (rituals, dance steps, language) do not have to be imitated to be repeated. My way of pronouncing "pot" with virtually absolute certainty differs from yours, so it can't qualify as a very exact imitation, but it easily qualifies as a repetition. Non-language sounds, on the other hand, can only be imitated, they cannot be repeated.

This means that there can be a great many differences (between repetitions) that don't make any difference for the purposes for which the conventionalized behavior is being controlled.

This suggests to me that the purposes for conventionalized performance (language, etc.) are addressed to the conventional *type* (category, level-6 perception), disregarding detailed differences between one or another *token* (constituted by level 1-6 peceptions).

The type-token (class-membership) relation may appear so general in part because we can focus attention at different levels. It is unclear to me still how a set of relationships constitutes a category (not in the 73 book), but taking for granted that it does: perceiving any member of a category, we can attend to it on the relationship level, event level, transition level, configuration level, sensation level, or intensity level. We may descriptively attribute "class-hood" to sets of sensations on any of these levels, because language lets us do that.

A phoneme can be thought of as a class of sounds. The appropriate locus for thinking about phonemes is on the level of the relationship of contrast among the partially-repetitive utterances of a language. Sounds (phones) are not relationships, I take them to be events, demarcated by intensities, sensations, configurations, and transitions. The category /b/-in-English and the category /m/-in-English are constituted of the relations of contrast between /b/, /m/, /p/, and all the other phonemes of English (or +voice, -voice, +nasal, -nasal, etc., or the articulatory gestures and timing of the Haskins Labs group, etc.) There seem to be classes of sounds, or classes of sound features, but these are artefacts of our talking about categories with reference to levels of perception below the level on which they are constituted, the level of the relationship of contrast.

So far so good. Maybe. Let's say so for now.

How then do we get to talk about categories of events, programs, etc? (Noted previously.)

Lunch is over. Back to work.

Bruce Nevin
bn@bbn.com

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Date: Thu, 1 Aug 1991 15:10:58 EDT
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: reply to Bill

[Martin Taylor 910801 15:00]
(Bruce Nevin 910730 1238)--
>It remains adamantly true that language can be meta to anything,
>including all of itself. (Therefore the grammar of a language must be
>capable of statement in the language itself, that is, in its
>metalanguage, given if necessary invention or adaptation of the required
>small vocabulary. This is a very strong constraint on possible
>grammars and therefore on what can be there for hierarchical control.)

>
Why on earth do you say that? It seems an awfully strong hypothesis to say that language can describe all of what is done with language, especially if you add the (unstated) constraint that it be done in finite time. Personally, I think that only a kind of articulated skeleton of language can be expressed in language, and that most of what language does is inexpressible. And that goes double for non-linguistic experience.

Language is evolutionarily very new. It would be absolutely astounding if already it could do what you claim for it. It is a means of communicating, primarily for those things that can be publicly observed or acted upon, and for things that involve relationships (and relationships or relationships) among such things. As a means of describing natural objects, or the emotional effects of situations, or the actions of biological entities, or ... language, it is pretty poor, I think. Better than anything else we have, but a lot worse than we might have. And if you take as correct what some people have said, that mathematics can be described in natural language, then Goedel's theorem seems to contravert what "remains adamantly true."

Martin Taylor

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Date: Thu, 1 Aug 1991 15:26:32 EDT
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Self Organization Down the Drain

[Martin Taylor 910801 1520]
(Gary Cziko 910731 1458) --
> self-organizing systems
>self-organize in order to "obey" the second law of thermodynamics which
>says that the total entropy in a closed system cannot decrease but can only
>remain the same (in a system at equilibrium) or increase (in a system
>far-from-equilibrium like the filled sink with the drain plug removed).
>
Not quite right. The entropy of an isolated system cannot decrease, but in a non-isolated system (such as the ones we are talking about) can do anything. The entropy of a self-organizing system may well decrese, but that decrease is more than compensated by an increase somewhere else in the universe (because the flow is far from equilibrium). In our case, the increase is found in the redistribution of heat from the sun to space.

Martin Taylor

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Date: Thu, 1 Aug 1991 15:44:01 EDT
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Spoken sentences

I thought this discussion might be illumined by an actual example, since there seems to be a sense of hallucination about what people really say.

The following is from an academic monologue, not a child learning the language. It was an informal lecture, and sounded perfectly fluent and coherent to the listeners. I taped and transcribed a little bit of it. It is not an example of bad speech, but of normal spoken monologue.

" By saying something like 'delete my file' it's an action that in by saying it has been has been doen even if it's been done by a machine and not actually themselves."

Is that or is it not as bizarre when analyzed as the "doctor" sentence that "no child would ever utter"?

Martin Taylor

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Date: Thu, 1 Aug 1991 12:48:04 -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: Ecological applications

[From Rick Marken (910801)]

Bill Silvert (910801) says:

>I'm a new subscriber who has just decided to drop out for the present --
>my interests are really peripheral to those of most of the active
>correspondents in the group

It doesn't seem so to me. We are interested in control phenomena, particularly those exhibited by living systems. There has been a lot of discussion of language and conflict and whatnot -- different examples of control phenomena. Control of one's own food supply ("foraging") is certainly an interesting example fo control. I'm sure a discussion of what is known about this phenomenon, and the models used to explain it, would be of great interest to many members of CSGnet. I would certainly be interested -- and I would try to contribute to such discussions if I could.

> Currently I'm interested in
>strategies of optimal foraging with a mixed food resource, and I'm
>interested as well in the evolution of alternate control strategies in
>cases where the optimal control trajectory bifurcates.

This sounds like something that would be well worth discussing on CSGnet. I know of some work on this by J.E.R Staddon. I bet PCT could provide a new perspective on this phenomenon (as well as new research goals and strategies).

>If anyone is interested in any of these topics I would be pleased to
>hear from you.

I'm interested. But I think I would prefer to have the discussion over CSGnet, where we might benefit form the many different perspectives and backgrounds. One subversive goal of CSG is to break down, as much as possible, disciplinary boundries in the life sciences. You may find that some of your ecological problems have been solved by economists, psychologists or control engineers.

Regards

Rick M.

****

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Date: Thu, 1 Aug 1991 17:10:19 -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 #3

[from Gary Cziko 910801.1700]

All Control System Group members who have paid their dues recently should have received by now Closed Loop #3 edited by Greg Williams and sent out by Ed Ford. It contains 35 pages of selected threads from CSGnet from the past couple of months. (It is in tabloid form, so it is easy to read on the subway.) This issue's theme is "Competition, Morals, Religion, and Science."

Greg has also sent me a diskette of Closed Loop #3 to send electronically to people on CSGnet who would like a copy, but I will not do this until after the CSG meeting which ends August 18. My personal preference is to make Closed Loop available only to paid-up CSG members, but we will discuss this at the meeting and I may lose.

In the meantime, to be sure to get a copy of Closed Loop #3 and future issues, send $25 ($10 for students) to Mary Powers, 73 Ridge Road, Durango, CO 81301. Also, those of you who would like to be excerpted in Closed Loop but who have not sent a copyright release form to Greg Williams should let me know. I can send a form for you to fill out and send to Greg.--Gary

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Date: Thu, 1 Aug 1991 21:36:10 -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: Model-Behavior Correlations

[from Gary Cziko 910801.2115]

I am writing an article on educational research which will discuss perceptual control theory and I need some help with references from the modellers like Powers, Marken and Bourbon.

I need to cite some published studies that show high correlations between control-theory models and actual human behavior. In some ways this is a little silly since I have Powers's Demo2 and anytime I want I can get my own correlations of .99+, but I need some published stuff. I know of Bourbon's article in American Behavioral Scientist, but he gives correlations between behavior and disturbances and for the argument I'm making I need behavior-model correlations.

Thanks in advance for any help you modellers can provide.--Gary

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Date: Fri, 2 Aug 1991 09:48:02 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 <silvert@BIOME.BIO.NS.CA>
Subject: Ecological applications

After receiving several very nice replies from Gary and others about my dropping from CSGnet, I came up with a compromise of setting up a separate account for this and other voluminous mailing lists (such as cybsys). My reason for dropping out wasn't that I didn't like the list, but I get a lot of urgent mail and adding a couple of arriving mail messages an hour was a hassle.

Just to give some idea of my interests, here are some of the papers I've written that relate in part at least to control theory as applied to fisheries or marine ecology:

Silvert, W. 1977.
The economics of over-fishing. Trans. Am. Fish. Soc. 106:121-130.
Straightfoward application of control theory to optimizing discounted revenue from fishing. There is a sizable literature in this area now.

Silvert, W., and W. R. Smith. 1977.
Optimal exploitation of a multi-species community. Math. Biosci. 33:121-134.
A more sophisticated application of control theory in a case where the system being controlled has strong internal interactions (it eats itself!).

Silvert, William, and Howard Powles. 1983.
Applications of operations research to the design of field sampling programs.
In "Sampling Commercial Catches of Marine Fish and Invertebrates,"
W. G. Doubleday and D. Rivard, eds.
Can. Spec. Publ. Fish. Aquat. Sci. 66:268-278.

Silvert, William. 1981. Comment on optimum swimming speeds in fish.
Can. J. Fish. Aquat. Sci. 38:729.

Silvert, William. 1982. Optimal utilization of a variable fish supply.
Can. J. Fish. Aquat. Sci. 39:462-468.

Silvert, William. 1983.
Is dynamical systems theory the best way to understand ecosystem stability?
In "Population Biology" H. I. Freedman and C. Strobeck, eds.
Springer-Verlag Lecture Notes in Mathematics 52, pp. 366-371.
In this paper I argue that control theory is the best way to understand
ecosystem dynamics, since organisms are constantly optimizing their
growth trajectories rather than obeying continuous differential equations.
This leads to discontinuous switching behaviour and to models which
behave qualitatively different from, say, Lotka-Volterra models.

Several individuals replied to my earlier posting, and Gary kindly
forwarded their comments to me.

> Date: Thu, 1 Aug 1991 21:13:30 -0500
> From: marken@AEROSPACE.AERO.ORG (by way of (Gary A. Cziko) g-cziko@uiuc.edu)
> Subject: Ecological applications
>
> I agree with Rick in believing the control theory could be very useful to
> you. I would suggest that you get back on the net and post some messages
> about what questions and phenomena you are interested in and give people
> like Powers and Marken the chance to respond. Then decide if you think
> control theory is peripheral.

I hope I've made it clear that I don't consider control theory
peripheral, but only that it is a big subject and I felt that most
postings related specifically to humans as the major biological system
under consideration. I'll hang on see what I can learn!

> [From Rick Marken (910801)]
> Control of one's own food supply ("foraging") is certainly an interesting
> example fo control. I'm sure a discussion of what is known about this
> phenomenon, and the models used to explain it, would be of great interest
> to many members of CSGnet. I would certainly be interested -- and I would
> try to contribute to such discussions if I could.
>
> This sounds like something that would be well worth discussing on
> CSGnet. I know of some work on this by J.E.R Staddon. I bet PCT
> could provide a new perspective on this phenomenon (as well as new research
> goals and strategies).

Rick, I would appreciate details. Sounds interesting.

> I'm interested. But I think I would prefer to have the discussion over
> CSGnet, where we might benefit form the many different perspectives
> and backgrounds. One subversive goal of CSG is to break down, as much as
> possible, disciplinary boundries in the life sciences. You may find
> that some of your ecological problems have been solved by economists,
> psychologists or control engineers.

Actually much of the work I've done is based on work done by economists (and of course the optimal fishing work really is economics of renewable resources).

One general observation that I've made in ecology and which applies to many biological systems is that the high noise level means that sophisticated solution methods don't get you very far. This is fortunate in a way, because many of the problems we encounter are highly nonlinear and otherwise difficult to tackle rigourously. However, cruder methods like optimization in parameter space are simple and effective, especially since the choice of control strategies is often highly constrained.

I'll be happy to hear more on any of these topics as soon as I get this account business straightened out (mail sent from this account is being replied to at my other account, so ...)

Bill Silvert

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Date: Fri, 2 Aug 1991 09:05:48 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: limits of language

(Bruce Nevin 910802 06:52)

Martin Taylor 910801 15:00

>It remains adamantly true that language can be meta to anything,
>>including all of itself. (Therefore the grammar of a language must be
>>capable of statement in the language itself, that is, in its
>>metalanguage, given if necessary invention or adaptation of the required
>>small vocabulary. This is a very strong constraint on possible
>>grammars and therefore on what can be there for hierarchical control.)
>>
>Why on earth do you say that? It seems an awfully strong hypothesis to
>say that language can describe all of what is done with language, especially
>if you add the (unstated) constraint that it be done in finite time.

The operative words here are "can be" and "capable of". The constraint is that the grammar must be statable in the metalanguage that the language contains as a sublanguage. This does not mean that our control system must use language--as we pronounce it and write it for purposes of making, displaying, and replicating of perceptions--to maintain the relations and carry out the operations of the grammar in real time.

>Personally, I think that only a kind of articulated skeleton of language
>can be expressed in language, and that most of what language does is
>inexpressible.

It may be that your "articulated skeleton of language" is equivalent to what we mean by the grammar of the language.

One can use language to describe anything, including what one does with language. We don't usually do that, and so far as I can tell we don't do both the "languaging" and the metalanguage description of "languaging" at the same time. More to the point, the grammar of a language does not describe all that is done with the language, it describes the language with which we can do those things.

The relations and operations of grammar are few and easily within the grasp of language to state. The complexity is in their recursive combinability, and most of the complexity in the work of determining them by linguistic analysis is due to the layered abbreviation of that complexity by the reduction system.

As we understand better the implications of the observation that the objects and relations in language, and the relations and operations of grammar, are perceptions no different in essence from any other, and controlled in the same way and in the same control hierarchy as all other perceptions, the stipulation "as we pronounce it and write it" comes to seem somewhat arbitrary and artificial--in precisely the same way and sense, it seems to me, that language is arbitrary and our uses of it matters of artifice.

> And that goes double for non-linguistic experience.

The distinction between language and non-language perceptual control becomes I think more arbitrary the farther we get (in the perceptual hierarchy) from the conventionalized sounds or marks by which we perceive another's use of language and recognize it within ourselves. The observable fact that the metalanguage is in the language amounts I think to a reflection in the outward manifestation of language of its inner unity with perceptual control in general, together with the fact that we can talk about anything to which we can pay attention.

>Language is evolutionarily very new. It would be absolutely astounding
>if already it could do what you claim for it. It is a means of
>communicating, primarily for those things that can be publicly observed
>or acted upon, and for things that involve relationships (and
>relationships or relationships) among such things. As a means of
>describing natural objects, or the emotional effects of situations, or
>the actions of biological entities, or ... language, it is pretty poor,
>I think.

What we say about perceptions to which we are attending "makes sense" only to the extent to which our interlocutor can and does pay attention to perceptions of the same sort, and recognizes the correspondence of dependencies among the words and other structures of that bit of language to dependencies among those nonverbal perceptions.

It is precisely by this process of reaching agreement about perceptions which are necessarily *not* public that we make of them for ourselves and for one another public objects and relations. In this crucial sense, the word "publicly" in your statement above cannot be presumed as a prior given but must be understood as a product precisely of the communication for which you presuppose it. All that is public is an agreement, and that by virtue of the transaction of agreeing. Some

things are easier to come to agreement about (that box, we all know what a box is), others not (one sees the room as a mess, the other not), and this may depend upon prior agreements called a culture (c'mon guy, that's not a club, that's a spirit staff, what planet do you come from!). "Inner" perceptions such as bodily states, systems, principles, programs, etc. seem inherently less public because they are less easy of agreement and agreements about them are less easy to verify by coordinated action, but neither a box nor the chore of picking it up is public until an agreement is reached about it.

Typically, the correspondence of dependencies among some nonverbal perceptions to dependencies among the words and other structures of some bit of language is not identical in two people who have reached an agreement, but can be made closer through dialogue and need only be similar for people to coordinate their activities and to provide a framework to support further communication.

Communication includes much more than language. Communication of attitude, interest, emotional state, and other matters crucial for relationships between individuals, is largely gestural, and much less conventionalized than language is. To take just one aspect of this, language can be used to describe an emotional state, but doing so does not communicate that emotion. It falls flat, just as the translation of a joke from another language falls flat (and for much the same reason). But I have not said that language could do this. So we must distinguish two aspects of communication. The first, Harris calls transmission of linguistic information as distinct from communication in a more empathetic sense. The first regards a perception of agreement about a correspondence between the perception of structure in a bit of language use and a corresponding perception of structure in the (necessarily different) nonverbal perceptions of two people. You must imagine the words read in a flat, unexpressive intonation. The second invites empathy: how would it feel to me to be holding my face in that expression? To be pounding my fist on the table like that? To be speaking with such high pitch and loudness, with such strong differences in pitch and loudness between stressed and unstressed words? Not a Dr. Spockian "I observe signs of elevated levels of blood pressure in the area of your neck and face," but an immediate, and biologically much more primitive "Wow! You're really mad!" That's communication. And we can use language to do it, just as we can use the manner of driving an automobile to do it, but it's not what language does any more than it's what car-driving does.

>Better than anything else we have, but a lot worse than we
>might have.

One of Harris's and Sapir's interests has been how desirable capacities might be added to language. There's a paper on this in the longer original edition of Harris's collected papers. But the undeniable imperfections of language (mostly due to the limits of social convention felt in the reduction system and the more easily remediable limits of vocabulary) do not vitiate the point that the *grammar* of language must be within the same capacity for perceptual control as is the language itself. There--does that restatement help?

>   And if you take as correct what some people have said,
>that mathematics can be described in natural language, then Goedel's
>theorem seems to contravert what "remains adamantly true."

Borel's statement was not that mathematics can be described in natural
language, but that it is not self-sufficient, but rather depends upon
prior agreements established in and through natural language.

One mathematician could read any mathematical text to another over a
telephone, using the natural-language expressions that designate the
objects, operations, and relations of the formulae. Naturally, it would
be much easier to refer together to the formulae, and it would be
unbearably tedious to do it that way, but that is not a statement about
the capacity of language. Just so, it is unbearably tedious to render a
morpheme-by-morpheme translation of a text in Achumawi (even in normal
English word order). The assertion of evidential status with each
inflected verb is easy in Achumawi, just a choice of suffix a, suffix b,
or zero, whereas in English the higher operators and modifiers must be
explicitly stated. Suitable reductions could be added to English for
expressing frequently stated and so expectable complex ideas in compact
form (as an affix or "particle"), making a more specialized sublanguage
for mathematics. Indeed, special reductions are in fact developed in
specialized sublanguages of technical domains. For language users to
develop agreements about very compact reductions, such as the definite
article, or the -en of the passive (from something like "in a state of")
through normal processes of language change takes more than a couple of
generations. But such developments are in fact typical of language and
account for much of the diversity of languages that share a common
ancestry.

Goedel's theorem says that not all propositions are decidable. You'll
have to explain the connection.

        Bruce Nevin
        bn@bbn.com
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Date: Fri, 2 Aug 1991 07:29:55 -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: Language; vortex

[From Bill Powers (910801.0900)]

Bruce Nevin (910731) --

> ... the ordering of assertions or commands can coherently differ from
>the ordering of actions.

Right. What I'm trying to express is that the kinds of sentences or sets
of sentences we are most likely to use are those that designate
perceptions we actually experience. There are requirements of sequencing
in the control processes of changing a tire or shooting an arrow. We can
apprehend such sequencing directly, and we learn from experience which
sequences to create in perception -- most others are not controllable

(they don't work). Any sentence or set of sentences that manages to create the right experience of sequence among the perceptions referred to by the words bow, arrow, nock, release, and draw back will serve to indicate the actual sequence required to shoot an arrow. We may not fill in the pieces of the mental image in the same order that it is executed, but when we have finished the communication, the perceptual sequence must have been filled in correctly somehow.

The same perceptual capacity is needed to apprehend the ordering of words and phrases within a sentence and among sentences that is needed to apprehend the sequence of physical relationships to which the words refer, such as shooting an arrow. I'm trying to point to the ability to perceive in terms of sequence, not to the kinds of elements that are sequenced.

>Suppose I paint a picture. With acrylics. The colors, shapes, and
>relationships correspond in a satisfying way to colors, shapes, and
>relationships that I perceive beyond my easel. Then I strip away the
>paint from the canvas. What is left of the painting?

If you have seen it before the stripping, quite a lot but not everything. You may have a memory of the categories such as wharf, ship, clouds, and so on without remembering whether the clouds were over the ship or the ship was tied to the wharf. You might remember (in an abstract painting) that there was a sequence of figures each laid over the one before it, yet not remember what the figures were or what colors they were.

Higher-level memory is what we call "remembering that." I remember that I had breakfast but not what I ate. I remember that it was a beautiful principle for doing a merge-sort, but I don't remember the program steps. I remember that he said I was to meet him after lunch at the museum, but I don't remember the words in which he said it. This means that I remember the mental image of eating lunch and then meeting someone at the museum -- without the descriptive words that evoked these nonverbal perceptions.

"Remembering that" is remembering a higher-level perception without remembering the lower-order ones from which it was derived. So it's a useful phenomenon for our discussion here. It makes clear the difference between a lower level perception experienced by itself, the same perception accompanied by a higher-level one *about* it, and the higher-level one without the lower one. It shows what it's like to experience the higher-level perception without the normal background of "corroborative details." You can remember that someone spoke a sentence without remembering the sentence -- so "sentenceness" is something that can be perceived without its residing in the words of any specific sentence.

My son Denny thought of tacking "-ness" onto words to indicate that the lower levels are missing. "Understandingness" is the sense of understanding, which can be experienced without the actual phenomenon of understanding having taken place. We've all come out of brilliant entertaining lectures with that sense. What I'm talking about in terms of word sequences is the sense of sequenceness, which is the pure sequence-level perception divorced from the elements that are in sequence.

In ordinary unanalyzed experience all the levels of perception are projected into the same space. It isn't obvious that they are independent perceptual modes, nor that there are dependency relations from level to level. So the sense of sequence we experience in the words of a sentence appears to reside in the sentence; it's not obvious that the word-categories are the variables in a function that yields the sequence-signal, so that the sense of sequence is really a sort of opinion about the raw sentence. We can form the same sort of opinion about the course of events in three-cushion billiards or in jump-rope games or in playing pattycake. To understand what I mean here you have to be willing to consider that sequence is a way of perceiving and not an objective fact.

>You'll have to tell me which of Chomsky's notions you mean.

What do I know about it? I just mean my impression that Chomsky is trying to find some sort of structuring principle that is independent of the things structured. I don't think he's just trying to generalize. What he's looking for, in my opinion, is a level of perception.

>But we are much clearer and more consistent about it [designating
>perceptions] in the case of language because the meta-relationship of
>correspondence is largely conventionalized. We control for perceived
>conformity to social convention.

Yes we are and do. I've mentioned before a complex task that Dick Robertson used, which involved several levels of problem solving -- and that when I found a way to use word-sequences, I could solve it in one trial. Before that it took twenty or thirty trials (the conditions were varied randomly) using nonverbal means alone.

So I'm thinking two thinks about this. First, being able to symbolize offers some very great advantages. Second, language is a specific invention that makes use of the symbolizing ability in a particularly efficient way. What is invented about it is its conventionality, the reduction of the conventional set of symbols (out of the set of all possible perceptions that could serve) to a small and completely masterable number.

This speaks to me of the program level. I can see that languages will share the property that the program level is involved in their use. But are there any constraints that say that the programs must all share some particular rules or operations? I haven't heard that anyone (even Harris) has found program rules that will predict the form of absolutely every utterance in every language. If anyone had actually done and demonstrated that, why would there be more than one theory of language, the one that describes how it actually works? I think it's perfectly possible that different languages use different rules, and that there are no universal features (i.e., what universal features there are that seem to have been discovered are just the products of human ingenuity, having perhaps descriptive merit but no function in the system itself). It's possible that the only universality there is is in the sharing of the same categories of perception and control.

>An aside: there is no movement of words in operator grammar.

Accepted. I was just referring to "Is the woman who __ in the room is a doctor." MY aside: if anyone DID utter such a sentence, I would be able to answer it, assuming I knew whether, doctor, woman, room, in, was.

>Control systems y1 through y27 control perception of syllable onsets, >syllable rhymes, syllable nuclei and codas (or perhaps dependencies >among different sonority-classes of phonemes), for a syllable structure >we might represent as CVC. syllables. Control system W728 ...

OK, OK. It wouldn't be very informative to the likes of me (like standing too close to a half-tone picture) even if it's right. This is where you separate the linguists from the dilettantes.

>You might be arguing that the higher levels of control whose evidence we >see in spoken or written words are identical with and no more nor less >than those required for perceptual control sans language.

That is what I am arguing.

>But if this were the simple truth of the matter, then languages would be >more alike than they are.

This doesn't necessarily follow. The programs that run in computers aren't alike just because they run in (even identical) computers. To say that the program level allows implementation of rules doesn't imply that only one set of rules can be implemented.

>The language-specific patterning comes from constraints on what you can >say and how you can say it that are arbitrary and socially established >by individuals agreeing to conform to them.

No problem with this. If you are controlling for being understood, you adopt the conventions that are recognized by those around you wsith whom you desire to communicate.

Re: "Navajo" concept of time.

>Hopis. Benjamin Lee Whorf, a collection of whose writings were >published in 1954 as _Language, Thought and Reality_. No, it's not a >joke. The differences don't apply to perception or description of >temporal, but rather to our concept of time as "latering and latering".

Thanks. Do I have to read Whorf, or will you tell me what we can say that Hopis can't? I would very much like to think of an experiment to see whether this difference implies a perception that they have and we don't -- that they can control for and we can't, or vice versa. I live within striking distance of Hopis, and can easily get to know some people at Ft. Lewis who are concerned with Indian affairs (Ft. Lewis was originally an Indian school). Why don't we start trying to salt Ft. Lewis with CT people? Maybe we can turn the whole damned place into the Center for the Study of ...

Re: the completeness of descriptions of nonverbal perceptions:

>However, all of the necessary details *can* be described in
>language (as indeed you exemplified).

There's a difference between referring to perceptions and describing them. I spoke of "the way spaghetti looks when it's half-wound on your fork." That *refers* to a perception. But I doubt that anyone could describe this visual appearance so the description matches every detail of what is seen. "Seventeen strands, arranged so that the third and second .." I can't even start. But I can certain SEE what I would be trying to describe. I think that language leave out MOST of the details of perceptions, that just-so particular way they are experienced. Tell me, just what IS the difference between the sequences A,B and B,A? You can try to describe the difference many ways, using words like "before" or "then," but then you're stuck with trying to say how "A then B" is different from "B then A." Eventually you have to admit that the words can't convey; they can only point. Sequenceness is a perception.

I said:
>>At the level where language works in this meta fashion, it MUST have
>>left behind all the elements that get ordered by its actions.

You said:
>I assume you say MUST because of the problem of infinite regress.

Not infinite regress. It's because the "meta" perception is of a different logical type from the lower level elements. The sequence "B,A" is neither "B" nor "," nor "A". It comes from noticing that in the three elements arranged to imply (by convention) a left-to-right direction, there is something you detect as ordering. If you scan the other way, you perceive a DIFFERENT ordering in the same elements arranged the same way. If you perceive in reverse Polish, you see A as the connection from , to B, so translated to left-right scan the ordering is ,AB. I'm just trying to say that the elements of a sequence are not sequences; that they are the world at which a sequence-perceive looks.

Your notes from Harris' lecture might help here. I would suspect that a metalanguage statement must first involve attaching a label to some lower-level aspect of a sentence (noun, verb, etc) and then constructing sentences about those labels. But I'll leave that to you; I truly am trying to stick to being a control theorist. I don't have a handy way to explain, for example, how we go about attaching a label to a rule. You may end up being quite right about saying that language is meta in a way I haven't considered. But I think we have to do our best to explain what we can with what we have before making a fundamental addition to the model (which, if defensible, I would love to see).

>Language has its own structure, instantiated through control of
>perception of phonemic contrasts etc., and corresponding to structure in
>other perceptions (in the control hierarchy), but not identical with or
>entirely merged with it. This structure persists (with variation) when
>you examine utterances of one person or another, and it persists (with
>change) through time.

This is the basic issue. Are there things we do with language that require perceiving and controlling in categories that I have left out of

my proposed levels? Or are we only talking about doing with language things of a different kind but still within these general categories? If you can find perceptions of types that are in language but aren't in the HCT model, you will have made a major addition to the model. Of course we will immediately try to see if the same types can't be found in nonverbal control tasks! This, in recognition of my basic assumption that the brain contains no functions confined strictly to language. If that assumption is wrong we have an even more important change in the model to consider.

Re: fish talking about water:
>Lovely! But your fish was talking about eddies and pressures, not
>water, and your human didn't mention air at all.

When you analyze any perception, it turns out to be nothing but a collection of attributes. Water, to us, is a collection of attributes. Some attributes that a fish wouldn't know about are wetness (i.e., slipperiness between the fingers, evaporative cooling, the view from an air-to-water interface, difficult in breathing under it, drinkability, etc.). The human being can conceive of air without naming it. The fish (with a hypothetically advanced brain) can conceive of water on the basis of experiencable attributes (different, of course, from those making up the human conception). I don't think in terms of "abstractness," but in terms of levels of perception.

My objection to your "I do not deny that sentences have structure" concerned the word "have." I don't object when you say

>This structure is learned by attending to language as well as by
>attending to nonverbal perceptions and the correlation of nonverbal
>perceptions with language. Otherwise the arbitrary aspects of a
language would never be learned.

If sentences "had" structure, you wouldn't need to interact with others to find out what it is. You could take an isolated sentence or set thereof and directly experience its structure. My point is that structure (of whatever level) is a perception, both in an individual and in the individuals making up the surrounding culture. You find out what the other person intends as the structure of a sentence by using the test for the controlled variable: you try out variations based on different hypotheses about the structure, and look for resistance and correction, or evidence for or against comprehension, from the other person. In this way you converge to perceiving structures that are operationally equivalent to those that the other person perceives (whether or not they are in fact the same).

From me:
>>This is beginning to take more shape in my mind. Yours too?

From you:
>Yes -- but is it the same shape? And how can we tell? By the
>combinations of words we respectively are using in this dialogue.

.. and by trying different interpretations and seeing which the other resists or goes along with.

>For example, I am using "structure" collocated with "social," and you are not.

Now I am. But as a control theorist would, by considering HOW social interactions take place, such as:

[You may be] ... finding a way to reject a social aspect of linguistic structure and assert that it all arises from our control of nonverbal perceptions directing us how to put our words together. I don't know. But I'll try to guess, when I see your response.

Some of the nonverbal perceptions we control for have to do with other people: our beliefs about them, the systems into which we mentally weave them, our understandings of what they mean by their ways of speaking and acting, and so on.

It is easy to know when someone disagrees with (what is understood of) what you are saying. It is much harder to know whether agreement with what you mean actually exists (even after you hear "I agree"). Words are the best we can do toward letting another person sit where we sit and perceive as we perceive. But clearly they don't do this automatically or perfectly.

Gary Cziko (910731) --

The bathtub vortex is a lovely example of the kind of "self-organizing stability" that so many people confuse with control theory. The water isn't trying to get down the drain as fast as possible -- in fact, when it whirls, it goes down more slowly than before the vortex develops (centrifugal force creates a pressure gradient against the flow). When you put your finger into the hole, the spinning is killed by turbulence and friction precisely because no control system is involved: the water is still moving according to the dictates of external forces, and will make no effort to restore the vortex. If it were a control system, it would spin harder, overcoming the disturbance due to the finger.

The vortex appears organized because the human being looking at it is more interested in spin than laminar flow. Even in those terms, this organization is very weakly stable, as Rick Marken pointed out. See my post on the marble in the bowl.

Gary, thanks for the chapter. It looks, on first reading, like an admirable merging of control theory into the larger picture of evolution that you're developing. Very clear and full of nice illustrations (even without the figures). I can't wait to see the whole book.,

John Maag (910731) --

Got your note; thanks.

==========

Date: Fri, 2 Aug 1991 15:33:09 +0200
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Jean-Arcady MEYER <MEYER@FRULM63.BITNET>
Subject: A new journal

============================== Call for papers ==============================

A D A P T I V E B E H A V I O R

An international journal devoted to experimental and theoretical research on adaptive behavior in animals and in autonomous artificial systems, with emphasis on mechanisms, organizational principles, and architectures that can be expressed in computational, physical, or mathematical models.

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Submissions should be sent to:

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The title page (page 1) should have:

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- a daytime telephone number for the author with whom the editors should correspond.

The second page should contain an abstract of 200 words or less, a list of six or fewer key words, and a shortened title for use as a running head.

Begin the text of the article on page 3. Aid the reader by dividing the text into logical sections and subsections. Footnotes may be used sparingly.

Follow the text with acknowledgements on a separate page.

Begin the reference list on a new page following the acknowledgements page. References and citations should conform to the APA Publication Manual except: (1) do not cite page numbers of any book; (2) use the

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Date: Fri, 2 Aug 1991 07:44:58 -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: Mac for meeting

[From Bill Powers (910802)]

I have asked Roger Peters, co-chair of the psych dept at Fort Lewis College, if we can borrow his Mac. He says "no problem." We will get together with him on the 14th to make sure it's compatible with Clark McPhail's projection plate. If Rick brings a portable, we will have two Macs -- no waiting.

Bill P.

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Date: Fri, 2 Aug 1991 10:23:35 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: Martin's sample utterance

[From: Bruce Nevin (910802.07:45)]

Martin Taylor (Thu, 1 Aug 1991 15:44:01 EDT)

>"By saying something like 'delete my file' it's an action that in by saying
>it has been has been doen even if it's been done by a machine and not
>actually themselves."

Here's an attempt to reconstruct the intonations, pauses, and rhythmic breaks that you must also have heard on that tape, together with suggestions for five of the zeroings that of course could not be heard:

> By [its?] saying something like 'delete my file'--
> it's [saying to perform] an action that,
> in -- [I mean] by saying it has been [done]--
> [it's an action that] has been done even if it's been
> done by a machine and not actually [by the users] themselves.

A dash -- corresponds to a re-initiation of intonation, a re-starting of the phrase, sometimes with pause, but not necessarily as with the "in -- [I mean] by saying" repair. A comma indicates normal comma pause. If I heard the tape I could reconstruct these better, and justify them by the perceptual evidence.

Clearly, the issue is confusion in a program between machine's actions and user's actions. Without the context that speaker and hearers shared, I would guess that a program is issuing a confirmation message about deleting a file, and the user of the program might be misled as to precisely what was going to be deleted. With that context, I could reconstruct more zeroed words to get a more syntactically regular paragraph. I'm guessing that with the words 'delete my file' the speaker rephrased the confirmation message in terms of the question the user might ask about it ("Delete my file? Which file?). However, these words might also refer to a user command to delete a file. Not having the shared context, I don't know.

The re-initiations of the sentence as the speaker struggles to find a structure in language that accords with the structure in perceptions of user, machine, file, delete, action, done, saying, would remain. They are not "performance errors," they are important evidence about the process of creating an agreement between a nonverbal perceptual structure and a language structure, so as to say it in a form perceptible to the speaker's audience.

Bruce Nevin
bn@bbn.com

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Date: Fri, 2 Aug 1991 10:46:00 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@VMD.CSO.UIUC.EDU>
Subject: PCT and education

[From Hugh Petrie (910802)]

Joel Judd (910729), John Magg (910727), Rick Marken (910729), David Goldstein (910729), Bill Powers (910729)

Sorry I was not able to respond earlier to the interesting posts on PCT and education. I was helping get a grant proposal out by a deadline. However, most nearly everything I would have said has been said by others--and more eloquently, too. I would just simply add a couple of amens and suggest one additional implication.

Amen 1--To object to any scientific theory on the grounds that other theories also "explain", more or less, the same phenomena is simply to misunderstand the progress of science. OF COURSE, other theories, or empirical findings also apply to the phenomena, at least some of them. We wouldn't even consider them for a day if they didn't. Rick handled this very well.

Amen 2--Several folks, Bill, in particular, have explained very well the emphasis on perceptual learning which comes with PCT. This simply falls out of PCT, but also gives a theoretical basis for why the empirical phenomena of "imaging" seems to work so well as a learning technique. It also gives an absolutely straightforward explanation of all the observations of how increased expertise in any field seems to be a case of perceiving things differently--the chess master's perception of "I am strong in the middle". I have no idea what that perception would mean. The concert pianist's controlling the emotion with which the piece is played rather than the technical details. The expert teacher's "seeing" the need for review rather than inferring it from student behavior.

Amen 3--Motivation is, indeed, the difference between reference signal and perceptual signal. And, of course, people may fail to be motivated EITHER because they have no experience seeing certain kinds of things OR because they do not actually have reference signals for those things, which in turn may mean that they may not have seen that these lower order reference signals are ways of achieving the ends they do have. So the method of levels is appropriate. What Gary Cziko says, is also true, however. We cannot guarantee that any individual will reorganize in ways we find appropriate. The outcomes of education are not guaranteed.

One new item. Assessment looks very different under a PCT perspective. It essentially becomes a matter of saying to yourself as teacher, "If the student understands this concept, then if I introduce such and such a disturbance, it ought to be resisted." This is instead of assuming that we are "sampling from a preexisting set of responses," as if they are all ways of effecting outputs rather than controlling perceptions. Of course, since a disturbance to a controlled quantity can be resisted in any number of ways, we can never predict what a "right" or "appropriate" answer might be. This is why settings like doctoral orals are so much more satisfying than multiple choice exams. In the former, we can explore the various responses to see if they really do correct the disturbances. In the latter, the tester has predetermined what THE right answer must be. Once again, PCT provides a transparently effective criticism of multiple choice testing which, of course, could be criticized on other

grounds as well.

Hope this is helpful. I'm looking forward to Durango, although my schedule precludes my staying the whole time. See you soon.

Hugh G. Petrie, Dean
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Date: Fri, 2 Aug 1991 13:53:31 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: Language

[From: Bruce Nevin (910802 12:06)]

Bill Powers (910801.0900)

1. I have been resisting what I perceive as a rush to iconicity. Your use of "symbol" and "symbolize" sounds like iconicity. And in your examples, frequently, it appears that you expect there to be a 1-1 correspondence between each word and a corresponding perception on some level.

This works on the lower levels. For each concrete noun, there appears to be a corresponding configuration perception: lemon. For many intransitive (one-argument) or transitive first-order operators, where no argument can be an operator, there is a corresponding sensation, etc. And so on. But I believe there is also a perception that corresponds to the building up of multiple operators asserted of the same operator word(s) repeated through a bit of discourse, where no single word but rather the structural relations (repetition, parallelism with respect to operator-argument dependencies, parallelism with respect to word order after reductions, etc.) correspond to the perception of other relations, almost certainly not identical, but corresponding, among nonverbal perceptions. The structural fact of what is present in reduced or phonemically zero form corresponds to a perception of corresponding perceptions being so well known as not needing explicit statement. The ready availability of some reductions and not of others can make it difficult to access some perceptions: for many years, few noticed that the requirement in English to select one gender in the singular and the default to masculine was presumptive and prejudicial. Perception of that structural fact about English corresponds for many to perception of a pattern of sexism in our culture and in the choices and attitudes of individuals who control for conformity to it. And so on.

It is for reasons of this kind that I have kept coming back to the complexity of language. I recognize the need to start with something simple. But I think there needs to be some understanding of where the complexity lies in order to understand what constitutes "simple". The concept of a word-to-perception hash table just doesn't cut it.

2. I see that you are controlling for potential modifications to the model. I'm nowhere near proposing that. I'm trying to make what I know work in the existing model. The differences for language are differences of reference, not differences of kind. The same perceptual apparatus and the same kinds of control systems control language and perceive/impose structure in it as the ones that control nonverbal perceptions and perceive/impose structure in them. I do not believe (pace the Generativists and the "modularity of mind" buzz) that it is necessary to postulate a "language module" in the brain that is specialized in the sense of having basic capacities for perceptual control that other parts of the brain do not have. I believe there are areas of the brain that are specialized not in kind, however, but in reference: the perceptions that they control are perceptions of the objects and relations in language. Thus, Wernicke's and Broca's areas, damage to which affects control of language. As I said:

>>Language has its own structure, instantiated through control of
>>perception of phonemic contrasts etc., and corresponding to structure in
>>other perceptions (in the control hierarchy), but not identical with or
>>entirely merged with it. This structure persists (with variation) when
>>you examine utterances of one person or another, and it persists (with
>>change) through time.

This, which you identify as the basic issue, does *not* necessarily entail modification to the HCT model. However, work with the structures in language may disclose the need for modifications, and I too would expect any modifications not to be specialized for language.

3. You object to my saying that sentences and indeed language "have" structure, independent of my perceiving that structure. This is sticky because we are talking not about one individual's perceptions but about ways-of-controlling-perceptions that multiple individuals share. If one individual dies, or forgets, or goes away, the language still exists. It is still there for another person to perceive, for a new child or immigrant to learn.

This is in some respects analogous to any physical object that I might perceive. If I go away from this office and never return, it is still here for someone else to perceive, to move in to and use.

The analogy fails in that language exists *only* in the perceptions of its users. To anyone else, it is a collection of sounds, or marks, or an untelligible language, but not the language that the language users perceive. The difference is in being able to recognize (perceive, remember, associate) and use the objects and relations in it: contrasts, syllables, morphemes, words, operator-argument dependencies, reductions, discourse structures, and in being able to correlate these linguistic perceptions with nonlinguistic perceptions. (To revisit briefly: the correlating of one perception with another is "symbolizing". But not all the correlata on the language side are single words.)

You say:

>If sentences "had" structure, you wouldn't need to interact with others

>to find out what it is. You could take an isolated sentence or set
>thereof and directly experience its structure.

I don't know what you mean by "having structure". Supposing sentences "had" structure in your sense. How would you "directly experience" its structure? Conversely, of what nonverbal perception could you say that it "has structure" in your sense, and how does one experience its structure directly? This is not a challenge, but a request for clarification.

>you try out variations based on different
>hypotheses about the structure, and look for resistance and correction,
>or evidence for or against comprehension, from the other person. In this
>way you converge to perceiving structures that are operationally
>equivalent to those that the other person perceives (whether or not they
>are in fact the same).

I believe this describes how one learns the structure of the language. It's a good start at what I do when I sit down with an elderly speaker of Achumawi. However, you are concerned with the correlation of a *particular* sentence with a particular set of nonverbal perceptions. You introduced the above with:

>You find out what the
>other person intends as the structure of a sentence by using the test for
>the controlled variable:

With this preface, the same paragraph describes how we determine the structure of a sentence, if that sentence is structurally ambiguous--that is, if more than sequence of reductions could have had the same result. Also the process of determining higher-order relations across the sentences of a dialogue, which is what we are more often unclear about.

The second process--interpreting a given utterance--is a process of using the structure in a sentence to reach agreements about nonverbal perceptions to which it corresponds. This second precess *depends*upon* the first: it depends upon prior knowledge of the structure of the language, which the given sentence only in very small part exemplifies. This is like the information-theoretic thing, where the sentence has meaning only by contrast with all the other possible things that could have been uttered. The range of possibilities (this operator word and not that one, this argument word and not that one, this reduction reflecting that some words other than these are focalized, etc.) constrains and guides the process of reconstructing the operator-argument relations and the "binary array" of discourse classes and relations. The process of bringing one's perceptions of these structures in the sentence into congruity with nonverbal perceptions also constrains and guides the process of disambiguation and construal. There is a lot of controlling for congruity going on here, in multiple directions in parallel.

For an individual speaker, the structure that is in the language is there because it has been learned and is remembered as it guides the process of construing what another is saying or putting together an

utterance to correspond to some remembered or imagined perceptions (probably not real-time perceptions?) This structure pre-existed when she learned the language by perceiving how others controlled their perceptions for conformity with it, and by testing that control (hers and others') in various ways. This is some of why and how I say that there is structure in language, and that a language has that structure.

In summary: One cannot experience the structure in a language "directly," because one cannot perceive it all at once. (You didn't suggest that one could.) The processes of test and evaluation that you describe are precisely the way that a person can experience the structure in a language. They are also the way that a person can determine which of the possible structures in the language is the intended structure of a sentence. However, without knowing the structure in the language a person cannot perceive the structure in a sentence, or even that it is a sentence.

4. This is why I have been pushing the perception of language as a social reality, not just a psychological reality. Yes, there can be no society without individual psychologies. But reductionism doesn't fly well in either direction. Like it or not, accomodating language in the model means accomodating social facts as individuals learn them and control for conformity to them.

5. I too believe that Chomsky is really looking for a level of perception. I don't think he believes that. According to his statements, he is looking for evidence respecting a postulated biologically-innate language module that constrains linguistic performance. Performance, which he certainly does not see in terms of the control of perception, is avowedly uninteresting to him, except insofar as it provides indirect evidence that bears on this hypothesis. "Level of perception" would I think sound to him like something for the "performance module".

Enough. I will have to be terse next week. Or silent for a while.

Bruce Nevin
bn@bbn.com

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Date: Fri, 2 Aug 1991 14:32:59 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: Signoff & draft 2

[From Kent McClelland 910802]

Gary Cziko

I need to sign off the net temporarily, because I'm leaving town tomorrow to vacation in Maine for a couple of weeks before coming to Colorado. There is now an updated draft of my PCT & Sociology manuscript, which I will bring along to Colorado. I could send you an electronic copy of the manuscript, but it was already lengthy and has grown by about a third, so maybe we can put off trying to distribute it electronically until after the conference. In the meantime you should probably delete the first draft. I'm sending out

a few paper copies of the second draft soon to people who were kind enough to give me extensive comments on the first one.

I've only had a chance to skim your chapter, Gary, but it looks like a very nice job. I wish my treatment of the cruise control example was as clear and concise as yours.

Rick, Bill

Delighted to hear that we'll have Mac access in Durango. I'll stick a diskette in.

See you in a couple of weeks!

Kent

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

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Date: Fri, 2 Aug 1991 14:59:26 -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: Vacation Time

[from Gary Cziko]

For some reason my family wants to take me away from all the fun I'm having writing and thinking about perceptual control theory and participating on CSGnet. They call it a "vacation." I'll be away from now until after the CSG meeting in Durango ends on August 18.

I hope that CSGnet can last for two weeks without me.

For those of you also taking a break and not wanting CSGnet to flood your mailbox while away, let me mention again how to take a break.

Send the following message to LISTSERV@UIUCVMD.bitnet or LISTSERV@VMD.CSO.UIUC.EDU

set csg-l nomail

Then when you come back, just send the command

set csg-l mail

If for some reason this doesn't work (the listserver tells you that you are not on the list to begin with), it is probably because the return address on your command message does not match your address on the list. You can find your address on the CSG-L list by sending the command

review csg-l

, then you will need to make sure that the return address on your command matches that on the list.

Remember to send all comands to the LISTSERV, not to CSG-L.

I'm looking forward to seeing many of you in less than two weeks in Durango.--Gary

==========
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Date: Fri, 2 Aug 1991 17:59:59 EDT
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: limits of language

[Martin Taylor 910802 1755]
(Bruce Nevin 910802 0925)

>
>The operative words here are "can be" and "capable of". The constraint
>is that the grammar must be statable in the metalanguage that the
>language contains as a sublanguage.

That's the claim to which I was objecting--the idea that the operative functions of a *natural* language can be described using a grammar that can be expressed in the language. I don't think anyone has come close to succeeding in discovering such a grammar in 2000 years of trying, and (personal opinion) I think that the difficulty is intrinsic.

Later in the same post, Bruce asks whether my reference to a bare skeleton of language means the grammar of a language; yes, that's what I mean.

I can't answer all of Bruce's long post here. I'm just trying to catch up on today's messages before going on a 2-week vacation (458 messages to read on my return!). Suffice it to say that I agree with much of it, and in particular with the emphasis in many postings about each partner controlling for desired actions (responses) in the other. That's the central feature of my Layered Protocol theory of communication, which I still hope to summarize and send to Gary some day.

Martin Taylor

==========

Date: Fri, 2 Aug 1991 18:18:16 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: Language; vortex

[Martin Taylor 910802 1815]
(Bill Powers 910801 09000)
>
>If sentences "had" structure, you wouldn't need to interact with others
>to find out what it is. You could take an isolated sentence or set
>thereof and directly experience its structure. My point is that structure
>(of whatever level) is a perception, both in an individual and in the
>individuals making up the surrounding culture. You find out what the
>other person intends as the structure of a sentence by using the test for
>the controlled variable: you try out variations based on different
>hypotheses about the structure, and look for resistance and correction,
>or evidence for or against comprehension, from the other person. In this
>way you converge to perceiving structures that are operationally
>equivalent to those that the other person perceives (whether or not they
>are in fact the same).
>
Beautifully put. The so-called "grammar" of sentences is what people
conventionally do in determining the interrelationships and intended
functions of words. It isn't inherent, it's always changing in various
ways (the control system that stabilizes the use of language is as
strong as the Academie Francaise) like the self-organized structure
it is. It's different depending on who is using it, to whom, and when.

Everyone interested in the use of language (rather than the mathematics
of fictitious models of language) should take to heart the middle of
Bill's paragraph.

Martin Taylor
==========
Date: Fri, 2 Aug 1991 18:37:36 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: Martin's sample utterance

[Martin Taylor 910802 1820]
>(Bruce Nevin 910802 0745)
>
>>"By saying something like 'delete my file' it's an action that in by saying
>>it has been has been done even if it's been done by a machine and not
>>actually themselves."
>
>Here's an attempt to reconstruct the intonations, pauses, and rhythmic
>breaks that you must also have heard on that tape, together with
>suggestions for five of the zeroings that of course could not be heard:
>
> By [its?] saying something like 'delete my file'--
> it's [saying to perform] an action that,
> in -- [I mean] by saying it has been [done]--
> [it's an action that] has been done even if it's been
> done by a machine and not actually [by the users] themselves.
>

A valiant attempt. But my point was in part that the listeners did not even notice a problem with this production, which, when written down is at least as bizarre as the one that started this thread, which "would never be spoken by a child."

The grammar of written text, if one is ever discovered/invented, will not be at all like that of speech (an overstatement, but not a gross one). But I think the underlying control-theoretic principles are the same, the differences being two-fold: (1) The talker and listener operate synchronously whether or not the listener has the opportunity to provide feedback to the talker, so that any transient environmental influences are available to each, known to the other, and (2) spoken language uses information channels not available to the writer, such as timing and other intonation parameters, and body language.

Actually, what he was saying was that when you instruct a computer using a command of the type "delete my file" the perception is that you are deleting the file in the issuance of the command, even though the deletion is being done by the machine, not by you.

>
>The re-initiations of the sentence as the speaker struggles
>to find a structure in language that accords with the structure in
>perceptions of user, machine, file, delete, action, done, saying, would
>remain. They are not "performance errors," they are important evidence
>about the process of creating an agreement between a nonverbal
>perceptual structure and a language structure, so as to say it in a form
>perceptible to the speaker's audience.
>
In principle I guess that's right, but if the aliteral quality of the utterance is used by the listeners as clues to the mental processes of the talker, this usage is non-attended. What comes across to a listener is a clear, well-formed picture of the talker's intent.

Martin Taylor

==========

Date: Sat, 3 Aug 1991 10:36:11 -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, Misc.

[From Bill Powers (910803)]

Gary Cziko (910802) and others puzzled by my weird sense of humor which gets out of control now and then --

I didn't really expect anyone to be shocked and horrified. I was -- let's see, how does this work? -- I say that everyone would be shocked and horrified that I was having this little problem with alphabetizing as a way of acknowledging that expecting anyone to give a hoot was itself a gross exaggeration. If you follow that you're doing better than I am.

Mucho thanks for that editing job -- now I can sort on last names and have a list in which I can look up addresses. I promise not to make any

more confusing remarks, unless I get confused.

Bruce Nevin (910802) --

>Bill, how does the control system know that the classes of nonverbal
>experiences are rare? Whatever your answer, why does it not apply
>equally to verbal experiences?

Rare experiences are experiences that few people have or that individuals have only rarely. It isn't necessary for the individual to know how rare the experience is in general; if the experience turns up, the individual can refer to it with words, partially: "Hey, your aardvark is greening!" (This is one of those animal-shaped planters coated with seeds). This will not happen very often, and therefore the expression referring to the event will not occur often. The linguist who is collecting statistics only about word-usage, and not about meanings, will get the impression that exclamations like the above must violate some obscure linguistic rule because they hardly ever occur. All I'm saying is that some apparent rules of word-usage distribution may be traceable to the commonness of the experience to which they refer, and are not rules at all.

> Aside: how hard is it to model coordination of parallel control? Some
>meta-perception required?

Yes. Rick could show you with his spreadsheet model if you were coming to the meeting. Curses.

>You want to derive the verbal likelihoods from the nonverbal. Are the
>nonverbal perceptions privileged in some way admitting of likelihood
>judgements precluded to verbal perceptions?

This seems to be the main topic today. See above.

> .. the linguist can sit down with people and ask, for pairs of
>utterances, whether they are different as to their likelihood.

Do the linguist and the subject interpret "likelihood" the same way? The linguist is thinking in terms of some hidden rule that applies, in every individual, directly to the pair of utterances strictly as strings: he thinks of likelihood as the result of a rule tending to forbid or require certain combinations, in the linguistic machinery of homo sapiens. But the subject isn't thinking that way. A combination that doesn't make sense ("Put it red") doesn't make sense because the meanings don't jibe: the subject can't imagine an act like putting being applied redly or in a red direction or whatever. So the subject says, "No that's not likely." This has nothing to do with the subject's knowledge of what combinations are likely over a population or even in that person's own linquistic habits. The subject simply can't imagine a coherent experience to go with the sentence. The experimenter might come up with some clever rule that happens to work, but this doesn't show that a rule operating inside the subject has anything to do with why the sentence is rejected. The experimenter and subject are talking at cross-purposes.

If all instances of "rules" that are really experiential rules are eliminated from linguistics, those that remain valid should be much more

clearly conventional: for instance, word order in Joe hit Mac.

>The nonverbal perception of pounding with string is much harder to
>imagine than the nonverbal perception of pounding with a hammer or
>something of category "hammer". Why? (If the only tool you have is
>string, every problem is bound to look like a package.)

Because when you imagine picking up a piece of string and pounding something with it, the string is too soft, it flops around, it doesn't weigh enough; you conclude that the nonverbal model described by the sentence won't run. You take the sentence at face value and try to make it happen in your head, as a working model. Your own experience fills in what the sentence doesn't say, and you find that this model is inconsistent with your understanding of how strings and pounding work. It isn't that you can't understand the expression; it's that you can't make sense of it.

>What is happening when I refer to a perception on some other level?

>   * a specific intensity ("yes, the second one is definitely brighter"
>   "Ow! That's too loud!")
>   * a perception (yellow) [SENSATION goes here; perception's generic]
>   * a configuration (lemon)
>   * a transition (turning)
>   * an event (a toss)
>   * a relationship (material for lemonade)
>   * a sequence (steps to make lemonade)
>   * a program (what to do if no sugar)
>   * a principle (have contingency plans)
>   * a system (making things with Sarah helps our marriage)
>   * a conversion/paradigm-shift (this new definition of quality time
      makes sense)

>Categories mapped onto each of these? You may be right, that all the
>"meta"-ness of language is in a capacity of the category level to apply
>to any perception whatever, but that feels like shifting the problem
>under a different rug.

I went around and around on this before I saw the "right" answer, at least for the levels below category. Good old block delete.

Words like "yellow" refer to categories, not color-sensations. If I say "Yellow" you can ask "Like my shirt? Or that bowl over there? Or this cantaloupe?" and I will say, "No, not cantaloupes, they're orange." It is very hard to designate in words a PARTICULAR intensity, sensation, configuration, transition, event, or relationship: that shade of yellow right there that I'm looking at, and not the other one almost like it. It takes a lot of words to eliminate the categories you don't mean. Consider the relationship called "above." Hold your left hand above your right hand. You can say "You mean JUST above, like this, or WAY, WAY above, like this? Do you mean above and also vertically over? Do you mean palm up or palm down or in a fist, or what?" Some categories are fairly specific, but a signal designating a category can't represent differences

among its members.

I think categories are arbitrary. The category of "useful things" is formed out of things you have found useful, like screwdrivers and automobiles and elevators.

When you go UP from the category level, we're talking about something else. A word like "principle" doesn't have any low-level referents other than the word itself (or verbal definitions of it which are also words). We call words like this "abstract" because of the lack of concrete nonverbal referents. These words are being used by higher-level systems as an indirect means of pointing to a perception of higher order. In order to refer to a particular principle, we must give examples of it: "Look before you leap" evokes a way of acting in which we can see a principle, but the way of acting itself is not a principle. That is, if you look before you leap (a relationship between two events), you will be demonstrating the same principle exemplified by "Don't cross the street before looking both ways." A phrase like "Be careful" brings to mind ways of doing things, and in these ways you can sense a principle, a generalization about behaving. But the principle itself, I think, is a nonverbal perception.

>I ask again: are we growing these neural connections when we learn?
>When we establish a long-term memory? I am ignorant.

Ask Joe Lubin. I seem to recall a Science News article during the past year in which someone saw significant changes in [rabbit?] dendritic connections over a space of five minutes.

Don't send me more stuff to read just now. My overload button will pop.

Rick Marken (910802) --

Good about the Mac. I have a promise of another one from Roger Peters, co-chair of the Psych Dept at Ft. Lewis College -- as I already said. We should be in good shape.

Bill Silvert (910802) --

Don't give up -- just start a new thread. There are probably lots of people listening who are waiting for something more interesting to dominate the discussion for a while. Marine ecology sounds interesting.

Martin Taylor (910802) --

You're making lots of sense to me -- I'm starting to drown in this stuff. I think I get hung up inside the language framework and start to lose track of the way in and out. You provide some very nice escape hatches.

--- & Bruce NEvin (910802) --

>One can use language to describe anything, including what one does with
>language.

I think I take exception. My posts are time-lagged, so I don't know if

you have already seen my distinction between language referring to an experience and describing an experience. I don't think that language can describe everything we experience.

Further, saying that

>Therefore the grammar of a language must be capable of statement in the >language itself, that is, in its metalanguage, given if necessary >invention or adaptation of the required small vocabulary.

... makes a very strong assertion, which is that someone has in fact captured the entirety of grammar in language. I don't see why the grammar of language MUST be capable of statement. All that MUST happen is that grammatical control of statements be exerted by neural organizations. Rules can be built into circuitry without being expressed in words. One can, of course, make an attempt to express them -- but the expression, if in words and not circuit diagrams, will be in the wrong language.

>Goedel's theorem says that not all propositions are decidable. You'll >have to explain the connection. [Bruce]

It occurred to me that the proof of Godel's theorem also assumes that every combination of symbols must assert some proposition. In the English language, it sure isn't true that if you take the first word from one sentence, the second from a different one, and so on until you have exhausted all sentences, that you will end up with a statement that means something, much less something that is either true or false. Most sentences formed in this way would be gobbledykgook.

It also occurred to me that when you two (martin and bruce) get to talking about language, the quality of the discussion is improved when I stay out of it.

Last thought:

Bill Silvert, from your expanded notes about your interests, I judge that we had better talk first about what you mean by control theory, before assuming that we are all talking on the same subject.

Best to all,

Bill P.

==========

Date: Sat, 3 Aug 1991 12:07:09 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Tom Hancock (via Ed Ford)" <ATEDF@ASUACAD.BITNET>
Subject: PCT/education:linguistic processing

Subject: PCT in education/training: language processing, memory use--modelling?) Tom Hancock (910803)

LET ME INTRODUCE MYSELF
I was at last summer's CSG meeting. I have read CSG and Powers

materials for 40 or so hours stretched over the past year (mainly this summer). I have been on the CSG net for about a week.

MY RESEARCH CONCERN

I am presently trying to appeal to HCT to help (or direct) modelling and explanation of the cognitive processes operative in computer based learning environments in three training/experimental domains: college students responding to multiple choice semantic knowledge questions, Air Force trainees (and college students) doing radar recognition drill tasks and performing air intercept simulation part-tasks.

DATA GATHERING

I am using data along the line of the following:

1. response certainty ratings (and possibly other metacognitive judgments: feelings of knowing, judgments of learning, ?"remembering that"?, etc.). For example, immediately after responding (or at some delay--see Nelson in this month's Psychological Science) in an interactive computer environment, the learner/subject is asked to rate how certain he is of the correctness of his response. I suspect this rating may be indicative of a subject's judgement of the amount of error generated from the system which is controlling for meaning or correctness--presumably by means of monitoring lower (or higher?) system's memories as they interact with the perceptions activated by the initial question reading. (More complete thoughts on control processes of memory, as viewed by some information processing perspectives such as ACT* will hopefully be sent out later.)

> (An aside: Perhaps high certitudes occur as activated memories and inferences have organized themselves into stable attractors?--a la fractals or CSG talk on self-organizing systems. I wonder if it is possible to sensibly understand human symbolic-laden-functioning in terms of the control mechanisms operating on chaotic dynamical system-like processing.)

2. response latencies (reading speed and response times)--presumably indicative of some generic control processes (but I am wondering about specifics);

3. objective response correctness (dichotomous or by degrees)--which interacts very informatively with the potential extra-organism induced disturbances (I assume memories can serve as disturbances in the inward environment) of informative feedback or post-response information. By the way, in the training/education environments of the Air Force, objectively correct responses (and prediction) are very important.

4. Feedback or post-response information. Quite likely to initiate a disturbance.

5. Feedback processing time--which should increase as discrepancy increases (as I indicated at CSG last summer, and tried to demonstrate on normed data using a gross discrepancy measure).

(6. And the above continued to be measured over more than one trial.)

YOUR CONTROLLING

As I write this I am hoping that you, the reader, access enough sensible perceptions and respond without being exited due to violations of the accepted methodology or due to my own incomplete or inaccurate PCT understandings. (I am planning to try work that is more tightly constrained by The Test. But up to now, old data is all I am using! And time available for reading PCT material seems to be beyond my control.)

YOUR HELP

If my approach seems unfruitful please let me know! None of us wants to come to the end of life's journeys having barked up the wrong tree. If you can help direct me to specific portions of the CSG readings (I have many of them) which would address some specific foci, I would be indebted to you. Or if you care to support some arguments, or to point out the inconsistencies I exhibit, or to try to help me reorganize my perceptions, please feel free.

MY CONTROLLING

As I do this work I am personally controlling for: 1. fits with my novice perceptions of control theory and my persistent memory attractors derived from current cognitive psychology; 2. potential applicability to air crew training and education in general, and specifically to forming something that approaches a truly intelligent computer tutor system that is sensitive to each learner's inferred cognitive states (ie. controlled perceptions and efficacy of memory use) on a frame by frame basis (this specific application being the avowed purpose of my present short term research grant); 3. perceived potentials for generating money for continued research along this line (this first research grant has given me some new reference standards regarding the way to spend my life). (4. And I have a control system at a somewhere level which from time to time seems to need large quantities of feeling or perceiving that I understand the elusive--like most of you I presume.)

QUESTIONS ABOUT SOME DATA

I have a question along the line of my present research interests. -----What should the varied reading speed of a student answering multiple-choice questions indicate about possible control processes that might be operative (assuming the student is controlling, at one level, for answering the question correctly)?----- For example, I have noticed that even though a student gets a question correct on a test, and is correct again on a one hour delayed posttest, the questions that are eventually missed on a one week delay test are those that were initially processed longer than those questions that are again answered correctly. (For example, Jon answers a question at 3.5 words per second ((response latency from display of the screen until answer-choice key stroke)), gets it correct ((College Board Achievement judged correctness)) and on a 15 minute delay and one week delay test gets it right again; but

also Jon answers a question at 2.5 wps gets it right twice in a row but eventually misses it.) And this phenomenon seems to appear across all levels of the subject's initial confidence in the correctness of initial responses (certitude ratings of 1 --random guess to 5--absolutely certain). But contrariwise, a student who has missed an original question but on subsequent testings (following a display of the correct response) gets it correct, compared to a student who subsequently continues to miss the question, has more often than not taken longer to answer the original question. (Regarding perceptions about the use of multiple choice questions, unless I am an unprepared examinee, I, too, prefer oral exams to multiple choice questions! But I am trying to make a living doing research.)

It may be that the more control levels that a student must traverse (or co-process?), the more likely it is that she will take longer with the associated task; and it appears that as there is more discrepancy in a symbol-laden control system, there will be more processing to reduce that discrepancy. And in current theories of human information processing, such as ACT* (J. R. Anderson), it is argued that as stored information is unrelated, processing should fan out and take longer, but as information is integrated (ie. organized) then the processing should be more rapid. BUT I would certainly like to reduce my own task related errors (see "my controlling above") and fit all this together! Can anyone help?

SOLICITATION OF COMMENTS REGARDING MODELLING or seeking correlations between inferred control states and observable behaviors.

(BILL POWERS, TOM BOURBON, RICK MARKEN, JOEL JUDD, ET AL: you must have many hours available in your day if you carefully read all this net mail!) I am presently thrilled by the possibilities.

BILL AND OTHERS: What do you think about the following?

MEASURING CONTROL STATES

1. The feasibility of treating a metacognitive judgement such as certitude as a measure of discrepancy at the level of a system controlling for meaning construction or correct answering (or at other levels depending partly on how the judgement is asked for.)

2. Treating response latency--such as reading rate or signal detection rate (such as in the training/experimental tasks mentioned above)--as an indicator of varying states of a control system (possibly related to monitoring memory inputs from several levels and "averaging" or "fuzzy processing" them). Depending on the response times that are separated into categories according to subsequent response patterns. For example, with persistent correct responding the initial control state of the systems involved might be: a. (rapid responding--ie individual normed z scores that are extreme) well formed/automated control systems efficiently converging on organizations of remembered perceptions that

successfully reduce discrepancy at the level controlling for correct responding; or
b. (longest response latencies /slowest words per second rates) competition between control systems monitoring the adequacy of integration of memories and/or systems that are not accessing many previous perceptions, memories; or
c. (intermediate response rates) controlled systems that take time to "search" for relevant activated memories but find none and thus have no memory input with which to reduce discrepancy in this organization seeking and perceived correct answer seeking control systems.

3. Using plausible (I cannot mess with my subjects) feedback information as potential disturbances.

4. Using feedback processing time (study?) as a measure of discrepancy reduction attempts--the success of which would be determined by the experimenter (or computer intelligent tutor system) by means of later correct responding, improved response latencies, and change in certitude ratings. For example, an individual who has longer feedback frame processing times is more likely to correctly (and perhaps more rapidly--but this I have not determined yet) respond later. Thus, discrepancy indicated by original processing times and certitude ratings of self-perceived error, would have been reduced.

MODELLING?

5. Does there seem to be anything to combining these measures to form an iterative function using something along the lines of the following: RL = RT - C, where RL equals some state of the reference level(s), RT equals a measure of response time that may be indicative of the amount or adequacy of perception?, and C is a measure of response certitude, the subject's perception of the error in the system controlling for correctness, subsequent to the reception of messages from activated prior perceptions---BILL, I wonder about these particular attempts at quantifying, since it has been stated something like: the difference between a reference level and a perceived input equals error output; or that a reference level is some function of the difference between the reference standard and the perceived input. How about RL = f(RT, C)?

One might investigate, for example, by assuming no error output from the appropriate memory access and meaning construction systems, when the outward response continues to be correct and the subject continues to rate her certainty as high, and the response times are at individually normed extreme z scores (rapid responding). Or contrariwise one might set the RL (at the memory access level) at zero when the subject continues to respond incorrectly, etc. (and yet is still focusing on the task and appears to be trying to learn). Or when instructional feedback has no observable affect we can assume that a measure of the reference level (I'm not sure what kind of measure it is!) is at zero. This latter state appears to be the case when a subject has responded with high certitude correct answers and giving him

feedback more than simple response verification has no effect
(in terms of changes in my basic measures above).

Signing off. This started out to be my first quick, brief foray into the network! You are dedicated indeed if you have read all this.
Well, now I need to figure out how to send this. I'll go over to Ed Ford's house.

Tom Hancock
14210 N. 56th Pl
Scottsdale, Arizona
85254

College of Education
Grand Canyon University
3300 West Camelback Rd.
Phoenix, Arizona
85017 tel: 602-249-3300 ext. 261

Until August 22 (then its back school-teaching/advising/committees/etc):
Human Resources Lab/Williams Air Force Base (tel: 602-988-6561 ext 261)
E-mail: HANCOCK%HRLOT1.DECNET@HQHSD.BROOKS.AF.MIL
==========
Date: Sun, 4 Aug 1991 07:42:14 -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: Learning CT

[From Bill Powers (9108.04a) --

Tom Hancock (9108.03a)

>I am presently trying to appeal to HCT to help (or direct) modelling and
>explanation of the cognitive processes operative in computer based
>learning environments in three training/experimental domains: college
>students responding to multiple choice semantic knowledge questions, Air
>Force trainees (and college students) doing radar recognition drill
>tasks and performing air intercept simulation part-tasks.

Control theory doesn't adapt very well to experimental situations that have been set up on the assumption of a stimulus-response model. The first thing that has to be done is to recast the situation as a control process -- finding out what the subject is controlling and then figuring out ways of measuring the parameters of control. As Hugh Petrie pointed out (910802), the multiple-choice format assumes that we are measuring responses; this is not an effective way to find out what a student has learned (although it gives testers a comfortable feeling of knowing what was learned). Radar recognition drill tasks are organized (like most drill tasks) around the idea that the visual stimulus is to produce a recognition response as automatically as possible. I don't know what the air intercept simulation part-tasks are, but the concept of a "task" is

usually also organized around the production of outputs, responses, rather than controlling perceptions. In recognition tasks there is some element of studying perception, but the role of perception most likely is misconceived as a cause of behavior.

>I am using data along the line of the following: 1. response certainty >ratings (and possibly other metacognitive judgments: feelings of >knowing, judgments of learning, ?"remembering that"?, etc.).

One point of using control theory is to get away from statistical studies in which experimenters are jubilant (typically) over correlations as low as 0.8. Facts that are determined statistically are true only of a population and are next to useless for predicting the performance of an individual. There is a tendency to elevate findings that are true only of a majority of a population (say, 60 percent of subjects) so that they are assumed true of the whole population. So you end up concluding "Pilots with short reactions times are best in combat," where in fact that might be false concerning a pretty large number of pilots (for example, those who aim carefully before firing).

In your findings about the relationship between delay time and long-term objective response correctness, was it true of ALL subjects that longer delays went with better long-term correctness? If not, the results would have little meaning to a CT modeler, because he or she wouldn't know when to apply the model to an individual and when not to. Unless your results are remarkably more reliable than those of most statistical tests, there isn't going to be much that CT could say about them.

>If you can help direct me to specific portions of the CSG readings (I >have many of them) which would address some specific foci, I would be >indebted to you.

Control theory is a general theory, a new approach to understanding human behavior from the ground up. What you're asking is rather like asking what parts of mathematics you should study in order to solve a particular problem. Can I skip learning addition, subtraction, and multiplication if all that's called for is division? The answer the control theorist would give is the same one that the mathematician would give: learn the subject; then you can figure out your own answers to specific problems.

The learning job is more difficult with respect to perceptual control theory, because no one has yet written a text that starts at the beginning and develops the ideas as one would do for a college course. So much of the work is still "in progress," and so much effort still goes into differentiating control theory from other approaches, that writing such a textbook would be impracticable now. Dick Robertson's "Modern Psychology" (CSG Press, Gravel Switch, KY) was a first try at doing this. There will be more.

You can't learn control theory in 40 hours spread over a year. The material available isn't organized well enough to permit that. And anyway, PCT isn't just another drop in the bucket of human knowledge about behavior. It's a new bucket.

Patience and persistence.

Best,

Bill P.

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Date: Mon, 5 Aug 1991 07:02:42 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: limits of grammar

[From: Bruce Nevin 910805 7:03]

(Martin Taylor 910802 1755)

Martin,

If you distinguish between a description that accounts for the informational capacity of language from one that accounts for its communicative uses, the former is what I mean by grammar and the latter is I think what you are objecting that a grammar such as I describe cannot include. As to whether one has been created or not in the past 2000 years, please do look at references cited for operator grammar. (And of course you have to go back more than 2000 years for Patanjali's language-as-object-of-meditation grammar of Sanskrit, which might also qualify.)

This comes close to the Generativist distinction between competence and performance, without the hypostasis and without the rejection of performance *data*.

Bruce Nevin
bn@bbn.com

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Date: Mon, 5 Aug 1991 07:42:48 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: Bill's overload button

[From: Bruce Nevin 910805 7:48]

Bill,

You asked for some more re the Hopi and Whorf. I will be sending that.

I also batched various responses this weekend. My intention was to save most responses for next weekend. That should slow the carousel to fewer horses per unit time, with hopefully less baggage per horse.

So prepare some buffer space, those files are coming. I'll try not to overrun the buffer.

I did fire off an impulsive response to Martin this morning. I'll have to follow that up with a clarification. Should have known better to

than to respond to one without having first read through all of today's CSG messages.

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Date: Mon, 5 Aug 1991 09:39:51 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: followup to Martin

[From: Bruce Nevin (910805 0803)]

My first reply was too quick. I think we agree that grammar (the skeleton of language, if you will) does not account for communication and other uses of language. My concern is the converse one: that control theory cannot account for communication and other uses of language without control of the objects and relations in the language--its grammar. This is something in addition to control of the perceptions to which utterances may refer. Why do I insist that certain perceptions (objects and relations) are in the language? Because the language is a given in the world when a child comes to learn it, just as the characteristics of the playroom and toys are a given. The fact that the structure in the sounds and marks by which we know that language is happening are due to the control of perceptions by others in no way sets it off (in the point of view of the child) from these other artefacts, which also were made and arranged by people controlling their perceptions. Cleaning up the playroom is an adult activity with the toys in the space. Talking is an adult activity with the objects and relations in the language. The toys, the space, the objects and relations are pre-existent givens for the child.

[Martin Taylor 910802 1815]
>(Bill Powers 910801 09000)
>>
>>If sentences "had" structure, you wouldn't need to interact with others
>>to find out what it is. You could take an isolated sentence or set
>>thereof and directly experience its structure. My point is that structure
>>(of whatever level) is a perception, both in an individual and in the
>>individuals making up the surrounding culture. You find out what the
>>other person intends as the structure of a sentence by using the test for
>>the controlled variable: you try out variations based on different
>>hypotheses about the structure, and look for resistance and correction,
>>or evidence for or against comprehension, from the other person. In this
>>way you converge to perceiving structures that are operationally
>>equivalent to those that the other person perceives (whether or not they
>>are in fact the same).
>>
>Beautifully put.
>. . .Everyone interested in the use of language (rather than the mathematics
>of fictitious models of language) should take to heart the middle of
>Bill's paragraph.

Traffic laws exist. People obey them, bend them, disobey them, but it

is the rare and short-lived driver who does not factor them in to the setting of reference levels and the control of perceptions involved in driving a car. Drivers lean on this externally-imposed system of rules to help them anticipate what other drivers will do, and to stay clear of unpleasant surprises.

The structure that is in language is something like that: a socially established standard to which the conduct of communication with words is always oriented, whether strictly followed or not, and on which communicators lean for added redundancy as they construe what the other intends.

But grammar is not like traffic laws, though the Academie Francaise (and such as William Safire) tries to make it so. Grammar is more like the convention by which you know the appropriate distance apart for you to have face-to-face conversation. This distance is very close for Arabs, Latin Americans. Their comfortable distance is for us too close for comfort. Edward Hall describes amusing scenes of persons of one culture chasing persons of another around a party, each controlling for a comfortable "appropriate" conversational distance. (Easy to put that in the "crowd" program!)

So far as an individual learning how to get along is concerned, social conventions exist in the coordinated, agreed-upon outputs of other human control systems, and in corrections that others give when you test for them. Call them part of one of your "protocols" if you wish. They are learned by children and immigrants as they learn to participate in the local culture (learn the details of its protocols). Unlike traffic laws, the vast majority of them are not written down and most often are never even brought to awareness, except by such as anthropologist, ethnologists, and linguists. The ones that are noticed in rules of etiquette or in the grammatical rules of Miss Grundy or L'Academie Safire are shibboleths: differences of conduct that are noticed precisely because by them we can distinguish members of a privileged in-group from non-members.

The shibboleth-maintainers do not recognize that language changes (a point with which I certainly agree) as well as varies. To be consistent in his logic, a Safire should be bewailing the loss of the case inflections of Old English. (Or IndoEuropean, or earlier stages.) But of course these language commentators are not bewailing and resisting change. They are putting those who speak differently in their place.

There is another aspect, too. Human cultures are very elaborate, very beautiful, and very different from one another, and each culture to a child born into it is simply another given about the world. As a particular case of this, I do not at all deny that the structure in language and in a given sentence is a perception, both in the individual and in the individuals of the surrounding culture. But there is a cumulative ongoing creative process in human cultures to which the individuals' control of their perceptions contributes, mostly without their awareness. When you look at the astonishing variety of human cultures, you come ineluctably to see that they present a range of evolving choices of how to be human in the world. Yet (with few exceptions) no individual in them has consciously made any of the

choices implicit in her or his culture. Instead, each participates with the others in the way that the world appears to be, as a given. This is the nature of social reality. And it is simply not perceptible to us until we experience ourselves as foreigners in some other culture, then learn something of what it takes to operate as a native in that other culture, then come back to our own and experience ourselves (perhaps transiently) as foreigners in it, then as natives in it again. This brings the normally unconscious processes by which we conform to social norms to a conscious level. Then we can *begin* to look around and notice them.

My references to gestures from West Africa and so on are not just exotica. They are my attempts to point to the existence of something pervasive in which we fish are swimming.

The Whorf material that I am sending Bill suggests that perhaps some cultures are fish swimming in water, others moles burrowing in earth, others birds flying in air. Control of perception and perceptual control systems are at some level of discussion identical. But much of what taxes us about higher levels is culture-specific, and examining different cultures and different languages can help us to distinguish the general from the specific, the innate from the learned, and properties of hierarchical control from recursive properties of the language we use to talk to one another about it.

>The so-called "grammar" of sentences is what people
>conventionally do in determining the interrelationships and intended
>functions of words. It isn't inherent, it's always changing in various
>ways (the control system that stabilizes the use of language is as
>strong as the Academie Francaise) like the self-organized structure
>it is. It's different depending on who is using it, to whom, and when.
>

Yes, grammar is a system of conventions. No, it is not inherent in language, except for a minimal structure of operators, arguments, and dependencies, and their definition in terms of the dependency requirement of other words (which makes a mathematical treatment possible). (Generative grammar says that more than that is inherent.) Yes, it's always changing, but generally too slowly for people to notice (we just perceive it as variation). Yes, what we do with it is always changing also, but that is a different matter. I may ride my bicycle various places for various purposes and in various manners by various routes but it is the same bicycle with the same capacities. No, I do not think language is a self-organized structure, it is created and maintained by perceptual control of individuals in a culture. So as I read it there is only one disagreement in this, and I am not sure what you intended by that last.

[Martin Taylor 910802 1820]

I recognized your point, that this utterance was bizarre and agrammatical. That point was addressed to Bob. However, he is looking at a purported rule as data, not at the string of words that is said to exemplify the rule. What I invited him to say was how you would determine the presence of the rule (and the regularity of utterances of

that form) by testing for perceptual control. I believe familiar techniques of linguistics can be restated fairly directly in CT terms. Bob?

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Date: Mon, 5 Aug 1991 09:45:41 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: weekend batch responses

[From: Bruce Nevin (910803 6:40)]

Writing up notes written on the train.

I asked the train conductor where he got his earplugs, and he gave me a pair of company-issue plugs. Turns out I don't need them on the train, though I do on the subway.

I experimented informally later, on the walk from Alewife Station to my office. I noticed that when I talk at a normal amplitude, the buzz of the fundamental frequency of the larynx tends to swamp the transients that are a principal (for voiceless stops the only) acoustic cue which consonant is pronounced. Speaking sotto voce is much clearer. Without plugs, loud voice is clearer, with soft voice environmental sounds interfere with acoustic perception. Straightforward signal-to-noise stuff.

When I first got the plugs on the train, I put them in. I noticed that while talking in a somewhat loud voice to my seatmate I had the perception of my pronunciation becoming sloppy, slurring, like a loose steering wheel. This combines (masked) environmental noise with the interference of the fundamental frequency "buzz" of the larynx. I could understand my seatmate fairly easily, by the way. Maybe cotton plugs block mostly lower-frequency rumble?

More experimenting is in order when I have a tape recorder and some blank tape handy.

From: Nora Gallagher. 1984. Ursula Le Guin: in a world of her own. _Mother Jones_, January 1984, 23-27, 51-53.

Le Guin did not know all this when she began the novel [_The Left Hand of Darkness_]. For her, it started with a vision of two people pulling something across a lot of snow, and much of its content was "told to her" by the characters as she went along. Once she discovered something about them, she would go back over the novel, changing pieces here and there. (p. 26)

Mozart heard his music all at once, she had told me earlier; then he had to write it down, to extend it into time. (p. 53)

"Understandingness" seems to be the memory of a perception of understanding, without the memory of the other perceptions involved in that understanding. Those other perceptions may be there, but not recoverable.

Or, it could be a mistaken perception of understanding: the feeling of satisfaction that goes with understanding, without the understanding itself. Later, you notice that the perception of integration, congruence, or the like, does not hold--some other perception does not fit the configuration, or sequence, or program, etc. Or else having forgotten the supporting perceptions (as in the first case) you never notice the lack of closure and can recall only the perception of having understood, but that perception is mistaken. I think this is what you meant--the brilliant lecturer carries you so swiftly and persuasively along that you don't consider corollaries not mentioned by her, and perhaps it is these gaps in part that make her argument difficult to reconstruct. Unfamiliar subject matter most likely.

So we are using attributes of memory and attention (related matters!) to isolate and come to agreement about different levels of perception.

Suppose we don't always use all the structure that operator grammar finds in sentence S to construe S. Suppose we do rely on redundancy in the correlation of S with nonverbal perceptions and just bypass some of the parsing.

* The structure in language is still there.

* Different parts of the structure in S may be left unexploited in different contexts for saying S.

* And anyway redundancy is probably used for confirmation, part of the sense of congruity.

Stephen Johnson's sentence analyzer for the information in texts turns out a growing pool of operator-argument dependency trees. It starts with a dependency tree from the lexicon entry for each word in turn, with empty slots for the argument requirement of that word. It fills the empty positions by matching up subtrees and forming more complete trees. "More complete" and "congruent" are criteria determining which gets attention, but all are going on in parallel, even the "losers." Ideal for pandaemonium parallelism.

In a CT version, dependencies with and among nonverbal perceptions (present, remembered, imagined) are included. In construing an utterance like the one Martin transcribed for us, the strictly linguistic analyzer could "grow" several subtrees that didn't match further so as to form any single sentence. With nonverbal perceptual

information I believe we would adapt words and dependencies, actually make changes in them, so as to make the matches and integrate those subtrees into one or a few sentences. I believe an informed listener would produce such sentences if asked "what did he say?" I believe that this automatic "normalization" process underlies the performance of transcribers, whatever their training. My own experience confirms that it requires repeated hearings, checking and rechecking, to get an accurate transcription. We write our reconstruction of the speaker's intention rather than (a representation in conventional spellings of) the actual acoustic signal.

Pairs of language users rely on the learned patterning in language, shared with all users of that language, as means for finding a useful analogy between their necessarily private systems of control of private perceptions, and coming to agreement about the analogy and about its uses for each. To be determined is the extent to which learning the patterning in a particular language entails developing certain control systems and/or connections among control systems not shared by speakers of other, different languages. This is the linguistic relativity question discussed in connection with Whorf and the Hopi, which I will take up separately.

Bill:

What coordinates the parallel control of different kinds of structure?

> Sometimes many configurations can be seen in the same collection of sensations; then we usually experience all the configurations at once. Mutually exclusive pairs are rarer than non-exclusive sets. When many configurations can be perceived at once, they can often be _controlled_ simultaneously (with practice) and even independently. A man can create an expressive intonation of

voice, a phoneme, a posture, and a facial expression all at the same time, and think nothing of it. (BCP:127)

Is this a configuration of configurations?

The current sentence--about how we are capable, obviously, as our capacity to create and sustain long interruptions (often themselves interrupted) and then to resume what was interrupted again and again demonstrates, of managing the interruption of a configuration (or sequence, or program) by another, then resuming it--illustrates (somewhat taxingly in this outermost embedment) the point that it asserts.

Your reading of this may have been interrupted by a sip of coffee, a telephone call, etc.

How does the perceptual hierarchy handle recursion? This is presumably on a program level, and involves memory. Is it not similar to the coordination of parallel configurations noted above?

When by practice I become a proficient pianist (or typist, etc.), do I become so by neurological changes, the growth of new control systems and/or connections between existing control systems? Help me in my ignorance. I don't know the neurophysiological facts here.

I am guessing that if this requires modification of the model, it is in the clarification of how the currently postulated levels are implemented and interconnect.

This smells to me like your comments on associative memory "the idea that any perception can be used as the address of any other perception stored `with' it, whatever `with' means. . . . This means that the control hierarchy has to be involved in creating addresses and detecting responses from memory in many systems at once. . . . " (910731.0800). If so, then I'm asking importunately for answers that just have yet to be determined.

On the input side, it is easy to see how concurrent perceptions are in parallel because they are taking place concurrently. The parallelism is given with events beyond my control that I am perceiving. On the output side, why should I produce the word, the intonation contour, the expressive timbre, the facial expression, the hand gesture, the body posture, and so on, concurrently? Perhaps part of the answer is that they are not *initiated* simultaneously. The intonation contour is already running when I start the word, the word is already running when I start the phoneme, and so on. The successive initiations of these configurations is a sequence? But the facial expression appears to be part of more genuinely a parallel track, not participating in this constructive hierarchy. And no matter how I look, even limited to just language, or just body language, I seem to get configurations of configuration or the like.

Gary:

Regarding this from your Chapter 5 (section 5.6.2):

> . . . in much the same way that we "instinctively" know how to keep our body temperature at or near a constant temperature of 37 degrees Celsius through such automatic, unlearned behaviors as shivering and sweating, organisms also appear to inherit other control systems which underlie adaptive, species-specific behaviors. So it is not that the spider is programmed with a fixed behavior pattern which will result in the construction of a web, but rather the spider is able to control its perception of its environment to match an inherited perception (reference level) by varying its behavior as needed to construct its web.

Would you not say that the spider inherits a control system for the pattern of the web as well as a reference level for that control system? The same neural signal (reference level) input to a control system for, say, leg configuration would not result in a web. Should not this say that the spider inherits a control system on the third

level for web configuration? I wonder what the highest level of control is for a spider. Do we know how to determine that? Then we might concern ourselves with possible inheritance of reference levels at the highest existing perceptual level.

Compare your quote from Bill:

[I think] . . . All that can be inherited are control systems, and at the highest exist[ing] level perhaps some reference signals.

What do we know about the levels of perceptual control of a neonate? Developmental stages? The Moro reflex indicates control for falling (or for body attitude): lower the infant's head suddenly backward toward the floor, and she opens her arms widely and lets out an angry sounding cry. This presumably reflects presence of a control system at birth. Coordination of looking with hearing, seeing with grasping, etc. comes at about 2 months. Does this reflect the development of control systems not present in the neonate? Piaget records hiding a watch from a 9-month- old boy under a cover. After three times there, he hid it under a wool garment on the opposite side of the boy. But the boy kept looking under the cover for it on repeated trials, seeming puzzled at not finding it there. Does this indicate that reorganization does not at this stage apply to the control systems involved in remembering and finding the watch? Piaget's conclusion I think was that the child does not yet attribute independent existence to objects, such that they can change location independently of his perceptions of them. Have some in the CSG reformulated the findings of Piaget and other developmental psychologists in CT terms?

Bob Yates:

There seems to be a wonderful opportunity here to assess just what species-specific inherited reference levels may underlie the acquisition of language and universals of language. Does it not seem to you that HCT constrains the problem in useful ways?

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Date: Mon, 5 Aug 1991 09:49:04 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: Whorf/Hopi

[From: Bruce Nevin (910803 10:27)]

Back from kid stuff for a bit.

I have some Whorf excerpts that I will send you by snail. This material is copied from the book _Culture in crisis: a study of the Hopi Indians_, by Laura Thompson. What I have is Chapter 8 (Time, Space, and Language) which comprises mostly excerpts from Whorf's writings with an introductory note and a couple of added footnotes by Thompson. The photocopy was given me by Alan Strain when I was at the Hokan-Penutian Language Conference at the beginning of July.

I have typed an sample from this that bears especially on the question of Hopi perception of time. I am sending this in a separate post, which CGS readers who are uninterested in this thread should delete unread.

Whorf also writes about Shawnee, Apache (related to Navajo), Nootka of Vancouver Island, Maya, and other languages.

The following two paragraphs are not in the Thompson reprint. They are from the article "Languages and Logic" (_Technological Rev._ 43 (1941), MIT, reprinted in John Carroll, _Language, Thought and Reality: selected writings of Benjamin Lee Whorf_, MIT, 1956, perennially reprinted):

. . . segmentation of nature is an aspect of grammar . . . as yet little studied. . . . We cut up and organize the spread and flow of events as we do, largely because, through our mother tongue, we are parties to an agreement to do so, not because nature itself is segmented in exactly that way for all to see. Languages differ not only in how they build their sentences but also in how they break down nature to secure the elements to put in those sentences. This breakdown gives units of the lexicon. "Word" is not a very good "word" for them; "lexeme" has been suggested, and "term" will do for the present. By these more or less distinct terms we ascribe a semifictitious isolation to parts of experience. . . .

We might isolate something in nature by saying `It is a dripping spring.' Apache erects the statement on a verb ga `be white (including, clear, uncolored, and so on).' With a prefix nO- the meaning of downward motion enters: `whiteness moves downward.' Then tO, meaning both `water' and `spring' is prefixed. The result corresponds to our `dripping spring,' but synthetically it is `as water, or springs, whiteness moves downward.' . . . The same verb, ga, with a prefix that means `a place manifests the condition' becomes gohlga: `the place is white, clear; a clearing, a plain.' These examples show that some languages have means of expression--chemical combination, as I called it [Whorf was a chemical engineer]--in which the separate terms are not so separate as in English but flow together into plastic synthetic creations. Hence such languages, which do not paint the separate-object picture of the universe to the same degree as English and its sister tongues, point toward possible new types of logic . . .

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Date: Mon, 5 Aug 1991 09:57:58 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: Excerpts from Whorf

[From: Bruce Nevin (910803-04)]

A few samples of Whorf's writings on Hopi from the Thompson book mentioned in prior post, mostly originally published in _Technology Review_, MIT, 1940 and 1941:

To fit discourses to manifold actual situations all languages need to express durations, intensities, and tendencies. It is characteristic of our language to express them metaphorically. The metaphors are those of spatial extension, _i.e._, of size, number (plurality), position, shape, and motion. We express duration by long, short, great, much, quick, slow, etc.; intensity by large, great, much, heavy, light, high, low, sharp, faint, etc.; tendency by more, increase, grow, turn, get, approach, go, come, rise, fall, stop, smooth, even, rapid, slow, and so on through an almost inexhaustible list of metaphors that we hardly recognize as such since they are virtually the only linguistic media available. The non-metaphorical terms in this field, like early, late, soon, lasting, intense, very, tending, are a mere handful, quite inadequate to the needs.

This condition is part of our whole scheme of _objectifying_ -- imaginatively spatializing qualities and potentials that are quite non-spatial (so far as any spatially-perceptive senses can tell us). Noun-meaning (with us) proceeds from physical bodies to referents of far other sort. Since physical bodies and their outlines in _perceived space_ are denoted by size and shape terms and reckoned by cardinal numbers and plurals, these patterns of denotation and reckoning extend to the symbols of non-spatial meanings, and so suggest an _imaginary space_. Physical shapes move, stop, rise, sink, approach, etc., in perceived space; why not these other referents in their imaginary space? This has gone so far that we can hardly refer to the simplest non-spatial situation without constant resort to physical metaphors. I "grasp" the "tread" of another's argument, but if its "level" is "over my head" my attention may "wander" and "lose touch" with the "drift" of it.

The absence of such metaphor from Hopi speech is striking. Use of space terms when there is no space involved is _not there_ -- as if on it had been laid the taboo teetotal! The reason is clear when we know that Hopi has abundant conjugational and lexical means of expressing duration, intensity, and tendency directly as such, and that grammatical patterns do not, as with us, provide analogies for an imaginary space. The many verb "aspects" express duration and tendency of manifestations, while some of the "voices" express intensity, tendency, and duration of causes or forces producing manifestations. Then a special part of speech, the "tensors," a huge class of words, denotes only intensity, tendency, duration, and sequence. The function of the tensors is to express intensities, "strengths," and how they continue or vary, their rate-of-change; so that the broad concept of intensity, when considered as necessarily always varying and/or continuing, includes also tendency and duration.

Tensors convey distinctions of degree, rate, constancy, repetition, increase and decrease of intensity, immediate sequence, interruption or sequence after an interval, etc., also _qualities_ of strengths, such as we should express metaphorically as smooth, even, hard, rough. A striking feature is their lack of resemblance to the terms of real space and movement that to us "mean the same." There is not even more than a trace of apparent derivation from space terms. So while Hopi in its nouns seems highly concrete, here in the tensors it becomes abstract almost beyond our power to follow.

Reality as "Matter" versus Reality as "Events"

To sum up the dominant contrasts between our own and Hopi formulations which have been discussed . . . Our own "time" differs markedly from Hopi "duration." It is conceived as like a space of strictly limited dimensions, or sometimes as like a motion upon such a space, and employed as an intellectual tool accordingly. Hopi "duration" seems to be inconceivable in terms of space or motion, being the mode in which life differs from form, and consciousness _in_toto_ from the spatial elements of consciousness.

We have analyzed reality largely in terms of what we call "things" (bodies and quasi-bodies) plus modes of extensional but formless existence called "substances" or "matter." We tend to see existence through a binomial formula [ex: "stick of wood"--BN] that expresses an existent as a spatial form ["stick"] plus a spatial formless continuum ["wood"] related to the form as contents is related to the outlines of its container [ex: "cup of water"]. Non-spatial existents [e.g. time] are imaginatively spatialized and charged with similar implications of form and continuum [e.g. "5 minutes of time"].

On the other hand, the Hopi seem to have analyzed reality largely in terms of _events_ (or better "eventing"), referred to in two ways, objective and subjective. Objectively, and only if perceptible physical experience, events are expressed mainly as outlines, colors, movements, and other perceptive reports. Subjectively, for both the physical and non-physical, events are considered the expression of invisible intensity-factors, on which depend their stability and persistence, or their fugitiveness and proclivities. This implies that existents do not "become later-and-later" all in the same way; but some do so by growing, like plants, some by diffusing and vanishing, some by a process of metamorphoses, some by enduring in one shape till affected by violent forces. In the nature of each existent able to manifest as a definite whole is the power of its own mode of duration; its growth, decline, stability, cyclicity, or creativeness. Everything is thus already "prepared" for the way it now manifests by earlier phases, and what it will be later, partly has been, and partly is in the act of being so "prepared." An emphasis and importance rests on this preparing or being prepared aspect of the world that may to the Hopi correspond to that "quality of reality" that "matter" or "stuff" has for us.

What surprises most is to find that various grand generalizations of the Western world, such as time, velocity, and matter, are not essential to the construction of a consistent picture of the universe.

The psychic experiences that we class under these headings are, of course not destroyed; rather categories derived from other kinds of experience take over the rulership of the cosmology and seem to function just as well.

Reflection in Behavior Patterns

[The beginning of this portion of the reprint is unfortunately garbled. This section appears to be from citation 1941b, whatever that is--the photocopy blanks out here, and I lack Thompson's bibliography. I will call Alan Strain and ask him. This text an prior citations of 1941b don't correspond to anything of that date in the collection edited by John Carroll. Problematic bits I put in square braces.]

These contrasts are reflected not only in habitual thought but also in behavior. And they explain some of the major differences between [Hopi would naturally suppose that his thought (or he himself traffics]
of Hopi behavior which are obviously related to the linguistic and thought patterns which have been discussed.

A characteristic of Hopi behavior is the emphasis on preparation. This includes [. . .] and getting ready for events well beforehand, elaborate precautions to insure persistence of desired conditions, and stress on good well as the preparer of right results.

Hopi preparing behavior may be roughly divided into announcing, outer preparing, inner preparing, covert participation, and persistence. Announcing, or preparative publicity, is an important function in the hands of a special official, the Crier Chief. Outer preparing is preparation involving much visible activity, not all necessarily directly useful within our understanding. It includes ordinary practicing, rehearsing, getting ready, introductory formalities, preparing of special food, etc. (all of these to a degree that may seem over-elaborate to us), intensive sustained muscular activity like running, racing, dancing, which is thought to increase the intensity of development of events (such as growth of crops), mimetic and other magic, preparations based on esoteric theory involving perhaps occult instruments like prayer sticks, prayer feathers, and prayer [corn] meal, and finally the great cyclic ceremonies and dances, which have the significance of preparing for rain and crops.

Inner preparing is use of prayer and meditation, and at lesser intensity good wishes and good will, to further desired results. Hopi attitudes stress the power of desire and thought. With their "microcosm" it is utterly natural that they should. Desire and thought are not only the earliest stage of preparing but accompany all subsequent preparatory stages. Moreover, to the Hopi, one's desire and thoughts influence not only his own actions, but all nature. This too is wholly natural. Consciousness itself is aware of work, of the feel of effort and energy, in desire and thinking. Experience more basic than language tells us that if energy is expended effects are produced. _We_ tend to believe that our bodies can stop up this energy, prevent it from affecting other things until we will our

bodies to overt action. But this may be only because we have our own linguistic basis for a theory that formless items like "matter" are things in themselves, malleable only by similar things, by more matter, and hence insulated from the powers of life and thought. It is no more unnatural to think that thought contacts everything and pervades the universe than to think, as we all do, that light kindled outdoors does this. And it is not unnatural to suppose that thought, like any other force, leaves everywhere traces of effect.

What . . . do we suppose our consciousness is dealing with when we are thinking of [a corn plant]? Probably we think it is dealing with a "mental image" which is to the [plant] but a mental surrogate of it. . . . we are dimly aware that we carry about with us a whole imaginary space, full of mental surrogates. To us, mental surrogates are old familiar fare. Along with the images of imaginary space . . . we tuck the thought of the actually existing [corn plant], which may be quite another story, perhaps just because we have that very convenient "place" for it.

The Hopi thought-world has no imaginary space. The corollary to this is that it may not locate thought dealing with real space anywhere but in real space, nor insulate real space from the effects of thought. A Hopi would naturally suppose that his thought (or he himself) traffics with the actual . . . corn plant . . . that he is thinking about. The thought then should leave some trace of itself with the plant in the field. If it is a good thought, one about health and growth, it is good for the plant; if a bad thought, the reverse.

The Hopi emphasize the intensity-factor of thought. Thought to be most effective should be vivid in consciousness, definite, steady, sustained, charged with strongly-felt good intentions. They render the idea in English ad "concentrating," "holding it in your heart," "putting your mind on it," "earnestly hoping." Thought power is the force behind ceremonies, prayer-sticks, ritual smoking, etc. The prayer-pipe is regarded as an aid to "concentrating." Its name means "instrument of preparing."

Covert participation is mental collaboration from people who do not take part in the actual affair, be it a job of work, hunt, race, or ceremony, but direct their thought and good will toward the affair's success. Announcements often seek to enlist the support of such mental helpers as well as of overt participants, and contain exhortations to the people to aid with their active good will. [In a footnote: unlike cheerleaders, these "get in their deadliest work before, not during, the game."] . . . one purpose . . . is to obtain the mass force of many good wishers to offset the harmful thought of ill wishers. Such attitudes greatly favor cooperation and community spirit [and] . . . must help vastly toward the rather remarkable degree of cooperation that in spite of much private bickering the Hopi village displays in all the important cultural activities.

Hopi "preparing" activities again show a result of their linguistic thought background in an emphasis on persistence and constant insistent repetition. A sense of the cumulative value of innumerable small momenta is dulled by an objectified, spatialized view of time

like ours, enhanced by a way of thinking close to the subjective awareness of duration, of the ceaseless "latering" of events. To us, for whom time is a motion on a space, unvarying repetition seems to scatter its forces along a row of units of that space, and be wasted. To the Hopi, for whom time is not a motion but a "getting later" of everything that has ever been done, unvarying repetition is not wasted but accumulated. It is storing up an invisible charge that holds over into later events.

[Analogy to concept of acceleration in physics: perhaps the Hopi] recognize naturally that force manifests not as motion or velocity, but as cumulation or acceleration. Our linguistic background tends to hinder in us this same recognition, for having legitimately conceived force to be that which produces change, we then think of change by our linguistic _metaphorical_ analogue, motion, instead of by a pure motionless changingness concept, _i.e._, accumulation or acceleration. Hence it comes to our naive feeling as a shock to find from physical experiments that it is not possible to define force by motion, that motion and speed, as also "being at rest," are wholly relative, and that force can be measured only by acceleration.

[. . .] It is sometimes stated that Newtonian space, time, and matter are sensed by everyone intuitively, whereupon relativity is cited as showing how mathematical analysis can prove intuition wrong. But to lay the blame upon intuition for our slowness in discovering mysteries of the cosmos, such as relativity, is wrong. Newtonian space, time, and matter are no intuitions. They are receipts from culture and language. This is where Newton got them.

Our objectified view of time is, however, favorable to historicity and to everything connected with the keeping of records, while the Hopi view is unfavorable thereto. The latter is too subtle, complex, and ever-developing, supplying no ready-made answer to the question of when "one" event ends and "another" begins. When it is implicit that everything that ever happened still is, but is in a necessarily different form from what memory or record reports, there is less incentive to study the past. As for the present, the incentive would be not to record it but to treat it as "preparing." But our objectified time puts before imagination something like a ribbon or scroll marked off into equal blank spaces, suggesting that each be filled with an entry. Writing has no doubt helped toward our linguistic treatment of time, even as the linguistic treatment has guided the uses of writing.

It is clear how the emphasis on "saving time" which goes with all of the above and is an obvious objectification of time, leads to a high valuation of "speed," which shows itself a great deal in our behavior.

Still another behavioral effect is that the character of monotony and regularity possessed by our image of time as an evenly scaled limitless tape measure persuades us to behave as if that monotony were more true of events than it really is. That is, it helps routinize us. We tend to select and favor whatever bears out this view, to "play up to" the routine aspects of existence. One phase of this is behavior evincing a false sense of security or an assumption that all

will always go smoothly, and a lack of foreseeing and protecting ourselves against hazards. Our technique of harnessing energy does well in routine performance, and it is along routine lines that we chiefly strive to improve it -- we are, for example, relatively uninterested in stopping the energy from causing accidents, fires, explosions. [Whorf speaks here from experience in his career as a fire-prevention engineer for Hartford Insurance.] Such indifference to the unexpectedness of life would be disastrous to a society as small, isolated, and precariously poised as the Hopi society is. . . .

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Date: Mon, 5 Aug 1991 09:06:20 -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, Mindreading

[From Rick Marken (910805)]

Kent McClellend -- thanks for the copy of the spreadsheet. Great graphics. I think what we could do is use you graphics macros to plot data that is generated iteratively by the operation of the hierarchical control model. Maybe we could play with this in Durango -- I just have to be sure that I bring the newest version of Excel (I had an old version at home and it wouldn't read your worksheets).

I had the pleasure of seeing Dag Forsell's presentation to the "Secular Humanists of LA". Nice work Dag. The meeting was interesting -- partly because it made me realize that I'm not a secular humanist (it still seems like a plausible label but this was not my group). Anyway, it was interesting to see the familiar re-interpretations of the control model. For example, one fellow thought that control theory was all about the importance of perception in controlling behavior. This was after Dag spent some time explaining that control systems control perception. Have fun Dag. You have far more patience with people than I do.

Hasta Luego

Rick M.

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Date: Mon, 5 Aug 1991 15:49:23 -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: Intentional Behavior Demo

[From Rick Marken (910805b)]

Ed Ford (910805) posted a personal note reminding me to:

> bring that intentional demo model to the
>meeting.

I will do that Ed. I take it you mean the one where you move several objects around the screen and the computer determines which one is being moved intentionally. I wanted to mention this on the net because I have been working on that program lately and I've made some improvements that make it work better and that (I think) make the method of its operation a bit more tranparent. I think it should be relatively easy to convert the program to a form compatible with any IBM PC.

For the benefit of those who are not familiar with this demo, here's how it works. There are five numbers, 1..5, displayed on the computer screen. The two dimensional position,p, of each number is determined by the value of a number generated by the computer (the disturbance,d) and the x,y position of the mouse, m. Thus, the position of each number at any instant is

p.i = d.i + m i = 1 to 5

Each disturbance is a narrow band filtered random noise waveform; a different waveform is added to each number. If the mouse, m, is not moved then it looks like five numbers wandering aimlessly around the screen in different patterns. The subject is asked to pick one of these five numbers and make some pattern with it -- such as a circle or figure eight. In fact, the
patterns can be quite arbitrary and always changing. In order to do this the subject must move the mouse so that the mouse values, in combination with the disturbance, produce the intended pattern of number movement.

Of course, the mouse affects all five numbers at the same time. So the movement of any of the numbers could be called a "behavior" of the subject; movements of all the numbers are a result of muscle tensions produced by the subject. An observer cannot tell, by looking at the movements of the five numbers, which number is being moved intentionally by the subject. But it is only by determining which number is being moved intentionally that an observer can come to a legitimate conclusion about "what the subject is doing". The demo shows that, in order to know what a person is doing you must figure out what perception they are trying to control. The computer can do this by doing a version of the test for the controlled variable. In the original version of the demo (which doesn't work too well) the test was done by continuously computing a quantity called the stability measure, s, for each number:

s.i = (var(d.i)+var(m))/var(p.i)

where var() is the variance of the quantity in parentheses.

The numerator is the expected variance of number i if it is not controlled.

The denominator is the actual variance of number i. If number i is controlled, the actual variance will be much smaller than the expected variance and the s for that number should be much larger than that for the others.

For various reasons, the stability measure approach to "detecting intention" is relatively flakey. It worked, but there would often be times when one of the non-controlled numbers would have a particularly low variance oe some-thing. So I now compute the correlation between the position of the mouse and the disturbance to each number. For the controlled number, the correlation between mouse and disturbance is large and negative. This measure of intention works extremely well -- very reliable.

The demo is now nearly perfect. As a subject you can sit there and pick one of the numbers and move it in some weird pattern and the computer picks it up in a matter of seconds and makes that number larger than the rest. If you then mentally switch to another number, the computer will detect the switch in a few seconds, make the previously moved number normal size and enlarge the currently moved number. You can mentally switch from number to number (controlling each one for a few seconds) and the computer will pick up, within a few seconds, the number that you are currently moving intentionally.

There is now virtually no way that an observer, looking at the behavior of the numbers on the screen, can tell which number is being moved intentionlly at any moment. A clever sole might look to see which number varies the least in its movement -- under the assumption that control produces stability. Before I improved the program (with a simple secret that I will describe at the meeting) this was a reasonable possibility. But now there is no necessary difference in the variance of controlled and uncontrolled numbers -- unless the subject, say, keeps the intentionally moved number in one place.

I've also made it possible to print out the correlations and variances for all numbers, at the bottom of the screen, at any point during the run. This will help people understand the "intention detection" algorithm.

This demo shows, in a very dramatic way, why an objective approach to behavior fails. I plan to use this demo in a talk dealing with the fact that behavior is a subjective, not an objective, phenomenon. The point is that behavior is intended perception, not an objective result of efferent neural impulses. You can objectively identify what a person is doing (as is done in the demo) only when you can determine what he/she subjectively, wants to perceive.

Regards

Rick M.

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Date: Tue, 6 Aug 1991 06:35:16 -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 rules

[From Bill Powers (910805.0900)]

Bruce Nevin (910805) --

>Why do I insist that certain perceptions (objects and relations) are in
>the language? Because the language is a given in the world when a child
>comes to learn it, just as the characteristics of the playroom and toys
>are a given.

If we think about this point carefully, I think we can finally reach a consensus.

The same phenomenon exists in every aspect of a society, not just in language. PCT tells us that all rules, conventions, laws, and so on (by which people actually live) must, in order to be effective, exist as reference signals at the appropriate level inside each person. They do not reside outside people, even when they're written down or present as physical constructions. They are not implemented by any mechanism outside individual human beings. There are no natural control systems outside the individual human being. Not even in a society.

At any given time, a society is made of adult members and of young people getting familiar with it. What they have to get familiar with is not some external structure, but the other people in it, and the way the other people construe and use those external structures. The adults teach the children by example and by explicit instruction how to use language, how to use a knife and fork or chopsticks, which side of the road to drive on, how to get money in an acceptable manner, and so on. Each adult teaches these things out of a single person's understanding of them.

This teaching includes teaching what to perceive as well as the reference levels for the perceptions. If a child construes the world in some novel way, the adults will not see any sense in the child's control actions. There will be both active and passive pressure to see the world in the conventional way and learn to control conventional perceptions.

There are, however, variations from person to person. The child doesn't get the same story from everyone. Also, children come up with novel ways of saying and doing things, and adults pick them up because they're funny, insightful, and refreshing. The children don't always get corrected. Sometimes they are allowed to introduce variations of their own. I still love "far out!"

All this goes to show that there are no "social reference levels." If there were, there would be control actions that always bring the social variables back to the same form. What happens instead is that all pressures to change the social forms are resisted (because they create errors in individual people), but at the same time the perceptions in

individuals gradually change, and the reference levels chosen from among them also gradually change. In the long term there is no resistance at all to social change; that is how we know that there are no external social control systems. There is inertia, but no control.

In the short term, people learn and retain ways of perceiving and controlling. Each person comes to an understanding of what is worth perceiving and what is worth controlling. The main teacher is conflict. Conflict frustrates control and causes a waste of energy. So people naturally modify their own goals and perceptions to minimize conflict with those around them. When they try to deviate too far from social norms, they create errors in many other people. Each other person, in opposing the disturbance, pushes back in some fashion against the deviant behavior. The deviant person feels the sum of a thousand mild resistances as if it were one powerful sanction against the change. A thousand points of light make a searchlight.

This is what creates the inertia. In order to minimize conflict and maximize freedom to control, society-wide changes must always be gradual so that in effect, everyone changes at once. No short-term deviation can escape what appears to be coordinated social presure against the deviation. But the only coordination that is necessary to achieve this effect is that each person resist what that person perceives to be an error. This resistance does not even have to be exerted directly against an individual's attempt to reach a goal. Others are affected only by side-effects of control behavior. All that is required is for one of those side-effects to disturb some variable that's important to another person.

To this natural appearance of coordination of opposition, we can add, of course, deliberate coordinations of opposition to deviants such as carried out by the Academie Francaise, police forces, schools, and scientific disciplines (appropriate word!). This more organized way of resisting deviations, however, works exactly the same way: one person at a time. There is simply a more conscious attempt to reach alignment of goals among the enforcers. The result is also a narrower definition of what amounts of a deviation. I suspect, too, that the time-scale of change is shortened rather than lengthened by this sort of deliberate coordination. The reason is that when people try to define their goals very narrowly, and to resist strongly the slightest deviation from them, the attempted coordination is more likely to turn into dissention and eventually into fragmentation. Fanatics necessarily end up as loners.

With respect to language, I'm trying to show how language can appear to be a thing, a universal force or rule, without actually being that. Of course the same argument applies to any apparent social ordering influence that seems too long-lived to be associated with individuals. My argument is that individuals are entirely responsible for such things, but that in their need to avoid direct conflict and in their natural resistance to disturbances, they seem to be under the control of something larger than themselves. In fact, they are: they have no choice but to go on being control systems.

"Erwin" (910803) --

The demo disks are in two parts. The first part is an introduction to the phenomenon of control. Interactive demonstrations, with the user being a control system (using a mouse or joystick) show the major phenomena that require a theory to explain them. The second part is an introduction to the theory of control (CT). Again with the user being the control system, the program builds up a block diagram of a control system, showing how each block works and what it means. This is followed by a development of the basic equations of control. At the end is an section in which a model is applied to simulation of real (the subject's) tracking behavior, with user control over the parameters of the model.

These are "shareware" programs. This means that you can try them, copy them and give the copies away, and do anything you want with them that is noncommercial. If you use them professionally, or just feel generous, there is a suggested fee of $35 for part I and $60 for part II. In the spirit of shareware, which is to encourage rather than limit the dissemination of useful programs, payment is up to the conscience and resources of the user.

My own resources being limited, it would be nice if people requesting these programs would at least send a disk (formatted) and self-addressed stamped mailer to me. I will send copies as requested as long as I can afford to.

To run the programs you should have an AT-compatible DOS machine, with CGA...VGA, Hercules monochrome, or ATT400 graphic display, and a mouse or a game joystick (the mouse works much the best). The programs will run on XT-class computers, but slowly. They seem to run on 386 machines too, under DOS. The programs are not Windows-aware or anything else fancy.

The programs are distributed on one 360-K 5-1/4 inch disk (I can't do 3-1/2 inch disks). The files are self-extracting compressed ZIP files. There is a READ1ST batch program that explains how to decompress and use them. I usually send the Little Man pointing simulation along, too, because it will also fit on the disk. The shareware fee for that is $35 if you want to pay it.

To send the disk I do need your full name and your Uncle Sam address. If anyone knows how to put these programs into a network-accessible data base for downloading, tell me or go ahead and do it if you have the programs. Guaranteed no viruses -- they're compiled from source code, and I run a virus checker now and then (and almost never download programs from others).

Best

Bill P.

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Date: Tue, 6 Aug 1991 09:42:21 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: social rules

[From: Bruce Nevin (910806 09:50)]

Bill Powers (910805.0900)

This is just to acknowledge your response, Bill. As promised, I am going to try to batch replies on the weekend. I believe in that way I can be more concise and also be more responsible to my employer.

I have to say that ideologically I find your position most congenial. I have been an anarchist for as long as I can remember. But the tendency to hypostasize the constructs we make of Family, Society, the State, etc, is pervasive, and not to be dismissed I think without plumbing its depths, so I proceed arguendo. And it is precisely those agreements that we have no memory of making that are problematic for our coming to consensus.

I believe my response will use an analogy between the relations among people as control systems and the relations among control systems within people. The top level in both cases is reference values, and it is the reference values that are socially inherited.

There is no forest, only the trees, eh? There is no society, only the people (control systems) in it . . . there is no person, only the control systems in her. . . . So long as we don't shift from one kind of thing (control systems) to another (neurons), you might be able to get away with this reductionism.

Hope the Hopi stuff is suggestive of good experiments. I think the linguistic relativity question is an excellent field for CT to reinterpret. Attempts to prove or disprove the Whorf/Sapir Hypothesis have all been tortuously couched on the Procrustean bed of an S-R perspective. I could send you a brief survey paper that I wrote as one part of my PhD prelims, if you like. I think there is great opportunity there. Given learning of word-category association, why not learning of category-{relationship, event, transition, configuration, sensation} associations peculiar to (the people in) a given culture? Details of kinesthetic configurations can obviously be specific to subcultures, e.g. Ray Birdwhistell's work (midwestern US vs. Yankee ways of smiling). Why should not a monolingual speaker of Apache have a different neural organization of control systems involved in perception of a spring (Whor's example) than we English speakers do?

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Date: Tue, 6 Aug 1991 12:53:59 -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 rules

[From Rick Marken (910806)]

Bill Powers (910805.0900) says:

>   There are no natural control systems outside the
>individual human being. Not even in a society.

Except, of course, other people. But that was the whole point of your post. Social rules are the result of the mutual interation of hierarchical control systems. As you say:

>   What happens instead is that all
>pressures to change the social forms are resisted (because they create
>errors in individual people), but at the same time the perceptions in
>individuals gradually change, and the reference levels chosen from among
>them also gradually change. In the long term there is no resistance at
>all to social change; that is how we know that there are no external
>social control systems. There is inertia, but no control.

A big question is whether the drift in references and perceptions that we see happening historically is basically random or whether it is constrained, to some extent, by intrinsic references that are common to ALL people. My impression is that there are some general constraints on the inertial change in reference levels. I also think that technical developments have made certain directions of drift more likely, as an accidental side effect. Birth control pills and safe surgical procedures have surely made it easier for references to change regarding sexual mores, gender roles and abortion. The resistance to these changes produced by other control systems is obvious. But, nevertheless, a drift has occurred and, I think, will continue toward more "liberal" sexual and sex role references (AIDS notwithstanding). And I think this change is being eased (or exacerbated, depending on your reference setting) by the technical developments. Of course, this is also producing more strenuous resistence by those with "conventional" references. So maybe the "push" by these technological "lubricants" to change is offset by the efforts of the control systems with conventional references.

>   In order to minimize conflict and
>maximize freedom to control, society-wide changes must always be gradual
>so that in effect, everyone changes at once. No short-term deviation can
>escape what appears to be coordinated social presure against the
>deviation. But the only coordination that is necessary to achieve this
>effect is that each person resist what that person perceives to be an
>error. This resistance does not even have to be exerted directly against
>an individual's attempt to reach a goal. Others are affected only by
>side-effects of control behavior. All that is required is for one of
>those side-effects to disturb some variable that's important to another
>person.

I agree. But I also think this assumes that majority references are completely arbitrary. I think that there are "natural" disturbances that contribute to the
difficulty of controlling relative to a "deviant" references. For example, societal references regarding acceptable levels of sexual activity are surely enforced, in part, by the unpleasant consequences of adopting

"deviant" levels -- ie, you get venereal disease or pregnant. To the extent that inventions like penicillan and the pill reduce the chances of such consequences, more people will be willing to test new references for sexual activity. They will still get resistance from the majority but now more and more of the new generations of control system can try the new references with not only fewer natural consequences but less resistence from the fewer control systems around trying to defend the currently accepted references. So this might be the way that technological change can "push" social references in new directions.

I would guess that similar kinds of developments ease changes in references for language rules. Groups that rely on written language communication technologies will experience an "easing up" in certain directions of language reference inertial change. Groups that rely on auditory language (street communication, TV, radio) should experience easing up in other directions. I think this is what we observe (though I think it would have been difficult to predict). Certain usages seem to be accepted in auditory communications that are not accepted in written. I would argue that this is a simple example of a technology (writing) influencing the ease and direction of inertial reference drift.

My gosh, I think I just argued for a dynamic attractor model of social rule drift. Yikes.

Regards

Rick M.

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Date: Wed, 7 Aug 1991 07:26:38 -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; Shareware

[From Bill Powers (910806.0900)]

Bruce Nevin (910803) --

Driving with a loose steering wheel is a nice metaphor. I can see that you're fully hooked on control theory. For you it's language; for me it was finger-wiggling. Yes, these are perceptions that I see and feel. Yes, I can make those perceptions change any way I want (almost). No, I can't feel the outgoing signals ... so I guess control theory still works, this morning anyway. Thirty-five years of mornings, and I still have to check it out.

> * The structure in language is still there.
> * Different parts of the structure in S may be left unexploited in
> different contexts for saying S.

Maybe I can say a little better what I've been trying to say about the structure of language. If language utterances are structured at the program level, they are put together by running a program. The program structure doesn't bear any necessary resemblance to the word-structures that come out of it. Considered as a whole, a program structure is a network of choice-points, where the branchings are not constrained to bifurcations but can be multidimensional (not an "if" statement but a "case" or "switch" statement). The entire program with all its branchings exists simultaneously as a web of connections between choice-points waiting to be activated. The path that is taken through this web depends on the inputs and the rule-functions embodied in the web. At any point in the web, an output can be generated (a reference signal for the sequence level) in passing as the program moves from one stage to the next. So as the program runs it calls for generation of short sequences of signals at the next lower level; producing them in perception is the business of the sequence level, not the program level.

The program itself manipulates signals. Neural signals. It does not consider the meanings or referents of these signals: they are just signals. Their meaning lies in their antecedents at lower levels and their re-perceptions at higher levels. At the program level there is simply a computer running a program -- and acting on lower-level systems to produce those inputs that make the program run properly.

I think that this picture conforms to your two statements above.

>What coordinates the parallel control of different kinds of structure?

Not all parallel control actions are coordinated. I can use my hand to gesture as part of speaking a sentence (coordination) or to scratch myself while speaking (independent parallel control in a different hierarchy).

Coordinated control of parallel systems implies that the perceptions under control by each of the systems contribute to a single perception at a higher level. I have shown in several contexts (the Byte article, Part 3; the Little Man demo) that the error signal in the higher system does not have to be transformed through any complex output calculation in order to be turned into appropriate reference signals at the lower level. The error signal can simply be copied into as many output branches as necessary and fed to the reference inputs of the lower systems. The only choice that is needed is the SIGN of the reference signal. For each branch, the sign must be chosen for negative feedback through the related subsystem. As long as an increase in error leads to a change in a given lower-order perception that tends to decrease the error, by each possible path, the higher level control system will work properly.

So coordination is effected primarily through the perceptual side; through the function that makes one higher-level signal out of the perceptual signals in many lower-level systems.

I've demonstrated this for lower-order systems. There may be more to the output story for higher-order systems. It will still remain true that the main aspect of coordination is in the dependency relation between perceptions at different levels. But converting errors at one level into appropriate behavior of reference signals at a lower level might well involve more than just copying the error signal into lower-level reference channels. A transition error, for instance, probably has to be time-integrated once to create a continuously-changing reference signal for distribution among the lower-level systems. An event-error probably requires turning a single value of error signal into a series of settings of lower-level reference signals. This is why I retain an "output function" box in diagrams of all control systems at all levels. This box has to contain the means of converting from an error of one type into reference signals of the proper lower-level type.

>How does the perceptual hierarchy handle recursion?

I'm not convinced that it needs to. In all real (i.e., perceivable) examples of recursion, only a few cycles are ever represented. Even in when you place mirrors facing one another, each image is smaller and dimmer and shows less detail; you can see only half a dozen images (and they are not identical). When we speak of infinite processes in general, we always give just a few elements, saying "etc." or " ... " to refer to the whole infinite process -- we are incapable of DENOTING the whole infinite process. What most such representations do for us, perceptually, is to suggest a generating process (sequence) or algorithm (program) which, if kept operating indefinitely, would converge toward a predictable result. We never actually operate it indefinitely, however, and can neither perceive nor imagine doing so. The word "infinity" has definitions (i.e., other words) but no perceptual referents.

Computer-like recursion (in which a subroutine calls itself) requires saving (in memory) the relevant states of the machine prior to each self-invocation -- which implies that what is invoked is not the SAME process, but only a new copy of the process, even though the same code may be used. Recursion in computers is an elegant and terse way to write certain programs, but a running recursion is very slow and it constantly threatens disasters such as running out of memory. To work, it has to contain criteria for keeping it from running forever. It is certainly not infinite or anything like it.

Perceptual recursion seems to occur when the same level of perception is applied over and over to different parts of one experiential field. Consider a page containing a large image of an "A", composed of pixels which are smaller images of "A"s. One can imagine that the smaller "A"s are composed of even smaller "A"s, etc. (The "etc.", of course, fails when you reach the limit of printing or visual resolution). This looks like a "configuration of configurations ..." where there is the same limitation on the practical meaning of " ... ".

My model of perception can't handle this elegantly. I propose that there's just one "A"-ness detector. It has to serve for the big "A" as well as the little ones. It emits only one signal, so you can't have a signal representing the big "A" and at the same time a different signal representing the little "A"s. I've thought about this for years and have never come up with an answer that satisfies me.

One possibility is to say that there really is just one "A"-ness signal. You can get it from the big "A" or any of the little "A"s. Once this signal is present, that's all the "A"-ness you're going to get, wherever in the perceptual field, and on whatever scale, you apply your "A"-ness perceptual function. This proposition has implications concerning how flexibly a perceptual function is connected to lower levels. It also implies something about the role of attention in directing a given perceptual function's focus and determining other properties of the function such as its scaling factor. These ideas are, of course, vast complications being forced into the model. I can't imagine how to model such a process (although I suspect that some of the people playing with neural networks could come a lot closer to doing so that I could). All I can say is that I recognize the problem, and that I don't have a solution for it.

>When by practice I become a proficient pianist (or typist, etc.), do I
>become so by neurological changes, the growth of new control systems
>and/or connections between existing control systems?

The growth of new connections, which does happen at the dendritic synapse level of organization, has been used (even before it was verified) to explain all learning. I think that goes too far and unjustifiably deletes memory as a storage function.

In learning a new control system, it is necessary to bring together signals from lower levels into the inputs of a new perceptual function. Also, the new error signals have to be routed (in the excitatory or inhibitory sense as needed for negative feedback) to the appropriate lower-level comparators. Both routings require changing connectivity in the nervous system. I think that plasticity in the dendritic synapse connections is precisely the ticket for achieving this kind of learning of control.

But once a control system has been established, we need (by my model) recordings of specific perceptual signals to serve as the pool from which specific values are to be picked to serve as reference signals. The KIND of perception has been established by the new connections. But the BEHAVIOR of that new signal depends on variations in the signals arriving via the new connections, and that is not determined by the connections. What we remember is the behavior of the signals, not the connections that create them. So we need a storage-type memory (of behavior of the world) as well as a connection-type memory (of the form of a perceptual control process).

There is a third type of "plasticity" possible. Through the use of inhibitory signals, higher systems can completely suppress the activity of lower-level perceptual or output functions, effectively removing them from the functional hierarchy. This sort of process (which I haven't even

attempted to put into the model) can VERY QUICKLY change one hierarchy of control into a completely different one, without altering a single connection or a single memory. More questions are raised by this possibility (probability). What happens to control systems that are taken out of action and then brought back in? Do they retain, somehow, the state they were in when turned off, so they can continue? Is this true of some kinds or levels of system, but not of others? There are potential experiments that could test such ideas.

All I can really do is to point to such elaborations. By the time they are ready for serious consideration, control theory will have advanced to the point where no one person could possibly follow out all the ramifications that need to be pursued.

I think we're OK for now at the level of understanding we have (as you implied in an earlier post). I agree with you that we ought to do as much as we can with the present simple form of the model, and wait until experiment and naturalistic data force us into adding complexities. We'll just have to let some questions go unanswered for now: write them down, and leave them for another generation to tackle with better tools.

The Benjamin Whorf material is immensely interesting. The Hopi language seems to contain specific and laconic reference to levels of perception! A simple prefix says that I am attending to this clearness at the transition level: downward-flowing. Another prefix refers to the configuration level: it is a downward-flowing object, a spring. I expect that with equal terseness, a prefix could change the meaning to "snowing."

Another interesting aspect is that the Hopi don't distinguish between internally-generated and externally-generated perceptions. As a result, they place imagination (and I suppose dreams, too) in the same reality as what we call real-time or objective perceptions. This must be very confusing!

Also, while they can refer very explicitly to reference signals, they clearly don't grasp HOW behavior controls perception. Not that this distinguishes them from foriegn scientists. They think, as a result, that they can set a reference level for a corn plant's growth in the same way they can set one for the act of picking corn. Even animals have this problem: when my cat sits in front of a door staring earnestly at the doorknob, it's perfectly clear to me that this cat has a reference-image of the doorknob doing its thing to open the door. This actually works quite often, but I'm afraid that my role in getting the door to open at last isn't fully appreciated.

This must be the basis for all magic. If willing can cause a hand to move or a spear to travel to its target, why can't it also cause the game animal to come closer or the rain to fall or the seed to sprout? There isn't any reason -- but for the theories of nature that you entertain. I should think that a Hopi scientist would be flabbergasted and delighted at the CT explanation of how wishes, prayers, hopes, and so on work. Know any Hopis who would be willing to listen? Could control theory be

expressed in Hopi?

This guy Whorf really speaks to control theorists, doesn't he?

After reading this stuff, I'm more than ever hopeful that linguists can help straighten out the levels of control. I don't so much mean through applying linguistic theories of language, but though the sort of in-depth grasp of differences between the world-views of speakers of different languages that Whorf shows. I speak physics, mathematics, programming, and English, and this has helped me greatly in getting away from the invisible influences of English. But I understand no other language sufficiently to see its structure as Whorf sees it. My levels, therefore, contain an unknown amount of influence of my own linguistic habits and assumptions, particularly in areas where my understanding of those other languages is irrelevant. When we see the same (presumably the same) perceptual world being described in different languages -- the more different the better -- we can discard the purely linguistic differences and perhaps see more clearly that to which these various forms are intended to refer. I'm sure you know what I mean by "we", here.

Rick Marken (910805) --

The new version of the Mind-Reading program sounds like a real jaw-dropper and eye-bugger. I can't wait to see it run.

There is one problem with using the disturbances as part of the calculations. Doing this is OK as long as you know what the disturbances are. But in most natural settings, I think, this is not so easy -- disturbances can arise in ways invisible to subject and experimenter alike. In that case, all you can do is look for the lowest correlation between action and controlled variable. This obviously won't let you make quick determinations -- you just have to sample long enough to get good statistics, as you found when you were using the stability factor. I think the latter is the general method.

But using information about the disturbances is going to make for a much faster determination of the controlled variable, as you know. You must have found some additional trick, though, to make it work in a "few seconds!" Derivatives? Don't tell me -- I'll find out next week.

By the way, David Goldstein almost decided he could come to the meeting, but last-minute airplane reservations cost too much. When I talked with him, he said you were coming WEDNESDAY? I had understood TUESDAY. Which is right?

Gary Cziko and Kieth Deacon: Would you post your arrival dates/times and flight numbers? We're trying to arrange to meet as many people as possible, to save them transport costs from the airport to Ft. Lewis College.

The number of people attending the meeting will vary from day to day because some people can't attend for the whole time. But we figure 30 at the banquet on Friday night, and a total of 38 people including guests

and children overall. A very nice size for the meeting.
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Date: Wed, 7 Aug 1991 09:06:02 -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: healthy personality

From: David Goldstein
To: members of CSGnet, Qtemp
Subject: healthy personality
Date: 08/07/91

I have described what I think the PCT (Perceptual Control Theory) image of the healthy personality is in Goldstein (1990). For those of you who have not read this paper in Wayne Hershenberger's book entitled "Volitional Action," I will be glad to send you a copy of it if requested. Just send me your regular mailing address (not Email). The description of the healthy personality in the paper was presented at a CSG conference and modified by the discussion.

As a follow-up, I thought it would be interesting to translate the results into terms of the Myers-Briggs Type Indicator(MBTI) variables of: extroverted(E) versus introverted(I), sensing(S) versus intuitive(N), feeling(F) versus thinking(T) and judging(J) versus perceiving(P).

As some of you may know, these are four individual difference variables which result in a personality typology of 16 personality types which I am taking as a description of 16 different types of self-images. The question I am asking is: Which of the 16 types of MBTI self-images correspond to the PCT healthy personality (at least my version of it).

To accomplish this, I made use of the information in the manual of the MBTI expanded analysis report (MBTI-EAR) to come up with a set of 40 adjectives (and their definitions). Then I q-sorted the adjectives so that the q-sort described the PCT healthy personality according to my conception of the implications of PCT.

Self-Image Study (n = 1)

The Healthy Personality

With the healthy personality description in mind, I used conceptual ranking of the MBTI-EAR 40 adjectives to arrive at a description of the healthy personality.

The healthy personality is (top 09 and bottom 07 items):

Most Like: Compassionate; rank = 17
Questioning; rank = 16
Theoretical; rank = 15

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                          Tender; rank = 15
                       Accepting; rank = 14
                       Affective; rank = 14
                      Systematic; rank = 13
                  Stress Avoiders; rank = 13
                      Expressive; rank = 13

Most Unlike:               Quiet; rank = 5
                         Emergent; rank = 5
                            Tough; rank = 5
                     Experiential; rank = 4
                        Receptors; rank = 4
                        Contained; rank = 3
                          Critcal; rank = 3

The results of the above q-sort of the 40 adjectives can be
scored to yield Myers-Briggs Type Indicator scores as follows:

            Extrovert score = 10 ; Introvert score = 5
               Sensing score = 9 ; iNtuitive score = 8
               Thinking score = 7; Feeling score = 14
                Judging score = 9; Perceiving score = 10

This results in a rank ordering of scores as follows:

Feeling(14)
Extrovert(10), Perceiving(10)
Sensing(9), Judging(9)
iNuitive(8)
Thinking(7)
Introvert(5)

And this is consistent with MBTI self-images of: EsFp, EnFp,
EsFj, and EnFj. The E and F prefererences are strongly present.
The healthy personality is both S and N; J and P to almost equal
degrees.

This means that as therapy progresses, one wants to see the
person become more E, more F, equally S and N, equally J and P.
This is based on the usual idea of therapy as helping a person
decrease psychological difficulties and increasing psychological
health.

This is an interesting result and should be replicated using
others. While the democratic side of me wants to think that all
people (self-images) are created equal, maybe they are but from a
PCT perspective they may not be equally healthy.
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Date: Wed, 7 Aug 1991 11:01:14 -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: Mind Reading Demo

[From Rick Marken (910807)]

Bill Powers (910806.0900) says:

>The new version of the Mind-Reading program sounds like a real jaw-
>dropper and eye-bugger. I can't wait to see it run.

>You must
>have found some additional trick, though, to make it work in a "few
>seconds!" Derivatives? Don't tell me -- I'll find out next week.

I don't want to get your hopes up too high. There is no additional trick; just the correlations (which is the original way I did the mindreading way back in 1982 or so). It just works nicely -- maybe it works in "several" seconds rather than just a few. But it can be quite astounding. The reliability of the algorithm is what gets me. I plan to extend the program to handle more possible variables -- like distances between pairs or groups of numbers.

I am arriving on Tuesday; see you Tuesday night.

Regards

Rick

****

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Date: Wed, 7 Aug 1991 15:10:54 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: quick response to Bill

[From: Bruce Nevin (910807 1044)]

Bill Powers (910806.0900)

Recursion: a property of a mathematical model which we sometimes reify as the ideal or "underlying" form of that which is modelled. Fooled once again into attributing properties of our description to what we are describing. I think that's right.

Might rapid change due to inhibitory signals taking control systems effectively out of the hierarchy may be what is involved in perceptual switches with e.g. Necker cube? I get a similar switch between big A and one of the little As comprising it. But this may not just be due to only one A-recognizer being available & being switched from one set of inputs to another: I get a similar sensation of switching in the configuration-of-configurations wobbly squares posted a while back.

(Kinesthetic component for me.)

Here's a question: in a gestalt-switch experience, where does the perception of the switching process reside? A transition (between configurations), but unsettling becasue we can't construe it as motion?

I thought you would appreciate Whorf. I hope you will get the Carroll collection and see what else he had to say. As you might imagine, his claims have been rather strainedly construed more often than not. Conversely, some of his statements about individual languages have been found debatable by others who have studied them.

Achumawi and great many other American languages have similar characteristics. Len Talmy wrote his dissertation on Atsuge (neighbor and close relative of Achumawi), dealing with the semantics of instrumental and classificataory prefixes in a way that I think goes even more deeply into these things than Whorf did.

>they place imagination (and I suppose dreams, too) in the same reality as
>what we call real-time or objective perceptions. This must be very
>confusing!

Not to the Hopi, but confusing for communication with many Anglos. But be it noted that there are many (intellectually marginalized) people in our Anglo culture for whom this makes natural sense. They call it by such words as intuition because it is difficult to articulate in the norms of our language and our accepted modes of communication. Whether this is a defect or simply a difference I am not in an authoritative position to say.

I don't know any Hopi, but my friend Alan Strain used to spend a fair amount of time with them. I have sent him a bunch of CT stuff, but he hasn't had time to do more than receive it, I think.

>When we see the same (presumably the same)
>perceptual world being described in different languages -- the more
>different the better -- we can discard the purely linguistic differences
>and perhaps see more clearly that to which these various forms are
>intended to refer.

Here, you come close to what motivates me. Learning languages is actually not easy for me, I have had to work very hard at it, but I have always had a very strong motivation to see the other aspect of the Necker cube, so to speak, and perhaps glimpse something more essential behind as the two perpectives switch back and forth. Here too is my interest in CT.

I am realizing that my plan to batch responses this weekend for email Monday runs against the schedule for the CSG meeting. Turns out I will be away this weekend--my mother's birthday Tuesday, and I am going with the kids down to see her, while Sarah goes off for a week to help her mother with recovery from a mastectomy. I'm sure that the net will be much taken up with ripples from the meeting the week of the 19th, but I will probably aim for then anyway.

Here's a thought that belongs there but fits with the Necker cube:

One control system per word . . . consider the case of the verb "to misle" in many people's childhood reading vocabulary. Some pronounce it "my-zle," I pronounced the s voiceless. I had it until about 12 years of age, when I suddenly connected two facts:

1. I only encountered this verb in the past tense or participle, with -ed after it.

2. I never encountered the participle or past tense of the verb "to mislead."

This word I pronounced "myscled" suddenly fell into place as mis-led. All the semantic and syntactic connections seem to have been in place, except for the derivational connection to mis- and lead. I could see it both ways in alternation, as in a gestalt shift. This is a shift from a construction of two sequences ("myscle" plus -ed) to a construction of three sequences (three morphemes mis-, lead, and -ed, plus a program reducing lead plus -ed to led). (The notion of construction I take to be on the program level, in operator-grammar terms of operators, arguments, and reductions.) This shift also has an interpretation as motion, or at least I get a similar kinesthetic feel to it as I look at the ambiguous string:

misled

We don't get the same effect with ordinary homographs like lead, read, etc. because the alternative readings are different in too many ways at once. The two readings of misled have connections to the same places in the control hierarchy--they are the same word, except for the phonemes and the peculiarly limited distribution of the phoneme string misle.

Well, enjoy the meeting. And don't let anyone misle you!

Bruce Nevin
bn@bbn.com

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Date: Wed, 7 Aug 1991 15:51:24 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: social references

From Ed Ford (910807.1600)

Rick Marken (910805)

>To the extent that inventions like penicillin and the pill reduce the
>chances of such consequences, more people will be willing to test new
>references for sexual activity. They will still get resistance from
>the majority but now more and more of the new generations of control
>system can try the new references with not only fewer natural
>consequences but less resistance from the fewer control systems
>around trying to defend accepted references. So this might be the

>way that technological change can "push" social references in new
>directions.

Having spent the last 25 years in a counseling office (among other places), I would say that presently there is emerging a growing change in references over the past few years and a perception of "natural consequences" not anticipated by those seeking "liberal" sexual and sex role references. The harsh reality is that all this new sexual activity has made creating satisfying relationship more and more difficult. As one physician client remarked recently, I hate dealing with "post orgasmic depression." It is my experience that humans learn more from their own created internal conflicts, that is from the consequences (other than social pressures) of their attempts to control a desired perceptual variable to a set reference signal than from having to deal with social pressures (disturbances), whether from home, cultures, organizations, or whatever. Sexual activity involves one living control system dealing with another but that specific activity involves only one very narrow and restricted aspect of our many relationships. Many are finding that kind of activity detracts rather than enhances relationships. And, there are many reference signals that go into making relationships. In human relationships, it is the value you see in others and their perception of value in you that brings the greatest enhancement. The question is - are the reference signals we set for building relationships really bringing long-term satisfaction? My experience in working with young people is that there is a growing trend toward a more conservative view of sexual activity, in spite of all the great scientific advances. Perhaps what really pushes social references is whether they bring continuing satisfaction over a long period of time. Occasionally, cultures test those references. Over the past 30 years, we seem to have done a lot of extensive testing in this country. Are we as a people a lot happier and more satisfied with our lives and especially with our relationships than we were 30 years ago?

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Date: Wed, 7 Aug 1991 18:29:00 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: Gibson, Gary, and Bizzi

(Rick 910726)
>You seem to think that Gibson is consistent with the control
>model -- even anticipating it. Please help me understand why
>this is so

Anticipating? Rick, you're putting words in my mouth--changing the subject. What I said was that Gibson recognized controlled perceptions, calling them executed perceptions or obtained proprioception. In his 1966 book, The Senses Considered as Perceptual Systems, he distinguished between what he called obtained perception and obtained proprioception. The former he described as being attentive/investigative (looking, listening),

the latter as being executive/performative (purposive behavior). He noted that the former "is the main topic of this book" but the latter is no less important. In his words,

"Obtained proprioception and the control of purposive behavior, the study of what has been called "cybernetics" is a new and important field of psychology and it is reasonable to expect that a great deal more will be known in the future about the 'steering' of voluntary actions and the execution of intentions than is known at present" (p.45).

Gibson was prescient. In 1973, a fellow by the name of William T. Powers published a modern classic on this very subject!

(Gary's Chapter 5)
>both classical and operant conditioning theory view all behavior
>as responses to external stimuli (including the stimuli caused
>by the behavior of other organisms)

Gary you may want to rewrite this sentence. Skinnerians claim that operant responses are NOT responses to external stimuli; that is, they are emitted rather than elicited. What about, "Both classical and operant conditioning theory view all behavior as being determined by prior stimuli which are either eliciting or reinforcing or both (including the stimuli caused by the behavior of other organisms)."

I like your chapter very much, Gary. It is an excellent exposition. Your evaluation of Tolman, for example, is right on target. Also, thanks for the note of credit--but why not simply cite my ABS article; I would prefer that?

(Bill 910826)
Bill, your draft of a rejoinder to Bizzi et al. is absolutely marvelous. Stay with it, Bill, because this is it! This is the superbowl. This is exactly the scientific confrontation for which you have been preparing yourself, whether you have realized it or not. This is a fortuitous opportunity, to be sure, but it is the perfect opportunity, nonetheless; don't pass it up. Bizzi not only has it wrong, he has it exactly wrong in a very public place; his thesis, the received view, is the antithesis of yours, exactly. He has unwittingly provided you with the perfect contrast--no middle ground and no place to hide. Whether you submit a letter, a technical note or an article does not matter. What matters is that you throw down the gauntlet in style and in plain view. I doubt that the editorial gatekeepers will even be much of a problem (they too will desert a sinking ship). Rick is right to be optimistic. I think we are, finally, on the verge of a paradigm shift. I can hardly wait.

On the other hand, what in heavens name is "real reality? Please, don't answer that.

See you all in Durango.

Warm Regards, Wayne

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Date: Thu, 8 Aug 1991 07:47:33 -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: Social control systems

[From Bill Powers (910808.0600)]

Bruce Nevin and Rick Marken (910806) --

... with an implied challenge to our social scientists.

>There is no forest, only the trees, eh? There is no society, only the
>people (control systems) in it . . . there is no person, only the
>control systems in her. . . . So long as we don't shift from one kind
>of thing (control systems) to another (neurons), you might be able to
>get away with this reductionism.

Reductionism results when you ignore higher levels of organization: Bach spent his life drawing little slanting dashes or dots with vertical stems on pieces of lined paper. I'm not sure what the name would be for the sin of extrapolating a theory to the point where it turns into a metaphor: synthecism? Synectady (in New York)? Argument by analogy?

The control systems inside a person consist of specialized input and output functions, with comparison processes variously achieved. These structures appear to exist independently at many levels in the brain. One of the levels organizes a person in an encompassing way we experience as being "a person" (and recognize in others through what we perceive of organization in them). Each person is a structure of interdependent systems and many levels.

In a society, there are no people who devote themselves to one level of function only, or to one specialized function in a single control system. It is impossible for a person to behave in such a way and live. So the control hierarchy in a single person stops at the highest level in that person; there is no way to continue it to a higher level outside the person. It probably continues downward through the biochemical rather than behavioral branch, however (starting roughly at the level of the hypothalamus) all the way to the inner working of the genome.

People have attempted to form societies organized as artificial control systems. As a society is envisioned by many people (including some in the White House), there are social mechanisms for monitoring the actions of

individuals (complainants, informers, covert and overt investigative agencies, panels of experts, news media); comparison processes for detecting deviant behavior (definitions of disease, insanity, torts, and crimes, definitions of health, obligations, and duties); and procedures for correcting deviant behavior (penalties prescribed by law for each wrong or crime and each omission of duty, treatments indicated for each deviant mental condition or illness or each incipient departure from health). This system is supposed to operate automatically because the specifications for all of its parts are written down -- and fairly, because it is automatic and applies unformly to everyone for the benefit of society as a whole. Like any control system, it is supposed to control through opposition to disturbances, the opposition adjusting from mild to overwhelming as befits the size of the disturbance.

This concept of a society is a natural mistake born of each person's need to have control of the experienced world. This mistake has been made over and over. Some people have tried to devise utopias and anarchies to get away from the flaws of the social-control design, without remarkable success except perhaps on a very small and localized scale. But most people are persuaded that we need law and government and medical treatment and the like: social control for the good of the many.

The greatest problem with this concept of an artificial social control system is that it comes into direct conflict with the basic nature of the individual, which is to control himself, herself, or (if living) itself. So each individual breaks the laws and flouts the rules of health in small and large ways every day, and devises means of not getting caught. The voters vote for the control of other people and against control of themselves. The powerful maneuver to obtain maximum freedom for themselves, and minimum freedom for the rest (particularly for those who would also like some power). The wealthy try to free themselves from restrictions on how to spend what they have and how to accumulate more, and try to set conditions that prevent others from taking back some of the wealth.

Each person wants to use this vast automatic machine as a means of controlling what happens to himself or herself. Thus individual freedom is in constant conflict with the social control system that has been set up for the good of society.

The greatest flaw in this concept of an artificial social control system is that it is not and cannot be automatic, running independently of any individual's whim. In fact it is run by individuals and is constantly subject to individual whims. I will leave it to Hugh Gibbons, our leading thinker on law and control theory, to explain just why this social control system is not in fact a control system and cannot possibly run automatically. Nor, as Gibbons clearly shows, does it in fact run automatically.

I will content myself with a few observations to end this comment.

The main one is that there is, in fact, no System. I said this in my '73 book and I still believe it. You can walk into any bureaucrat's office and all you will encounter is a person. When you stand before a judge, you do not stand before the law, but before a person who listens to you

(and, too bad for you, others) and tries to make sense of everything in terms of what the judge remembers and understands and wants of the written law. A different judge (or jury) will hear differently, understand differently, want differently -- and decide differently. The clerk at the driver's license desk can make it easy for you or endlessly difficult. The county assessor can be reasonable or implacable. The System consists of people, all of whom are different. You will never encounter anything but the people and their individual wants and desires.

The worst nightmare of anyone who has grown up in a free society is to lose that freedom, that independence from external control. As examples of threats to freedom, to what do we point? To dictatorships, whether of the proletariate, the armed and dangerous, the religious, the politically ambitious, or the deranged.

And what is a dictatorship? It is a system devised so as to exert social control exactly through the kind of automatic control machinery described above. What we fear most is law that is applied blindly and without regard to circumstances, by the book; force that is applied without regard to our wishes; goals that are imposed on us without our inner acceptance; duties that are demanded of us without consideration of what will satisfy us; loss of control over our very bodies, our very Selves, our very lives.

Even the freest nations in the world are still hanging onto the old forms, the old convictions that we need an automatic social control system that is not just human interaction but something larger, more protective, more powerful. Yet the freest nations are what they are precisely because the individual's need for autonomy has prevailed to some degree over the very system that people are convinced is needed to protect their freedom -- and which, in fact, may be needed to protect them against other people who would impose their rules even more strictly and thus go even more harshly against autonomy.

But this is not where we are headed -- toward the perfect social control machine. We are headed inevitably toward something else. I can't say what it will be -- we have yet to work it out. Understanding that social control systems are an illusion and a threat to freedom is the first step in working it out.

Best

Bill P.

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Date: Thu, 8 Aug 1991 08:43:56 -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 references, Gibson

[From Rick Marken (910808)]

Ed Ford (910807.1600) says:

> My experience in working with young people is

>that there is a growing trend toward a more conservative view of
>sexual activity, in spite of all the great scientific advances.
>Perhaps what really pushes social references is whether they bring
>continuing satisfaction over a long period of time. Occasionally,
>cultures test those references. Over the past 30 years, we seem to
>have done a lot of extensive testing in this country. Are we as a
>people a lot happier and more satisfied with our lives and especially
>with our relationships than we were 30 years ago?

You may be right. I was just suggesting that technologies may create a groove ("push" was probably the wrong word) that makes it easier for certain references to change in one direction rather than another. I used the example of sexual activity, not because I am in favor of a particular direction, but because it seems like there has been a change in the majority "reference" for, say, "women's perceived role in society" that seems to have been made particularly feasible by certain technologies (the pill, safe abortion practices, microwave ovens -- so I can cook dinner instead of Linda). I think you are suggesting the possibility (which I mentioned also) that there may be fairly universal "intrinsic references" that prevent the inertial drift in references from straying too far. I agree that this seems like a reasonable possibility -- societies have tried lots of different sexual mores (references for sexual principles) but none that I know of settled on an average norm that encouraged, say, incest (except among a select group of individuals, as in the royal families of Hawaii). So I am just suggesting that the "inertial reference draft" that Bill discussed could tend in one direction rather than another at particular points in history as the accidental
side effect of the development of certain tools. I think James Burke was making this point in his marvelously entertaining "Connections" series.

I do not believe that these technical developments act as some kind of "invisible hand" that acts as a reference signal outside of people that specifies how they should change. I believe, as Bill Powers (910808) said in his latest post, that the only references for how things "should" be in society exist in the individual members of that society. Actually, this concept once lost me a job. I was interviewed for a position with a law firm many years ago. I guess they wanted a psychologist as an expert witness or something. Anyway, one thing they asked was whether I believed that "companies" are entities in themselves -- or just the sum of the people that make them up. The goal was to see whether I believed that people (like the company presidents, vice pres's, etc) were liable when the policies of the company led to harm, or whether it was "the company". I said the former -- since "the company" was defined by the understandings, goals and perceptions of the individual members -- and never heard from then again.

Wayne Hershberger (910807) says:

>Anticipating? Rick, you're putting words in my mouth--changing
>the subject.

Oops, sorry. That's what I get for not quoting directly from your post. Sorry.

>   What I said was that Gibson recognized controlled

>perceptions, calling them executed perceptions or obtained
>proprioception.

>In his words:

>"Obtained proprioception and the control of purposive behavior,
>the study of what has been called "cybernetics" is a new and
>important field of psychology and it is reasonable to expect that
>a great deal more will be known in the future about the
>'steering' of voluntary actions and the execution of intentions
>than is known at present" (p.45).

OK, you're right. Gibson did talk as though he understood purpose
(at least as well as William James did 80 years earlier). But I still
think he has no model (reality 2) of how reality 1 maps into reality 3
(Bill Powers' terms).

I look forward to seeing you in Durango.

Hasta Luego

Rick M.

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Date: Thu, 8 Aug 1991 14:14:00 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: On choosing what is expected

[From: Bruce Nevin (910808 1208)]

OK, no waiting on these two topics then. I've got some time over lunch.

[From Bill Powers (910803)]

>Rare experiences are experiences that few people have or that individuals
>have only rarely.

People perceive that such experiences are rare. People are able to
perceive that other experiences are much less rare, much more expectable
or likely. Whence these perceptions of rareness and likelihood?

I assume it has to do with memory. At one extreme, experiences of which
we have no memory, not even any memory of someone telling us about them,
or about experiences of the "same kind" (category generalization).
Other experiences are more familiar because we have memories of them,

the more memories the greater the familiarity.

This is how I assume it works for words; I believe it should work likewise for other perceptions. I'll describe it again, for the general case. I perceive A. This evokes memories of having perceived A before. A memory that involves A on occasion o does so by some associative connection (mechanism TBD) between a control system for A and those for other perceptions on occasion o. Imagine the associative links for all the occasions o, o1, 02, etc. on which A has been perceived. Imagine them overlaid to form a mesh. Some links, and therefore some nodes (control systems) other than that for A, are repeated. On many occasions when A was perceived, B was also perceived--either both under higher-level control, or not. On occasion o, if I perceive A and B yet again, I judge that cooccurrence likely. If I perceive A and X, and X nowhere occurs in the mesh associated with A, I judge that unlikely. (Other things follow too, such as failing to notice that B is in fact not present, an error due to the fact that B is so strongly expected.)

It is too bad that the mechanism of associative memory is unspecified.

Bill Powers (910805.0900)

What you are resisting is a notion of suprapersonal control systems. But you don't have to assert that to talk about structures of social convention.

I am asserting that language has structure that can be observed and studied not only in the outputs of language users (speech, writing), but more especially in the results of testing for what it is they are controlling for in their use of language.

Assuredly, they can control for reference values of any kind only after having assimilated them into their own control systems. My only claim is that there *is* something there to be assimilated, pre-existent in the linguistic outputs of other language users and in their resistance to perceived error.

This structure is there because people cooperate to learn it, assimilate it as their own individually, and maintain it as their own collectively. By this last I refer to the fact that control of language and dialect is one very important means by which people identify the membership or nonmembership of people in groups to which they refer as "us" and "them" and "we" and "you."

Yes, this structure exists there in the language-learner's world of experience only by virtue of other people's individual control of perceptions. I do not deny that. Nor do I assert that there is some supra-personal control system governing it. I only assert that it is there. It is present not just in that individual who is currently teaching the child by precept and by example, but in a number of individuals, and on many occasions, so that the example is not isolated but rather is an example precisely of agreement and communal synchrony. Individual idiosyncrasies are also interesting and children learn from them, but it is the fact that they are shared and indeed must be shared to function that gives special appeal and importance to structures like

those of language. The structural facts of a language are not rare, they are expectable and expected.

It is there not because some superordinate control system sets reference values to which the individual people are compelled to conform, in the way that a control system for a certain kinesthetic sequence must, if stimulated, control for repeated nodding of the head and cannot do otherwise. Such compulsion is inimical to our nature as autonomous control systems. The coordination among individuals must be a voluntary agreement. We agree to refer to that blue, sometimes cloudy expanse above as "sky" and not as aseH'la. We do not remember having agreed to it, nor do we remember what must amount to many hundreds of thousands of other agreements by which we came to be persons recognizable as members of our families, various groups of friends and cohorts to which we have belonged, and other social groups and systems. Where with our fellows occasionally we perhaps forged new agreements, we did so by adapting what was already there, not by creating anew like the mythical Adam assigning names and attributes in the Garden.

Nor do we feel free to undo such agreements. We could invent new words for things only at the expense of dismooring ourselves from the linguistic continent of English and all its inhabitants, and that for a great many reasons we choose not to do. Having made that greater choice, we find ourselves not free with respect to the lesser ones that make it up. It is precisely so for the child learning the language. Given the commitment to participate in the ways of being human that are normal for her family and friends and community, it is as though all those others could reach in and set reference values within her control hierarchy for "sky" and myriad other matters of arbitrary, but (crucially) shared convention. It is as though she actively offers up these comparators within her to be set by the others around her. She is alert for evidence of disturbance, acutely observant and mimetic, and during the early years of most active language acquisition is quite amenable to explicit correction, especially by example.

In the process over many many generations of many many people *individually* "avoiding direct conflict and resisting disturbances" they have *collectively* created structures that are not mere dissipative systems like a vortex or a sandpile precisely because each participant (unlike grains of sand) has agreed to participate and controls for participation. These systems of agreements, in various aspects of language and culture, are of great complexity, elegance, and beauty, and are most worthy of study and appreciation. As Sapir observed, they are like collective works of art, which some individuals are more able to display and use than others, and which no individual holds entire. Through them, individuals not only make known to their fellows their membership, but what their contribution in membership might be. I mentioned some time ago the unanimity of attributing personality characteristics to voices (actually of the same few speakers) that differed only in particular vocal qualities. From such subtle differences ("Hiii! From particular the way I'm laryngealizing you will suppose that I am gay.) to obvious mastery of exposition and explanation and other skills that matter to the community, we advertise our public worth, our obligations, our wants and needs. All of these are arbitrary (though they may have biologically innate roots that have

been culturally refined and reinterpreted). What counts as an announcement of availability to serve a manifest social need in one community counts as arrogance in another and irrelevant posturing in a third.

Control Theory provides a crucial moiety that has been missing from the study of what human beings are and do, but it still must be seen as incomplete, as intersecting another perspective. This other perspective is concerned with what human beings externalize among themselves for the sake of relationship with one another.

(A)
>PCT tells us that all rules, conventions, laws, and so on (by
>which people actually live) must, in order to be effective, exist as
>reference signals at the appropriate level inside each person.

(B)
>They do
>not reside outside people, even when they're written down or present as
>physical constructions. They are not implemented by any mechanism outside
>individual human beings. There are no natural control systems outside the
>individual human being. Not even in a society.

(B) does not follow from (A). It is simply asserted. I say (C):

They do reside outside people, because that is where people put them, and they are implemented not by any mechanism outside individual human beings but precisely by those individuals, as autonomous (not independent) control systems *voluntarily* conforming to them. They do this for the sake of cooperation with other human beings. They do it because if feels good to belong--because there exist control systems that they have in common with all mammals (says Bateson) that control for relationship.

The study of these structures to which people agree to conform is not merely the study of behavioral outputs. Your critique of S-R theories makes it clear that we can only learn about them by testing for control, and that is precisely what the techniques of linguistics do. It does not present anything like a statistical average as "results" because the objects and relations studied are precisely defined as reference values for individual control. It is different from the study of control of, say, locomotion, because people have placed arbitrary constraints on the degrees of freedom normally available for control. They have done this stylizing and conventionalizing so as to differentiate membership from non-membership, relationship in the social sense from relationship merely in the sense of physics. This range of choices is in, the rest is out.

These things have been abused as matters of coercion, and will be, but they are not inherently so. They can be matters of play and mutual enjoyment, and often are.

What is lacking for there to be suprapersonal control systems is means for *setting* reference values from outside the person. The agreements I talked about depend upon means for communicating or transmitting or

advertising reference values, but the setting of these values is a matter of voluntary (or coerced) choice in each individual.

As Camus put it, we are condemned to freedom. We act as though we don't like our freedom, we seem to give it away as quickly as possible. For the most part, we do so for the sake of participating with others in some unity larger than any of its participants. As we grow, we become more discriminating.

Does this make for consensus?

Bruce Nevin
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Date: Thu, 8 Aug 1991 12:03:16 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Eric Crump <LCERIC@UMCVMB.BITNET>
Subject: social systmes

Bill--Some good points in your last post. Enlightening to a CT novice like me. But you lost me when you said:

"...greatest flaw in this concept of an artificial social control system is that it is not and cannot be automatic, running independently of any individual's whim. In fact it is run by individuals and is constantly subject to individual whims. I will leave it to Hugh Gibbons, our leading

The main one is that there is, in fact, no System. I said this in my '73 book and I still believe it. You can walk into any bureaucrat's office and all you will encounter is a person. When you stand before a judge, you do not stand before the law, but before a person who listens to you (and, too bad for you, others) and tries to make sense of everything in terms of what the judge remembers and understands and wants of the written law. A different judge (or jury) will hear differently, understand differently, want differently -- and decide differently. The clerk at the driver's license desk can make it easy for you or endlessly difficult. The county assessor can be reasonable or implacable. The System consists of people, all of whom are different. You will never encounter anything but the people and their individual wants and desires."

Is that intentional oversimplification to make a point? or do you mean to suggest that the system in no way limits behavior, that the license clerk will do whatever the license clerk wants to, no matter what? That's the way I understood the above. But surely the clerk's (and other bureaucrats, since we're using them for example) behavior is moderated at least by the rules of the system. If your paper work is in order, there is only so much the clerk can do to make your life miserable. Admittedly, such people as clerks and judges can be agreeable or not, and can have an effect on how the system operates, but if they play too fast and loose with the rules, and get caught, the system has mechanisms for "correcting" their behavior. Other people, playing by the rules, enforce them.

So, although you are right that we do encounter people and their individual desires, I would question the "anything but" and offer that we encounter desires *and* the system, desires in some measure reined by the system.

Eric Crump
University of Missouri

Date: Fri, 9 Aug 1991 08:14:38 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: What's the weather like?

From Ed Ford (910809.0830)

For Bill and Mary -

It being 113 degrees in Phoenix, it's hard to imagine cool weather. It would be helpful if you'd give us a hint as to what the weather is like in Durango. Things like the need for sweaters, how cold at night, how warm during the day, is there much chance of rain, usual stuff, etc. Thanks much.

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Date: Fri, 9 Aug 1991 10:11:37 -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 structures

[From Rick Marken (910808b)]

Bruce Nevin (910808 1208) says:

>What you are resisting is a notion of suprapersonal control systems.
>But you don't have to assert that to talk about structures of social
>convention.

>I am asserting that language has structure that can be observed and
>studied not only in the outputs of language users (speech, writing), but
>more especially in the results of testing for what it is they are
>controlling for in their use of language.

OK -- language structure might be a controlled variable. But that controlled variable cannot be seen just by looking at the language. That's the essential point. It may be "out there" -- but there are many possible structures out there. The goal is to find out what people are controlling. So looking for structure in the language itself is like looking for "affordances" in the environment or for the reinforcing properties of food. You might perceive interesting structures in language, but are likely to be side effects of what the language user is actually controlling for, just

as the three dimensional movement of the e. coli bacteria is a side effect of its control of a unidimensional quantity (e. coli cannot perceive in more than one dimension).

Eric Crump (910808) says (to Bill Powers):

>   Admittedly, such people as
>clerks and judges can be agreeable or not, and can have an effect
>on how the system operates, but if they play too fast and loose with
>the rules, and get caught, the system has mechanisms for "correcting"
>their behavior.

But what is the system that perceives that they are playing fast and loose (deviating from a reference) and that can do things that can (maybe) correct the behavior? You answer this in your next sentence.

>   Other people, playing by the rules, enforce them.

Then you say:

>So, although you are right that we do encounter people and their
>individual desires, I would question the "anything but" and offer
>that we encounter desires *and* the system, desires in some measure
>reined by the system.

But you yourself have defined the system as "other people" -- so I think you make Bill's case -- "the system" is enforced by the desires of people *and* by the desires of other people. What else could do it? There is no system other than the people who act to carry out their individual goals and, in so doing, allow you to perceive in the theircoordinated behavior a "system". This "system" is actually a perception in you; it is not an entity "out there" (same goes for linguistic rules).

Hasta Luego

Rick M.

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Date: Fri, 9 Aug 1991 13:44:09 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: language problems

[From: Bruce Nevin (910991 1340)]

Is there a distinction between language and the speech or writing by which we know about it?

On the face of it, examples like the transcript that Martin Taylor gave us would seem to suggest that there is. We carry around within us a model or standard of spoken English to which that string of words and others that people often produce do not conform. And as Martin pointed out, we "normalize" what we hear, reconstruct what we suppose we should have heard, and more often than not firmly believe that that is what we in fact did hear.

The idea that the heterogeneity and variation of actual speech should be taken as derivative from an underlying ideal, imperfectly executed, has deep roots in the use of language (along with many other things that we humans do together) as means to define and maintain social membership and distinguish "us" from "them." In the usual view exemplified by conservative pundits like the Williams Buckley and Safire, one of the variant dialects of a language like English is correct and all others are corruptions of it. Call this view Standard Linguistic Outsider Bigotry (SLOB). It is not difficult to show how SLOB, were its logic prosecuted consistently, would lead to absurd and self-defeating conclusions. That is not the present aim. (It also may be seen that I do what I can to conform to SLOB expectations for English, lest what I have to say not be well received.)

Ferdinand de Saussure, a Swiss linguist at the end of the 19th century and beginning of the 20th, and a revered progenitor of descriptive linguistics, distinguished speech (parole) from language (langue). The idea that an idealized, invariant structure may be abstracted from the observation and analysis of records of speech, and that the heterogeneity and variation of actual speech should be taken as derivative from it, would seem to be a close relation to SLOB. Perhaps each is the evil twin of the other. The crucial difference is in the applications. SLOB is a promiscuous Don Juan that would meddle in everybody's affairs. The new view has a platonic relationship only to the Inner Truth of its paramours, the Reality that underlies all the many sullied forms of actual speech, and interposes a cool, non-prescriptivist distance between itself and all the messy heterogeneity of speech--the same detached relationship as it just happens to find between Language and speech, but that is surely coincidence. I suppose we could call this a Proper Intellectual Modality (PRIM).

The contemporary and perhaps better known variant of PRIM is Chomsky's distinction between linguistic performance and linguistic competence. We are fortunate that no bigot has understood his biologicism well enough to advance a claim that ethnic groups differ in their biological endowment for language. Such a racist claim would be unfalsifiable, as of course the biologicism of generative linguistics is itself incapable of test. Much interesting and valuable results have come of the search for linguistic universals motivated by the supposition that languages are alike in all but superficial matters ("they're all alike under the makeup"); much harm has come of the implicit judgement that the heterogeneity of languages *is* superficial and of interest and value

only insofar as it discloses something about Universal Grammar. A theory is known by the questions it begs as well as by those it answers, for both are among the fruits by which its lasting value is determined.

Harris appears to be neither SLOB nor PRIM. For Harris, language structure is merely the scientific arrangement of speech, no more, as he put it in the following passage from a review in 1941:

> It is not necessary for us to agree on our idea of the nature of a phoneme: whether we are to understand it as a class of sounds (each sound being itself a slice out of a continuum of sound), or regard it as some new entity containing a `characteristic' sound plus an on-glide and an off-glide. For linguistic work it suffices to know how to recognize the phonemes of a language. But Trubetzkoy offers a specific picture of the phoneme as a `functional' sound: "The phonologist considers in the sound only that which fills a specific function in the language system" (14). And having established such units of function, he speaks of language structure, in contrast to speech, as "something general and constant" (5). Such talk may be considered a matter of taste. It makes no difference what picture each linguistic worker has of a phoneme, so long as each performs the same operations upon it.
>
> The Prague Circle terminology, however, has two dangers: First, it gives the impression that there are two objects of possible investigation, the _Sprechakt_ (speech) and the _Sprachgebilde_ (language structure), whereas the latter is merely the scientific arrangement of the former. Second, talking about function, system, and the like, without defining them in terms of operations and relations, fools even the linguistic worker. For by satisfying him with undefined psychological terms it prevents him from continuing his analysis. Thus Trubetzkoy says that each word is, in the language structure, a _Gestalt_, and that it therefore "always contains something more than the sum of its parts (i.e. of the phonemes), namely a unity (_Ganzheitsgrundsatz_) which holds the sequence of phonemes together and gives the word its individuality" (35). Had he not been satisfied with such words, he would have been forced tos seek for the physical events which enable us to consider the word as a unity and not merely a sequence of phonemes. And he would undoubtedly have realized that this physical event is usually the `zero juncture' (see below) defined as the juncture between phonemes of one morpheme (or the like) in contrast to other junctures. Had he recognized this he could not have written his next sentence: "In contrast to the individual phonemes, this word-unity cannot be localized in the body of the word."
>
> Review of N.S. Trubetzkoy, _Grundzuege der Phonologie_, by Zellig S. Harris, _Language_ 17.4:345-9 (1941).

Harris's attitude caused some to accuse him of "game-playing" with linguistic data, a controversy that Fred Householder once famously characterized as distinguishing "God's Truth" linguists from "Hocus-Pocus" linguists. ("You can't get away with cutting off a heel to fit the slipper, I want the Real Cinderella!")

In Harris's operator grammar, the informational (base) sublanguage whose grammar comprises only the operator-argument relations without the reductions (or with only obligatory reductions, as for plural -s and definite article the) is analogous to Chomsky's notion of deep structure, but it is no reification of something mysterious, beyond or behind mere speech, of different kind and implemented by different neurological and psychological means. It is of the same kind, and implemented by the same parts of the control hierarchy as the more ordinary sentences in which reductions have introduced all the variety and heterogeneity of the speech community. There are no abstractions or theoretical constructs like the ECP (Empty Category Principle) to be reified, their unique implementations to be sought severally in the neural connections of the brain. The same control hierarchy that suffices for events (actions), relationships (such as dependencies among words), categories, sequences, and programs for ordinary speech suffice also for the grammatically regular and informationally complete sublanguage that is available when one simply does not carry out customary but optional reductions.

I believe with CT and operator grammar we can have it both ways--we can model linguistic competence and linguistic performance and (more importantly) the connection between them, showing how the former arises out of the control systems and reference levels by which we manifest the latter. We can describe regularities in speech not only with reference to processes of idealization (categorization) involved in the control of language (comparably with many other aspects of human experience), but also with reference to the institutionalization of language norms and expectencies that users of a given language learn from and adapt in its externalization among themselves. We can describe language not as a platonic ideal which speech imperfectly reflects, but as the intentions which speakers carry out with a degree of adequacy which is determined, as in all their control processes, by trial and error. We can move beyond both the anaemic mooning of PRIM and date rape by SLOB to a real, working marriage.

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

Of the first five levels of the control hierarchy I believe we can say that each builds on the prior:

5. Event, act (short, unitary, well-learned sequence)
4. Transition (smooth(ed) change = motion)
3. Configuration
2. Sensation
1. Intensity

Bill (BCP:131) points out that level-4 transition or change can apply to any perception of levels 1-3.

Of levels 6-8, it appears that each can be meta to any of 1-5. It is difficult for me to perceive intensities except by by comparing them in relationship or in sequence, and one can speak of them only by way of categories for them (to which words attach). And so on for sensations, configurations, transitions, and events. It also appears possible to

make any of 6-8 meta to any of the others -- relationships between sequences, relationships between categories, and categories of sequences, as well as the reverse. Bill (e.g. BCP:139) gives examples of sequences of configurations and suggests that there are sequences (ambiguously both events and sequences) of lower-level perceptions of various kinds.

It may be argued that e.g. a dependency relationship between the phoneme or letter sequence "sing" and the word-class "noun" (its argument requirement) is part of a program. I note that there is a program-like property of sequences as implemented in BCP: the n+1th recognizer of the sequence is activated only IF the nth was activated, as well as IF the lower-level perceptual input is forthcoming. But there is no choice-point in a sequence, no branching. Either the sequence completes, or it does not, languishes, attenuates, and is no longer active. Either the third phoneme of "sing" is perceived or it is not, and whether or not some other phoneme, such as the t of "sit," is perceived is immaterial to the control system for the "sing" sequence. So it is I believe with a mechanism for "expecting" some other word to meet the argument-requirement of the current word: the expectation may be met by the memory of a recently uttered word, by the input of the word anew, or by input of a signal that the conditions for its zeroing have been met, otherwise the recognition process employing that word languishes and attenuates and is no longer active in the pandemonium pool of candidate interpretations (subtrees) for the linguistic input -- no choice-point, no branching.

I suspect we need the program level for the reduction system, because here we have the choice in most cases whether to make a given reduction or not, and in some cases perhaps how far to carry a reduction that is available in varying degrees.

I suspect we need the program level for the organization of complex sentences into discourses, for the foregrounding of topic against a background of support information, presupposition, and reference to shared knowledge, for the planning of logic and rhetoric, and so on.

Principles avowedly have to do with categories or types of programs. This supports my contention that we can apply category-level perception, and hence the words of language, everywhere.

The existing of taxonomies and associated hierarchies of classifier words in many languages, particularly in technical sublanguages, is the source of a lot of messiness that doesn't map well onto the perceptual hierarchy. The problem is that we get categories of categories of categories, to indeterminate depth. I don't know what to do with them. They seem to be more than simply verbalisms -- definitional facts in the language but not perceived as categories in the world of perceptions. We do have a kind of category-perception of insects as distinct from spiders, and of both together as distinct from reptiles and mammals. I don't think it is a problem that these hierarchies intersect in sometimes contradictory ways -- "fruit" and "vegetable" participate in more than one taxonomy, including horticultural and culinary taxonomies, and a tomato *is* a fruit from a gardener's point of view, and it *is* a vegetable from a cook's or eater's point of view.

Bruce Nevin
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Date: Fri, 9 Aug 1991 14:26:49 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: structure vs. affordances

[From: Bruce Nevin (910991 1356)]

Rick Marken (910808b)

>>. . . language has structure that can be observed and
>>studied not only in the outputs of language users (speech, writing), but
>>more especially in the results of testing for what it is they are
>>controlling for in their use of language.

>OK -- language structure might be a controlled variable. But that controlled
>variable cannot be seen just by looking at the language. That's the
>essential point. It may be "out there" -- but there are many possible
>structures out there. The goal is to find out what people are controlling.
>So looking for structure in the language itself is like looking for
>"affordances" in the environment or for the reinforcing properties of food.
>You might perceive interesting structures in language, but are likely to be
>side effects of what the language user is actually controlling for, just
>as the three dimensional movement of the e. coli bacteria is a side effect
>of its control of a unidimensional quantity (e. coli cannot perceive in more
>than one dimension).

Rick, you can only say this because you are unfamiliar with how much can
be done with the distributional relationships of the entities of a
language.

First, let's make sure you are looking at the structure in the language,
and not at the speaker's intentions in a particular utterance.

Beginning with the phonemes (which you have determined by the pair test,
a test for controlled perception), you find a recurrent-dependency
process such that in a large number of utterances for every string of n
phonemes, the number of possible next-successor n+1th phonemes decreases
and then jumps back up to a range at or near the full inventory of
phonemes in the language. The points of greater freedom of
combinability are (to a very good first approximation) the morpheme
boundaries in those utterances. This structure is in the corpus of
utterances. It reflects the arbitrariness of what constitutes a
morpheme in the given language ("glib" but not "flib", "flip" but not
"glip"), which is a matter of convention which must be learned.
Anyone learning the language can exploit this distributional fact
to learn it, though is of course not limited to this, that is not the
point. Similarly for dependencies among the morphemes. They can be
grouped into classes not because someone tells you that they are, nor
because they correspond to nonverbal perceptions that are categories in

your own perceptual hierarchy as a language learner (a very treacherous basis for learning a second language), nor even because other people correct you, but because they observably cooccur only in certain combinations in a large collection of utterances. One cannot learn to control a language like a native on the basis of this degree of structural information alone, but that is not the point. The point is that the structure is there, in the utterances of the language, where it can be observed, analyzed, and made one basis for learning. And so on for other levels of structure in the language. This structure is not apparent in any single sentence taken in isolation, but only by comparison and analysis of a great many utterances.

Looking for structure in the language is not gazing at a single sentence and expecting to see its structure as one might gaze at the jungle gym on a school playground and perceive its structure. It requires memory, and juxtaposition of parts of many sentences in various ways.

Looking for structure in the language is not like looking for "affordances" in the environment or the reinforcing properties of food because no one put the affordances there or the reinforcing properties there but people put the structure in the language. They learn it from other people and then they put it there in order to participate in what the other people are doing. In *using* language in a particular utterance a speaker is controlling for many things, including conformity to the social conventions reflected in the structure of the language, because with too great a departure from them they would no longer be participating in what their hearers are doing with language and what they said (no matter how well they controlled the other things) would be less intelligible and eventually unintelligible. It is not a side effect of control, as with movement of e. coli, it is among the reference values for control.

Bruce Nevin
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Date: Sat, 10 Aug 1991 07:51:11 -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: language and PCT

From: David Goldstein
To: Bruce Nevin, Bill Powers, others
Subject: language and PCT
Date: 08/09/91

I have been liste
you have been leading on
language and PCT. Very interesting, important but complicated!

Which theory of language do we try to mesh with PCT? Your choice is that of Harris. The choice of which language theory to integrate with PCT makes all the difference in the world. It defines what is language and therefore, what is not language. Before bending PCT out of shape to accomodate a theory of

language, maybe it would be worthwhile to discuss this background issue.

How do we go about making the proper choice, assuming that we are not going to create our own theory of language. Can you explain to us how you made your choice out of all the possible theories of language which exist? What were the considerations which lead you to chose Harris, in particular?

As I have indicated in a private post to you, my own vote goes for William Diver's theory of language. Diver is a retired professor of linguistics at Columbia University. I am familiar with Chomsky's generative grammar approach having studied it for a few courses and having conducted at least one analyis within the framework of this approach.

I will be glad to give a brief description of Diver's approach in a separate post but I think it would be more helpful now to discuss the issue of how to integrate PCT with other theories. This applies to language as well as other fields.

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Date: Sat, 10 Aug 1991 09:06:00 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: Gibson

[From Wayne Hershberger (910809)]

Rick Marken (910808)

>Gibson did talk as though he understood purpose (at least as
>well as William James did 80 years earlier). But I still think
>he has no model (reality 2) of how reality 1 maps into reality 3
>(Bill Powers' terms).

Yes. You are right on both counts. Gibson's understanding of purposeful behavior was good, but imperfect--that was not his specialty. Further, he certainly offered no account of how reality 1 maps into reality 3 (Bill Powers' terms). Perception, according to Gibson, is NOT a process by which a reality 3 is approximated by a reality 1. However, in this respect (rejecting representationalism) Gibson is championing an epistemological position which appears to me to be as true to control theory as Bill's. (In some respects they are virtually identical: Bill says that perceptions are signal based whereas Gibson said that perceptions are information based; are not the two terms synonymous?) Gibson is an ally rather than an adversary of control theory, at least as I read him. Others should decide for themselves. Inasmuch as this requires their reading him, that is what I recommended, and still do.

I am looking forward to seeing you (and your updated mindreading program) in Durango next week.

Warm regards, Wayne

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Professor of Psychology
Department of Psychology Home: (815) 758-3747
Northern Illinois University
DeKalb IL 60115 Bitnet: tj0wah1@niu

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Date: Sat, 10 Aug 1991 15:04:59 -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: PCT and Diver

From: David Goldstein
To: Bruce Nevin, Bill Powers, others
Subject: Diver and PCT
Date: 08/11/91

The following discussion comes from a paper I wrote in 1971 on the "Ontogenetic Changes In Word Meaning" when I was a graduate student.

Diver, a linguist at Columbia University, has provided an analysis of English from a semantic point of view. An example consisting of one small part of his analysis will be presented. First, some general statements about Diver's approach will be provided so as to indicate the interest value of his work to PCT fans.

Theoretical Approach to Meaning:

Knowledge of only the lexical(word) items in an utterance is insufficient to specify the message(train of thought) intended by the speaker. Consider the sentence "The boy had gone home." The lexical items are: the entity "boy," the occurrence "go" and the entity "home." If only the lexical items were spoken, missing from the message would be the meanings contributed by the grammatical items "the," "had," "-ne," etc..

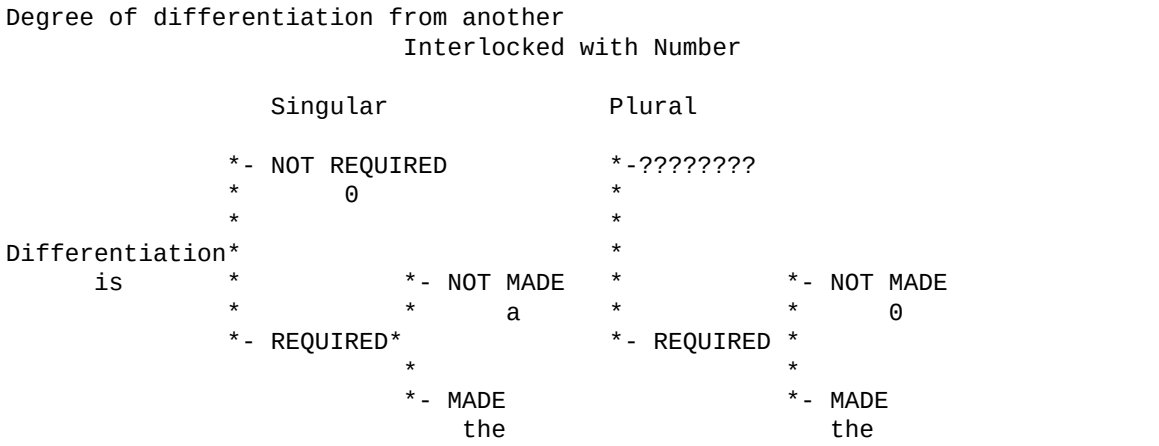
Traditionally these grammatical items have been regarded as lacking meaning. It is the discovery of Diver that this traditional assumption, among many others Diver challenges, is false. Diver takes the point of view that a speaker communicates grammatical and lexical meanings to the listener who constructs the intended message from these meanings. Diver calls the process of going from the meanings to the message "inference." This term stands for all the nonlinguistic factors which enter into the communication process. This is where physical, social, emotional-motivational, cognitive, etc.., contexts exert their influence.

From this brief description of Diver's point of view, it should be clear that: (a) The message of an utterance and the meaning of its grammatical and lexical items are at two different levels. The latter items are by themselves insufficient without the inferential activities of the subject, (b) The meaning of a

grammatical or lexical item is the contribution is makes to the message communicated.

Example of the kind of linguistic analysis done by Diver:

Let us consider the articles--"a" and "the." Diver's analysis is as follows:



These grammatical items are part of a grammatical system--"Degree of differentiation from another"--which is concerned with the communication of the degree to which the speaker has differentiated an entitty for the listener from other entities. This system is interdependent("interlocked") with a second grammatical system--"Number"--which is concerned with communicating information about the number of entities.

When the grammatical item "the" occurs before a singular entity, Diver says that two meanings are signalled--the fact that the entity requires differentiation from others and the fact that differentiation has previously been made by the speaker.

When the grammar is a singular entity(represented by 0), Diver says that the meaning signaled is--differentiation is not required.

The question marks suggest that when dealing with plural entities, it is always necessary to provide differentiation.

Some sentence examples:

He put a potato on the plate.

Comment: 0 before He
a before potato
the before plate

He put peas on the plates.

Comment: 0 before He

0 before peas
the before plates

Some PCT related observations:

Within Diver's theory of language, language consists of the meanings signalled by the grammatical items and seems to be limited to below sequence levels. It is possible to have a theory of language which does not involve the higher levels.

The meanings of lexical and grammitcal items are the set of nonverbal perceptions which are evoked when a person hears an utterance. Other perceptions may contribute some meanings. The message is constructed from all the nonverbal perceptions which are evoked. The message seems to involve sequence and higher levels.

I made this strange utterance when watching the movie the Terminator. "He was remembering what she had done before she did it." The context is that of a man who is a time traveler from the future. This sentence violates the normal use of grammatical signals connected with tense. Out of the context of the movie, it would be marked as being strange. However, the message makes perfect sense in the context of the movie.

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Date: Sat, 10 Aug 1991 15:06:49 -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: PCT and Diver--resend

From: David Goldstein
To: Bruce Nevin, Bill Powers, others
Subject: Diver and PCT
Date: 08/11/91

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Diver, a linguist at Columbia University, has provided an anlaysis of English from a semantic point of view. An example consisting of one small part of his anlysis will be presented. First, some general statements about Diver's approach will be provided so as to indicate the interest value of his work to PCT fans.

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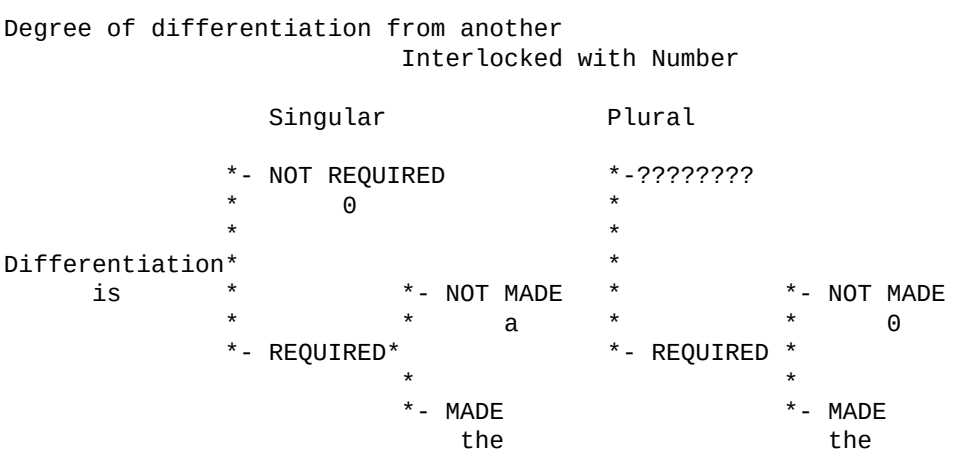
grammatical items "the," "had," "-ne," etc..

Traditionaly these grammatical items have been regarded as lacking meaning. It is the discovery of Diver that this traditional assumption, among many others Diver challenges, is false. Diver takes the point of view that a speaker communicates grammatical and lexical meanings to the listener who constructs the intended message from these meanings. Diver calls the process of going from the meanings to the message "inference." This term stands for all the nonlinguistic factors which enter into the communication process. This is where physical, social, emotional-motivational, cognitive, etc.., contexts exert their influence.

From this brief description of Diver's point of view, it should be clear that: (a) The message of an utterance and the meaning of its grammatical and lexical items are at two different levels. The latter items are by themselves insufficient without the inferential activities of the subject, (b) The meaning of a grammatical or lexical item is the contribution is makes to the message communicated.

Example of the kind of linguistic analysis done by Diver:

Let us consider the articles--"a" and "the." Diver's analysis is as follows:



These grammatical items are part of a grammatical system--"Degree of differentiation from another"--which is concerned with the communication of the degree to which the speaker has differentiated an entitty for the listener from other entities. This system is interdependent("interlocked") with a second grammatical system--"Number"--which is concerned with communicating information about the number of entities.

When the grammatical item "the" occurs before a singular entity, Diver says that two meanings are signalled--the fact that the entity requires differentiation from others and the fact that differentiation has previously been made by the speaker.

When the grammatical item "a" occurs before a singular entity, Diver says that two meanings are signalled--differentiation is required but has not been made.

When meither "the" or "a" appear before a singular entity(represented by 0), Diver sqys that the meaning signallled is--differentiation is not required.

The question marks suggest that when dealing with plural entities, it is always necessary to provide differentiation.

Some sentence examples:

He put a potato on the plate.

Comment: 0 before He
a before potato
the before plate

He put peas on the plates.

Comment: 0 before He
0 before peas
the before plates

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The meanings of lexical and grammitcal items are the set of nonverbal perceptions which are evoked when a person hears an utterance. Other perceptions may contribute some meanings. The message is constructed from all the nonverbal perceptions which are evoked. The message seems to involve sequence and higher levels.

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Date: Sat, 10 Aug 1991 17:33:01 -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; social control

[From Bill Powers (910810)]

Bruce Nevin (910808) --

We're running almost parallel now. The "rarity" problem that's left is mostly a muddle over causality. I agree that memory has to be involved in one's perception of frequency of any experience, in language or elsewhere. The mechanism you suggest -- how much of associative memory wakes up in response to a given perception -- makes sense to me (I can't offer a better one). The one question I have left, which is one of those I started with, is so what?

A person can recognize that a given linguistic form is rarely experienced. How does this become a linguistic rule that makes it rare? I guess what I'm looking for is some other explanation than mere frequency of occurrance. One that I offered, which would take care of at least one class of utterances, is that some sentences are rare because hardly anyone would have occasion to use them, even if the sentences would be correctly understood. I got a little hung up on trying to get an ack on that idea.

Of course the real problem is not that class of utterances, but the remainder. Why is it that "he ran in place" is OK but " he ran place" is not? The kind of answer I want is a description of how we make sense of a sentence, so that the second version creates an error. What is the error? The kind of error I'm looking for won't be found in lists of words. It's isn't just a violation of our expectations concerning what classes of words are to be assembled in what way (although when my question is answered, that sort of violation may still be part of the remaining remainder). It's a violation of the process by which we bring words and other experiences together into a form such that they properly mean each other. I'm trying to understand meaning, as a transitive verb.

Here is a lecture that comes to mind. It may be old stuff to you but it's a song worth hearing again once in a while.

There are two ways to understand natural phenomena. One is like trying to figure out a system for winning at roulette. You observe and observe, and finally you get an idea: every time two blacks and a red show up in that sequence, an odd number between 11 and 27 will win, but if the sequence is black, odd, black, red, the best bet is a number ending in 5. This is "looking for rules." It is also the basis for statistics, because when you're testing a rule like that you have to keep track of how often it worked. If it doesn't work often enough to be useful (i.e., to keep you from going broke), you go back to searching for more rules.

The problem, of course, is that even if a rule appears to work, you have to consider how many chances you had to find it, how many times it might have failed before you noticed it, and how often it will fail in the future. Even if the rule appears to work in all your tests, it may still have nothing to do with anything. Even if the rule works 20 times in a row, there is always the chance that it is irrelevant or will become irrelevant without advance warning.

In fact, all you need is one exception to show that the rule is irrelevant. If you can have one exception, then you can have two in a row, ten in a row, a hundred in a row, and go broke.

Of course there's always the chance that the rule you found actually has

some explanation; it might be a reflection of a real regularity in nature, so that the rule really has to work (even though you don't happen to know why) or sometimes has to fail (depending on occasional underlying circumstances you haven't discovered). This, of course, is what we hope for when we try to guess at the rules. This is the mode of research that I call "trying to get lucky." Getting lucky means stumbling across one rule among all the others that is an expression of an underlying mechanism.

If you get into the gambling hall after hours, you can look under the roulette table. When you see a little button where the croupier stands, you can immediately deduce a rule for betting that has some reason for working: bet (small) against the biggest betters. The game is rigged.

So this leads to the other way of understanding nature: look for the way in which the game is rigged. Don't waste too much time trying to guess at the rules just by watching phenomena. The only rules that actually work are those that work for an underlying reason. All the rest are illusions. If you just look for rules, you can't tell the illusory rules from the real ones. And the real rules don't work just because they work: they work because they have to work. The game is rigged that way. The system is organized that way.

Modeling is an attempt to see under the roulette table.

I said that control systems (other than the class of devices called servomechanisms -- artificial control systems) do not reside outside people. You say "they do reside outside people because that is where they put them." OK, pal, I know what's in my hand so I call your bluff. Give me an example of putting a (social) control system outside of people, and tell me where all the functions and signals are. Or let's make it easier (and double the bet): just describe the comparator to me. (I cheat: this means you have to describe the perceptual signal, the reference signal, and the error signal, too).

Eric Crump (910808) --

>Admittedly, such people as clerks and judges can be agreeable or not,
>and can have an effect on how the system operates, but if they play too
>fast and loose with the rules, and get caught, the system has mechanisms
>for "correcting" their behavior. Other people, playing by the rules,
>enforce them.

Just keep going and don't overlook the critical details. Exactly what are the mechanisms that "the system" has for correcting behavior? Here's a judge who says to the defendant, "You have a nice face. I can see plainly that you are Not Guilty. Case dismissed." What happens next?

>Is that intentional oversimplification to make a point? or do you mean
>to suggest that the system in no way limits behavior, that the license
>clerk will do whatever the license clerk wants to, no matter what?

It's not an oversimplification or even a simplification. It's a description. The system is only and exactly whatever the license clerk (and the rest) wants to do, no matter what. If you want to understand the system, you have to understand what the license clerk (etc.) wants to do, and why. There is no other place to look for it and nothing else to understand about it. It is not in the sky or in the air or in a hubcap or in the water supply (which you can drink or dump garbage in, as you choose). The system is people doing or trying to do whatever they want to do or want to try to do. This is how people work.

Best

Bill P.

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Date: Sun, 11 Aug 1991 10:28:31 -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: Language (Is that all there is?)

[From Bill Powers (910811)]

General interest headline from the Northwest Territories, relayed by Sam Randlett:

TUNA BITING OFF WASHINGTON COAST

Ed Ford (910809) --

>It would be helpful if you'd give us a hint as to what the weather is
>like in Durango.

Here's a combination of hearsay and what seems to be going on now:

Mornings start cool (60-70F). By noon, if the weather is clear, the temperature can be up to 80, and by 2:00P, 90+. In August, however, clouds generally build up in the afternoon and create some showers, cooling things off nicely. Otherwise back down to 80 by 6:00 and to 70 at sundown. In the evenings you may want a sweater if 65 seems cold to you. No reason it couldn't get colder (or hotter) -- this is the edge of the desert and the altitude is 6500 ft down at the post office.

One hint: there is relatively little air conditioning in Durango.

Bruce Nevin (910809)

>(It also may be seen that I do what I can to conform to SLOB
>expectations for English, lest what I have to say not be well received.)

Me too. I think that if you spend a lot of time trying to convey ideas clearly to others, you control for understanding, which means disambiguating sentences like the headline above, articulating so as not to confuse words that would make sense but are different, getting your pronoun references and verb numbers right, etc.. I also think that one

can be driven to extremes of elegance and elocution by competition and (during childhood) by adults who pretend not to understand perfectly clear but ungrammatical sentences. You know -- "Who do I give this message to about the baby in the burning car?" --- "Just a moment; do you mean 'To whom do I give this message ...'?"

I think that standard English is whatever other English speakers seem to grasp the first time, as you meant it. I can understand Vermont and Texas and Georgia, but not Sloppy and not Foreign, meaning truncated or masked by unfamiliar intonations, emphases, and phonemes. Heavy Chinese accents are really tough for me to make sense of. When it takes a lot of effort to turn spoken (or written) language into meaning, the chances of taking the wrong meaning escalate -- you have to put too much interpretation into it. Hearing standard English is effortless, which is how it feels when control is easy and skillful on both ends of a communication. I'm sure it's the same way for monolingual speakers of any language that is spoken with the aim of being understood clearly.

What I'm getting at isn't some version of chauvinism, but a possible way to understand where this idea of the "ideal" language comes from. It's not JUST a matter of consensus. It's a matter of making distinctions and avoiding unintended meanings. There are circumstances where you have to be able to say "She went with him in her car" so the listener knows whose car is going to arrive with whom where: she went taking him in her own car. In most human affairs, language is just a means to nonlingistic ends: What are you doing tonight, honey? Hand me that open-end three-eighths wrench. Where were you going to go last night when I guess I made you change your mind? Attention: Colorado pink Cadillac FZZ222, your lights are on and you are blocking the driveway. Language is about life, not vice versa.

>I believe with CT and operator grammar we can have it both ways--we can
>model linguistic competence and linguistic performance and (more
>importantly) the connection between them, showing how the former arises
>out of the control systems and reference levels by which we manifest the
>latter ...
>We can describe language not as a platonic ideal which speech
>imperfectly reflects, but as the intentions which speakers carry out
>with a degree of adequacy which is determined, as in all their control
>processes, by trial and error....

Now you're talking. It seems to me that your CT approach is taking shape and is going to end up making irresistably good sense. I think that language is going to turn out to work like any invented system. You start out with a simple design: let's say subject, verb, object. This works for a while, but then you find that you want the object to be a subject relating to something else: he has her dog. This puts words next to each other in a way that suggests wrong things -- subject verb object doesn't work right. So you have to add the ability to chunk parts of sentences within sentences to keep their meanings separate until the end when the chunks are put together. The design turns out to have properties you didn't anticipate -- this happens to modelers all the time. It would be interesting to approach English with the idea that somewhere in there is a very simple Jane love me core, with elaborations upon elaborations that arise because of things you can't say that way, meanings getting crossed

up, sentences you can't end, and so on. I wish she had told me where we were going before her mother got there and told her where she thought I meant --- start over.

Language is so important in our lives that we need to understand how it fits into the whole picture of perceiving and acting in order to have a workable theory of human nature. I'm still thinking that when we get to the real processes out of which language grows, they will turn out to be the same perceptual and control processes we use for everything. The Area of Broca is certainly essential for language. What else is it essential for? I think they forgot to ask that question.

>It is difficult for me to perceive intensities except by by comparing >them in relationship or in sequence, and one can speak of them only by >way of categories for them (to which words attach).

The lower levels do not have any language. It is possible to perceive intensity without talking about it, comparing it, objectifying it, and so on. What it takes is silent looking, smelling, feeling, and so on. This is a very subjective procedure but worth doing.

>It also appears possible to make any of 6-8 meta to any of the others -- >relationships between sequences, relationships between categories, and >categories of sequences, as well as the reverse.

This is a place where language truly gets in the way. Another factor that confuses the picture is the way we can bop up and down levels of awareness, one moment seeing everything as categories and the next thinking about principles. As we do this there is a constant stream of words babbling along describing what's going on. The words are being substituted for meanings and treated as if they were the meanings. We name a set of categories, creating word-objects in perception, and then we can see that one string of words is longer than the other, so we think we are looking at a relationship between categories -- the categories themselves, not the word-string-objects we just used to refer to them. We see that one member of one category will fit inside another member of another category, so we speak as if one category fits inside another category, which is not what we saw at all.

I tried to say something about this in the 1960 paper with Clark and MacFarland. I called it "order reduction." When you speak about something, you are creating symbols that are treated at the program level as elements of some rational program process. But the symbols are also lower-order perceptions and can be observed as objects, transitions, and events in relationship, and be re-categorized to create new (abstract) symbols, and so on. The result can be utter confusion if the symbols are not distinguished from the experiences for which they stand. You said something like this in commenting on the way descriptions get confused with the phenomena being described.

I don't know the true answers to the problems you raise. But I do know that you have to get rid of the words as much as possible and try to get in touch with the bottom layer of verbal abstraction: the signal that you

name by the lowest level of word. You have to experience a relationship and strip off all the descriptive terms. Wiggle your fingers without naming the movements; look at how they relate to each other without naming the relationships (they're mostly unnameable anyway). See them as "fingers" without attaching the label -- just get that sense of the way in which all those fingers are examples of the same thing; when you've seen one finger you've seen them all.

I have a feeling that I will never get across in words the basic experiences from which I derived the levels. The only way to get corrorobation, therefore, is for others to do the same kind of investigation, trying to see past the words to the silent perceptions they indicate. Think of the category "dogs," and then discard the word and look at the experience it means. THAT is a category. I don't think that there are relationships among categories -- only among the symbols used as their names, and then only among the lower-order perceptions that are being used at higher levels as symbols. The same is true of sequences, programs, principles, and system concepts. The whole perceptual hierarchy gets involved in trying to talk about these things. What is happening is that, for example, the principle level is ordering the lower-level perceptions in such a way that another person who also can perceive at this level can grasp the exemplified principle. The principle itself can't be spoken. A sequence can be exemplified, but the sense of sequenceness is a perceptual response to the example: just a signal, not a sequence.

I certainly don't blame anyone who gives up on all this and declares it to be mystical bullshit. What I'm trying to talk about is right at the limits of what I can grasp. Sometimes it's clear and sometimes it isn't. Nothing very practical or communicable is going to come out of this -- not from my head, anyway. In practical terms we'll get much farther starting from the other end: experimental testing of models that we know how to make. But the levels of perception and control exist and we will have to deal with them eventually.

>I note that there is a program-like property of sequences as implemented
>in BCP: the n+1th recognizer of the sequence is activated only IF the
>nth was activated, as well as IF the lower-level perceptual input is
>forthcoming.

Uh-Uh. If you adopt the program point of view, you can make a program out of anything: IF the raindrop has not hit the ground, keep falling; ELSE go splat. This doesn't mean that the mechanism behind every process is a program. The sequence-recognizer (in BCP) is simply hooked up physically so that each stage is activated by the prior stage. No little looping tests, no choices, no switches. As you then proceed to recognize.

>Principles avowedly have to do with categories or types of programs.
>This supports my contention that we can apply category-level perception,
>and hence the words of language, everywhere.

Sorry to keep harping on this. From a set of categories (1,2,3) you can derive a sequence: "next n = n + 1." A program can be derived from a set of occurances of this sequence "If not condition, do again; else do something else." From the operation of the program, a principle can be

derived: "etc." Or perhaps something less nameable, such as a variable that has at least the two states "terminating" and "nonterminating."

What would a category of principles be? I think you could come up with a category of names of principles, but can you truly perceive categories in a set of principles? I think what you get are system concepts.

>The existing of taxonomies and associated hierarchies of classifier
>words in many languages, particularly in technical sublanguages, is the
>source of a lot of messiness that doesn't map well onto the perceptual
>hierarchy. The problem is that we get categories of categories of
>categories, to indeterminate depth.

I agree that we need to improve the definition of the category level. Maybe the order-reduction idea will help. Note that at the program level, it is also possible to construct hierarchically-organized programs. Here I think that subroutines and sub-subroutines have real significance. Could it be that we are just messing around inside this one level with all this reasoning that's going on? I am very glad that you are worrying about all this, too. You (and other linguists) probably have a better chance than I of putting a little more order into this mess.

David Goldstein (910810) --

You make an excellent point: the criteria for accepting any one linguistic theory for merging with CT obviously can't come from any of the theories. What are the criteria? Or do we try to treat this as a buffet dinner?

Your post on Diver's theory of number is interesting: the diagram does have the form of a program. And I guess it works, although it doesn't seem to make allowance for the option "he put THE peas on the plate" (and the carrots somewhere else). Are these rules supposed to work ALL the time? Or if differentiation is not required, is it still OK to do? Is the meaning then the same whether or not differentiation is used? (I don't think "he put peas on the plate" means the same as "he put the peas on the plate." The first implies that there are still some left for the others; the second seems to mean that those were all of the peas.)

Best

Bill P.

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Date: Mon, 12 Aug 1991 05:06:35 -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: Level recursions

[From Bill Powers (910812.0300)]

I awoke in the middle of the night with what seems the answer to the recursion-of-levels problem. And some fallout ideas.

We can speak of a configuration of sensations. We can also speak of

configurations of configurations ... of sensations. But that is not the question.

The question is whether we in fact perceive and control such things.

You can speak of a pail of water. This puts the configuration-perception called pail into relationship with an unformed sensation-perception (to paraphrase Benjamin Whorf) called water. The latter is "in" the former, or "contained by" it. You can, in fact, create a pail of water through suitable actions used to control relationships.

You can also speak of pails of pails ... of water. But the latter is just a repetition of a pattern of sounds or marks and no longer refers to anything perceivable or controllable. It is created by manipulating words as objects in themselves without regard to their nonverbal meanings. We can do this in imagination as easily as we can make up gorphic words that seem to have grammatical properties and a teasing hint of meaning. We can imagine much more than we can actually perceive and we can say much more than we can actually imagine. That is both a blessing and a curse. We can manipulate words that sound as if they ought to have meanings -- e.g., "intelligence" -- just as if they actually did, using them in arguments and as the basis for conclusions, and never pause to see whether they mean anything. That's the "curse."

We are better off to winnow out such terms or expressions, and make sure that our sentences of practical importance actually have some counterparts in experience besides other words -- or at all.

Configurations of configurations:

In the apparently real world, sets of sensations coalesce to form objects. These objects can often be manipulated by acting on them. They are also affected by other people and by natural forces.

What we specifically do not experience are arbitrary coalescences of objects, once they are differentiated, into unitary new objects that we can perceive and control at the same time as the elemental objects -- not at the same time that the original objects remain independently controllable. If the objects do coalesce -- if the ice cubes in the bowl melt together and refreeze -- we discover that we have one object again, a fused mass. The individual objects have disappeared from reality precisely because they can no longer be independently manipulated. When we imagine manipulating the stuck ice-cubes they become objects again -- until we try to do this to the "real" ice-cubes. Then we realize that the individual configurations are only being imagined.

We can thus speak of the arm of a chair, but we do this in imagination only. By splitting the chair in imagination into components, we can create static relationships like "of" -- the arm OF the chair. If the arm actually proves to be arbitrarily manipulable independently of the rest of the chair, the chair is now a broken chair: it has come "apart." The arm, in fact, is no longer "of" the chair. There are now two independent configurations, not one. Even if the arm swings up and down on a hinge, we have two configurations instead of one, constrained by a relationship between them (the arc along which the arm must remain).

I believe that this has something to do with the way the configuration level detects configurations in the first place. In a static field of sensations, configurations can only be imagined; boundaries separating objects can be imagined anywhere in a static field made of more than one discriminable sensation (past experience aside). An object is detected when some subset of the sensation field begins a concerted or systematically related movement. The red and orange patches begin to rotate about each other and move to the right, while the yellow and green patches stay where they are and suddenly become the background configuration. A rolling ball, and linoleum. Now we can see higher order perceptions: ball rolling across linoleum.

Here's the fallout:
This thought now finally convinces me that I have the configuration and transition levels in reversed order. I have worried about this possibility for several years, because of the fact that in physics the lowest level of relationship between force and effect is acceleration, the next level is the integral of acceleration (velocity), and the next level is the integral of velocity (position). The static concept of position, which is related to such configuration-like variables as size, orientation, and shape, is derived by integrating motion. When I build models, for example the arm model, the lowest level controls acceleration, the next level velocity (which provides damping) and the next level position. It doesn't work in the reverse sequence.

In a model of arm control, the acceleration level is treated as the lowest level. In fact it is the sensation level, for it is a function (in the real system) of a large number of signals from force-detectors, signals that are combined to yield a net acceleration signal. The signal from a single force-detector is an intensity signal. The weighted combination of signals from many force detectors is the sensation of force that the aggregate system involved with a muscle actually uses as a measure of acceleration.

With this ordering straightened out, it would seem that the correct hierarchy should begin

1. Intensity
2. Sensation
3. Transition
4. Configuration

It is interesting that the operation making (2) out of (1) is (as before) weighted summation; (3) is related to (2) by time-differentiation, and (4) is related to (3) by time-integration. Calling the last operation time-integration is metaphorical only; the process must be related to time integrals but this is only part of object-identification.

The integral of a derivative leaves a constant of integration unspecified. This allows for resetting the "zero" of a configuration to any arbitrary state. It also allows independence of absolute position or orientation, and maybe size. I would expect that configuration signals derived by integration in this way would be tested somehow against a signal that stands directly for configuration (where possible); for

example,a joint-angle detector's signal. This sounds like the start of working out a model-based control system with provision for updating the model. But that gets off the track for now.

In the arm model, acceleration-perception is derived from tendon force-detectors: force (as torque) divided by moment of inertia, a constant, = angular acceleration. Velocity perception could be derived by integrating the tendon signals, but damping is actually obtained from sensing rate of change of muscle length directly (the phasic component of the stretch reflex sensors). This damping, however, takes place in the gamma-system, which is classed as part of the nervous system involved in unconscious reflexes. It may be that consciously sensed angular velocity is derived by integrating the sensation of effort that comes from tendon receptors. Something suggestive here which I will let go for now. This isn't quite right yet.

So anyway, we have, I think, straightened out the problem of configurations of configurations. One reason I think this is the right answer is that it seems to have sprung me loose to finally make that switch in levels that's been nagging at me for so long. It suddenly seemed plain that configurations are derived from integrated coherent changes in the sensation field -- which requires the transition level to intervene. There is no point in proposing TWO transition levels. Martin Taylor, didn't you say something about this on the net?

Now to return to the general "recursion" problem.

I think we can identify this problem as one of verbal manufacture. It comes from our ability to try out arbitrary manipulations of symbols according to rules but without regard to perceptual or "factual" constraints. We do precisely the same things with mathematics, which is, I think, a different use of the same level of function that reasons by verbal rules. Mathematics not only has a mysterious power to represent laws of nature, it has vastly more power to represent laws contrary to or irrelevant to nature. We could easily propose that the force of gravitational attraction goes inversely as the cube of distance. With that premise, we could work out a whole universe of celestial mechanics just by applying Newton's laws -- that is, the other laws. The only problem is that the observed flight of objects over any appreciable distance would depart noticeably from what the mathematics predicts; the predicted orbits of planets would not be those we observe (in fact there would be no planets or satellites in the mathematical universe). But the mathematics doesn't know that it is producing nonsense.

We select among possible worlds, all mathematically valid, by comparing the mathematical predictions with what is actually experienced. Experience would lead to immediate rejection of the mathematical fantasy in which gravitational attraction decreases as the third power of distance.

So mathematics doesn't have any mysterious power to describe nature after all. It's just a tool that can receive as input any systematic set of premises and derive their consequences according to the rules we've adopted. We have to go outside the boundaries of mathematics in order to decide which mathematical manipulations to use and which conclusions to

believe as being relevant to nature. We don't reject them by rejecting the systematic manipulations of mathematics itself; we don't pluck out our eyes if what they see offends us (Jesus was probably being sarcastic). We change the premises or use different (but internally valid) manipulations until the mathematical conclusions coincide with direct experience. Experience, not mathematics, is boss.

That, by the way, leaves me with a sense wavering toward certainty that particle physics has become a vast and expensive exercise in exploring a mostly imaginary universe. Anyway ...

So it is with words. We can speak sentences of all kinds, and use verbal rules to derive conclusion-sentences from them. A very few of all possible sentences and conclusions describe what we actually experience. We can't distinguish meaningful sentences and conclusions from meaningless ones just by making more sentences and conclusions. We have to look at experience to see whether the proposed meaning is imaginary, nonexistent, or part of what we call objective or directly-experienced reality. We have to experiment; we have to attempt real control of the perceptions that the words suggest. Then we find out if the words have any significant non-imaginary meanings.

A relationship between relationships is clearly an empty combination of words. By straining imagination one might think of an example, but in nature we simply have relationships, many different ones occurring at the same time. We never observe that "in" is "above" "on." That's why we don't use sentences like "in is above on," even though nothing keeps us from doing so. The only way we can make sense of such a sentence is to say "in" is above "on", where now the sentence can only mean the word-configurations written in spatial relationship: the word "in" written above the word "on." That is a relationship between configurations, which we do observe. The meaning of a relationship among relationships can only come from synecdoche.

And what about sequences of sequences? Here the solution is similar to that used at the configuration level. If we have two sequences of three elements each: 123 and ABC, for example, a sequence of sequences could be either 123,ABC or ABC,123. If 123,ABC turns out to be a naturally-occurring unit, then we have in fact just one sequence: 123ABC. We can imagine that it is composed of two sequences, ABC and 123, but if experience shows that the actual occurance is always 123ABC, then only that sequence is real; the others are imaginary. We might find it possible to control the world so that ABC occurs by itself, but we find that if we start by creating 123, the rest of the sequence, ABC, inevitably follows. In that case 123ABC is one sequence, and ABC is a different one.If it should turn out that 123 and ABC can be produced independently of each other, then there are two sequences and 123ABC is either not real or an independent sequence, even when the independent sequences happen to occur one after the other. We do not learn to perceive sequences that are accidental; we would be in big trouble if we took apparent but illusory concatenations seriously. Once we know that the arm is loose from the chair, we cease to treat the chair as if the arm is part of it. We will pick the chair up by the other arm, but not by the loose one. Once we know that buying a lottery ticket is not always followed by winning a prize, we cease to think of the sequence as a

natural one even though that doesn't keep us from imagining it.

Note how easily "123" and "ABC" come to be used as if they ARE the occurance of the actual sequences. Naturally, these two written letter-globs can be sequenced any way we like: they are configurations. If you don't keep straight the level of perception at which you're working, and if you mix up symbols with the perceptions they are suppose to indicate, all of this can become extremely confusing.

This whole discussion, I think, shows why it is so important to become conscious of the nonverbal meaning (if any) behind sentences or thoughts expressed in words. In some modes of discourse such as philosophy or high-flying cognitive sciences, sentences occur that sound just as though they ought to be laden with deep meaning; it takes some courage to look firmly at this sort of language (particularly if you produced it yourself) and ask to what the terms refer. The more important-sounding the term -- especially if it is italicized or underlined or in quotes -- the smaller, I think, is the chance that it actually refers to something perceivable OR imagineable. I think I realized this at about the age of 25. It was an awful experience for one who considered himself adept at philosophical argumentation.

I think this same argument applies without any problem at any level where the X of X ... difficulty seems to appear. Language proposes; nature disposes. Look at the perceptions, not the words.

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Date: Mon, 12 Aug 1991 11:53:29 -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: Updating CSG-L

[from Gary Cziko 910812]

I am back from my vacation in the north woods of Wisconsin (where we were neighbors for a week to a bald eagle family of two adults and two "chicks"). I am home on the on the network today and tomorrow until I take off for the Durango meeting.

I have been updating the CSGnet list so that all names are in the form of SMITH John instead of John SMITH or Smith, John. So a number of the people will be notified by the LISTSERVer that their entry has been updated. This only means that I have been editing the list and is no cause for concern (unless I've spelled your name wrong or taken your last name to be first and vice versa).

Thanks to all of you who had kind comments or otherwise concerning my Chapter 5. I hope to bring up some issues related to this chapter when I get back from the CSG meeting.

--Gary

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Date: Mon, 12 Aug 1991 20:45:28 -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: Stabilized Images

[from Gary Cziko 910812.2040]

re: Bill Powers (910812.0300)]

>This thought now finally convinces me that I have the configuration and
>transition levels in reversed order. . . .

>With this ordering straightened out, it would seem that the correct
>hierarchy should begin
>
>1. Intensity
>2. Sensation
>3. Transition
>4. Configuration

Bill, isn't the research on the stabilized retinal images relevant to this as well? This is the research where a little apparatus is attached to the cornea with a pattern which always falls on the same part of the retina since it moves with the eyeball. Subjects report that the configuration (a square or circle or whatever) soon starts to fade away and reappear in parts. Continual eye movements (self-produced transitions) therefore appear necessary for the perception of a visual configuration.

Valentin Turchin spends considerable time discussing this in _The Phenomenon of Science_ which Cliff Joslyn said you should soon receive.--Gary
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Date: Tue, 13 Aug 1991 10:55:20 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Comments: Converted from PROFS to RFC822 format by PUMP V2.2X
From: "Bill CUNNINGHAM - ATCD-GI (804"
              <CUNNINGB%MON1@LEAV-EMH.ARMY.MIL>
Subject: Transition/Configuration Inversion

:from Bill Cunningham:

Ref Bill Powers (910812.0300)
    Gary Cziko (910812.2040)

I feel much more comfortable with the revised order. The previous seemed contrived, but I couldn't say why. I peddle HCT where I work with varying success. At least three colleagues have challenged the previous order with independent, but identical, war stories: When moving through jungle under very hostile conditions, all sensory organs are wide open. Survival depends on detecting an adversary before being detected. These survivers all report correctly perceiving the presence of an enemy before anything resembling configuration has emerged. They can't pin it down to any particular sensory input, but they report a clear perception of a transition (absence of enemy-->presence of enemy). This experience was vivid enouth for them to remember it clearly 20 years later and to find immediate fault with HCT's previous order.

Camouflage (natural and manmade) works to defeat perception of presence first, and then to defeat perception of configuration. So does the protective behavior of wildlife. The most spectacular example I have seen (several times)
has been a heron caught fishing several yards from marshgrass cover. If you stop and don't actively threaten the bird, it will fade from view in a few minutes. You won't perceive its motion, but it will fade like a Cheshire cat--less grin. The original detection was possible because of the transition between water and bird, and perception of configuration follows.
The bird's retreat is so slow that motion is not perceived outright--only if you compare against a *detailed* reference image from a few minutes previous. As the bird nears the shoreline/marshgrass, the visual transition between the bird and background becomes less distinct and you have to remind yourself of the configuration.

I think the revised order is also more consistant with doppler detection, which
is one of the other local challenges.

Gary Cziko's reference to the vision experiments came immediately to mind, but I couldn't supply the reference. I'm glad he did.

Bill Cunningham
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Date: Tue, 13 Aug 1991 13:39:04 -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: Stabilized Images

[from Gary Cziko 910813.1330]

re: Bill Cunningham 910813

Interesting war stories on detecting the enemy and camouflage. Turchin

talks about the frog as well. Transition involving a small configuration means "eat" to the frog, while transition involving a large configuration means "flee."

>Gary Cziko's reference to the vision experiments came immediately to mind, but
>I couldn't supply the reference. I'm glad he did.

I didn't quite. I believe Turchin refers to a 1960 Scientific American article. I don't have the reference here in my office now but I can post it when I get back from Durango, although I'd be surprised if Bill Powers were not familiar with this research.--Gary

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Date: Tue, 13 Aug 1991 16:08:20 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: reply to David

[From: Bruce Nevin (910809 1458)]

David Goldstein (08/09/91)

You ask about choosing among alternative theories of language. I think among the criteria are:

* Coverage: is there a grammar of any language, written in accordance with the theory under consideration, that covers at least as much as, for example, Jesperson's _A Grammar of English on Historical Principles_? Harris's 1982 book claims to cover this much synchronic detail and more.

* Simplicity: how much of new mechanisms, devices, components, modules, principles, rules, conventions, formalisms, and the like must be made to fit into the existing HCT framework, and what is the impact of the mutual adaptation on either side? So far as I can see, Harris's theory calls for nothing that is not already there in HCT.

* Universality: does the theory apply as-is to the range of language variety in the world, without ad-hoc modification for particular cases? Harris's theory has been applied to a variety of languages, though not with the coverage of GEMP for English, including Modern Greek, Korean, and Takelma (a language of Oregon). Though language universals have not been Harris's focus of research, the proposed universals of language that I have looked at (a) appear to fall out

from properties of the base sublanguage or (b) if they are universals in reduction types or patterns they are quasi-universals with many exceptions, and I suspect they fall out from properties of perceptual control that in mainstream linguistic circles go in big fuzzy buckets with names like Pragmatics.

Please tell me what you think of these criteria, what others you think should apply, and how you see Diver's theory measuring up on all identified criteria.

I think advocates of various theories of language should put their views forward, support them according to agreed-upon criteria, propose more criteria or alterations to the current list, etc.

I will not do this work for other theories, since over the past 25 years I have concluded that Harris's is pretty solid and meets most of my standards of adequacy. (CT appears to promise what I have felt was wanting in Harris's theory.)

Other theories that might be considered include Government-Binding theory of Chomsky and others, especially the reformulation in terms of Joshi's Tree-Adjoining-Grammars (TAGs) by Joshi and Kroch which proposes to unify variants of GB theory; lexical-functional grammar (Peter Sells has written a good introduction to both GB and LFG in one volume); the socalled "cognitive linguistics" of Langacker and others; the revival of context-free grammars, head-driven PSGs, unification formalisms of various sorts, found mostly in the Computational Linguistics literature (Sag, Pullum, Gazdar, others); the Stratificational Grammar of Sidney Lamb is still alive and kicking, with a graph-theoretic/Computeresque formalism that should have great appeal to anyone familiar with CT. Let's see . . . tagmemics is as dead as Kenneth Pike, rest them both. Ah yes, there is the revival of Prague-School functionalism (Talmy Givon, others); various sorts of ethnolinguistics concerned with issues of language use with questions of language structure and meaning subordinated; Labov's type of sociolinguistics which might be called urban ethnolinguistics, don't let the emphasis on statistics put you off, their findings are absolutely brilliant and present fascinating challenges to staid dogmas of the field. . . . Well, I'm sure I'm leaving out other important contenders. The point is, proponents of other views will have to be got interested in CT so they can put their views forward and see how they and CT fare together. The competition (questionable metaphor) is not between Harris's theory and Chomsky's, or between Diver's and Langackers, but between one way of making CT accountable for language and another.

David Goldstein (08/11/91)

You observe that grammatical items like "the," "had," "-ne," in the example sentence "The boy had gone home" are traditionally regarded as without meaning, as distinct from lexical items like "boy," "go," "home" but Diver says this is false, the former have grammatical meaning and the latter lexical meaning.

Diver distinguishes the message intended by a speaker (and that understood by a hearer) is on a different level from the lexical

meanings of the lexical items and the grammatical meanings of the grammatical items.

Diver says that the meanings in the lexical and grammatical items of an utterance are insufficient to account for the message, and that the hearer must infer the message from the item meanings PLUS nonlinguistic contextual factors.

So far there are no substantive differences as to Diver's claims and Harris's. Harris accounts for the meanings of most grammatical items by their being reduced forms of lexical items in a more explicit, unreduced sentence. Thus for example:

* The -en of broken, forked, lost, gone, etc. (various allomorphs of the same suffix morpheme) are reductions of something like "in a state of" and appears in the passive, the past and perfect participles, etc.

  The window was broken by the stone.
  The window was in a state of the stone's breaking it.

  The boy had gone home.
  The boy had the state of his going home.

  The peculiarity of the unreduced sentences is a reflection of the strength of the convention to make the reductions. (By the way, this -en suffix is derived historically from a stative noun.)

  Restrictions on these constructions are now semantic and pragmatic, that is, governed by the otherwise known perceptual control hierarchy and not by something peculiar to grammar. Thus, you cannot say "George Washington has lived here" for the same reason that you cannot say a dead person "has" anything, can't have a state of being or doing something, any more than he can have a chair to sit in or a house to live in. Of course you can say this of the remains--"GW has been in this tomb for two centuries, but now they want to move him."

* The have, had of the present and past perfect constructions is then the same verb have found in e.g. "the boy had the stone." The restriction is on the stative noun which is reduced to the -en suffix on the following verb.

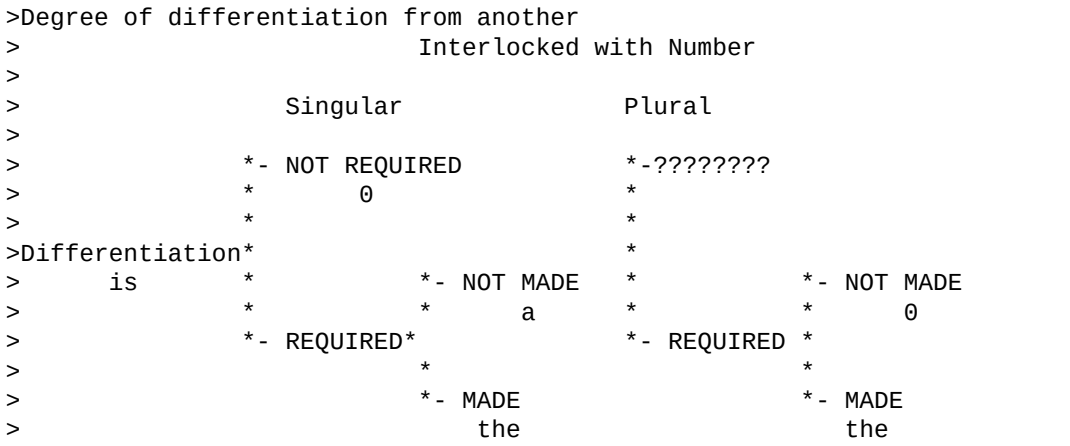
* Even the definite article is a reduction from the appositive construction "that which is". This accounts neatly for examples like

  The family doctor is fast becoming an endangered species
  That which is a family doctor . . .
  Yesterday, George Smith threw out the alarm clock that had awakened him for work every day for thirty years.
  . . . threw out that which is an alarm clock . . .

* The indefinite article is an argument indicator that is required when one of a class of primitive nouns called "count nouns" is entered under an operator in the construction of a sentence, but may later be zeroed. The suffix -ing is an argument indicator on operators when they are entered under another operator. Argument indicators are all

that remains of what are traditionally called grammatical morphemes, the others are all reductions of lexical items.

Diver's analysis requires a separate set of interlocking grammatical systems with their own vocabulary (required, not required, made, not made, singular, plural, etc.) and their own relationships expressed as decision trees I would guess on the program level. Here's your decision tree for reference:



This metalanguage is not contained in the language (one doesn't express decision trees very well in sentences). Nor is it contained in the existing perceptual control hierarchy, since it applies only to grammar. The interpretation of its vocabulary requires the same TBD integration of vocabulary with nonverbal perceptions, presumably the same as for the non-metalanguage vocabulary ("made" is the same in the decision tree as in a sentence "the boy made a mess" for which you are using the decision tree to compute the placement of definite and indefinite articles). All the issues about vocabulary-to-perception correspondences, iconicity, classifier vocabulary, confusion of description with thing described (when all you have is the words, everything looks like a category), and so on, remain unaddressed here. Exceptional cases, like that Bill cited and like the examples I cited above for -en and the, look to require modification and complication of Diver's decision tree. Differences between languages would require further complication and modification of something very basic to the semantics of the theory, whereas they are all in the reduction system for Harris, where all the arbitrariness of social convention lies. (The defininite article and the periphrastic passive are recent innovations in Indo-European, and differ greatly in their distribution from one language to another even within those related languages, God help you with Havasupai, Tzotsil, or Rwanda!)

>Which theory of language do we try to mesh with PCT? . . .
>How do we go about making the proper choice, assuming that we are
>not going to create our own theory of language.

I think that we are going to create our own theory of language, though it will be by adaptation and not de novo. The adaptation will I think be mutual. Developing a perceptual control theory of language and of language use will require some "bending" of any existing theory of

language that I know of, and will require some attention to some detail that has necessarily been pretty loosely specified in PCT as it is.

>Can you explain
>to us how you made your choice out of all the possible theories
>of language which exist? What were the considerations which lead
>you to chose Harris, in particular?

Have to question the presupposition there that I examined the range of all existing theories of language, compared them, and made a rational choice based on some superordinate criteria. I didn't. I couldn't have. Nor I believe can any of us or can we. But of course we or any one of us is welcome to try.

How I got into Harris's perspective was like anything else: trial and error of a control system, a combination of purpose and luck. I'll put a sort of answer to this at the end of this post, but I don't think it's really the question you intend here.

>The choice . . .
>defines what is language and therefore, what is not language.

I think we need to beat different theories of language together to find out what is language and what is not, and then beat the results (or the views of any particular theory, such as Harris's) against CT to find out how much of what appears to be there is really there, put out into social space by human beings as explicitly shared and mutually calibrated reference values. Other, nonlinguistic systems are also put out into

> What were the considerations which lead
>you to chose Harris, in particular?

OK, read on if interested, else skip. I'll try.

Harris's results answered (and answer) some very deep motivations in me about understanding how we come to know what we know and believe what we believe, what its relation is to experience, how we reach agreements and negotiate disagreements about it. Experiencing the world through another language/cognition/cultural system is important to me for addressing this still largely inchoate motivation. That's like parallax, the need for two eyes if one is to see with depth perception. The discoveries of structural linguistics and (Harrisian) transformational analysis and discourse analysis about the structure that is in a language made clear where and what these differences were. At the same time, these studies began to show something about the intersections, the commonalities, the universals of language that seem to have more to do with perception and cognition. The idiosyncrasies of different languages are disclosed as matters of social convention, arbitrary agreements.

Some of the universals may be agreements too, but at a deeper level, biological, rather than cultural. This is in the sense that the neural signals in a control system provide analogs to the whatever-they-ares of the world outside and between us humans. So CT and Harris's theory of

language both tell me about the unification of what the two eyes see (to continue the metaphor) into a single coherent image.

His methods engaged me because of their clarity and consistency. One abiding concern of his, especially explicit in _Methods in Structural Linguistics_, is verification. We may arrive at results by all sorts of means--hunches, heuristics, trial-and-error, analogy to a more familiar language or to a previous grammatical analysis of it, etc. However we come up with a proposed solution, we have to verify that our results bear a valid relation to the data. No one would follow some of the procedures in MSL as a discovery procedure for determining the grammar of a language, but in principle one could, and in the case of any particular feature of the results one can retroactively check and verify that such a relation obtains between it and examples of speech. Thus, for transformation, one can examine a pair of sentences of form A that are differentially acceptable and verify whether the corresponding sentences of form B have reversed positions on the same scale of acceptability. This criterion neatly partitioned paraphrase in the sense of conveying the same information from differences of nuance, emphasis, etc., at a time when most linguists in the field were greatly exercised about the fact that every transformational rule that anyone had come up with in Generative transformational grammar had exceptions that were not paraphrastic.

All I had was my intuitive nose for truth and my sense that Harris has a good grip on a big chunk of it. I tested and verified this in various ways. I should say that for the first couple of years of my work with Harris I was skeptical and set myself the task of finding what was wrong with his theory. What do you say to a guy who in effect says "Great! Let me know what you find out!"

There were these repeated confirmations that other folks were in various kinds of troubles and embroiled in raging polemics about things that were non-issues for Harris.

There was needless complexity and abstractness in more fashionable theories because of an unquestioning retention of phrase-structure grammar and its rewrite rules as a formalism.

There was the conceptual unsatisfactoriness of grammar as a filter on a completely unspecified performance mechanism.

And so on . . . I could go on about various things that convinced me and that confirmed my conviction. However, I don't know that my personal intellectual history is all that relevant to the task before us now. It can't be convincing to anyone else, and of what interest and value can academic politics and polemics ten, fifteen, twenty and more years passe' be to us today?

Bruce Nevin
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Date: Tue, 13 Aug 1991 16:09:09 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: reply to Bill Powers

[From: Bruce Nevin (910809 1607)]

Bill Powers (910810)

Re frequency, rareness, likelihood:
>so what?

You can say any of the following:

John plays piano and Mary plays violin.
John plays piano and Mary violin.

John plays piano and John plays violin.
John plays piano and violin.

There is a high likelihood of word repetition in parallel argument-positions of arguments under "and." Given this high likelihood, the second occurrence can be phonemically zero (though morphemically present, just as the zero past-tense morpheme in "that hit it" is morphemically present). So strongly customary is this reduction that to utter the repeated word in overt form lends itself immediately to the interpretation (at least in the case of nouns or of names like John) that they are not repetitions, that is, that two referents are intended. (For more on this, see _Language and Information_ starting at the bottom of p. 17.) This is just one relatively simple type of reduction based on high likelihood. I needed to have some way of a control system determining likelihood, or else some other basis for saying when high-likelihood reductions apply and when not. Harris talks about likelihood in terms of frequency but I don't think this is necessary in a CT context, although the mechanism of associative memory is distressingly murky.

saying when high-likelihood reductions apply and when high-likelihood reductions--prior same as parallel--do not apply

saying when high-likelihood reductions apply and when high-likelihood reductions do not apply

saying when high likelihood reductions apply and when they do not apply

saying when high likelihood reductions apply and when they do not

saying when high likelihood reductions apply and when not

It happens all the time, and not because I am controlling for nonverbal perceptions, but rather because I am controlling for conformity to the conventions for high-likelihood reduction in English. All languages have this, but they differ in their detail.

This is a very different matter from what you are looking at, which is the rarity of utterances like, say, "the blue aardvaark glided over to

the mantlepiece." Wrong problem, and not a problem. This rarity does not make the sentence any less easy to say or to understand. I tried to say this before.

>Why is it that "he ran in place" is OK but " he ran place" is not?

Because "run" is an operator with a single-noun argument requirement. It may come under another operator like to, from, in, around, for which require an operator and a noun as arguments (i.e. for which our control systems for language require there to be . . .). Having a noun in the position for a second noun argument without the higher operator (preposition) violates that requirement and violates our perception of conformity to the conventions of the language.

To say we associate these utterances with perceptions of some individual animate thing performing some actions that we call running, and that no other thing is necessarily involved as the recipient, object, patient, etc. of those actions, is only to say that that association between verbal perceptions and nonverbal perceptions exists. Nothing causative can be presumed because (a) that would beg the question and (b) other languages do it differently. I can say the equivalent of "he ran mountain" in Achumawi. You could insist that the verb stem should be translated "run to" but then you have a problem when there is no destination specified, or when he ran from the mountain, or indeed when he ran in place. The directional specifiers -mi "hither" or -ki "thither" could both be used in such a stem with aq'o "mountain" following and it could mean running either to or from the mountain depending on the relative positions of speaker and mountain. Yes, we have to figure out what the links between words and nonverbal perceptions are, and yes, the latter are crucial for understanding understanding, but no, it is not simply that word meanings are dependent upon and fall entirely out of nonverbal perceptual dependencies, some of it works the other way out of the arbitrariness of the language, somewhat differently for different languages.

>The kind of answer I want is a description of how we make sense of a
>sentence, so that the second version creates an error. What is the error?
>The kind of error I'm looking for won't be found in lists of words. It's
>isn't just a violation of our expectations concerning what classes of
>words are to be assembled in what way (although when my question is
>answered, that sort of violation may still be part of the remaining
>remainder). It's a violation of the process by which we bring words and
>other experiences together into a form such that they properly mean each
>other. I'm trying to understand meaning, as a transitive verb.

Bill, I'm not talking about lists of words. I'm talking about dependencies among perceptions. (The dependencies themselves are perceptions, of course.) Control of some of the perceptions manifests as speech, writing, etc. Control of others of the perceptions manifests as other kinds of behavior. Control of language depends partly but not entirely upon control of nonverbal perceptions. Control of nonverbal perceptions is certainly influenced by and may in some cases be said to depend upon control of language. We generally notice this only when language-based expectations lead us astray. People saw the label on the barrels saying "inflammable" and did not take precautions for fire

because they thought it meant "not flammable" (example reported by Whorf).

The sentence "He ran place" creates an error. Do you mean "He ran in place," "He ran the place," "He ran apace," or something else? How I interpret that to reduce the error depends on what I think you are talking about and what I can say to myself about that topic.

On the one hand, my attempts to amend what I heard to something I can believe you intended depend on my nonverbal perceptions (present, remembered, imagined) of the referent of "he," the activity of running as that entity might engage in it, the intended referent of "place" (or pace, etc.). I have no quarrel whatsoever with any of that and have said that CT shows how to fill a big gap for me.

But my attempts to amend what I heard to something I can believe you intended depend on the other hand also on my verbal perceptions (present, remembered, imagined) of what other sorts of words I must say or intend to say if I say the operator "run," and my perception that the operators and arguments must match up and so meet my expectations about their dependency structure as they almost always have in my experience.

Where they do not, I lean on the nonverbal dependencies and expectations to fill in the gaps, and in response to utterances like that which Martin posted I typically ask a question or produce my own paraphrase or extension of what I think was intended to demonstrate and verify comprehension. And where I have trouble connecting the speaker's words to my own world of nonverbal experience, I lean on the syntactic and semantic dependencies of the words to figure out what a "glorphic" might be, or to figure out how I might have been misled.

>Modeling is an attempt to see under the roulette table.

Modelling can also be concerned with the design of the roulette table itself. That is a matter of rules and conventions. Language is like that. If you don't get the structure of the roulette wheel right in your model, you're playing with a hole in your racquet.

>I said that control systems (other than the class of devices called
>servomechanisms -- artificial control systems) do not reside outside
>people. You say "they do reside outside people because that is where they
>put them."

Bill, please reread what I said. I did not say that control systems reside outside people. I said that reference values--standards and norms, conventions, etc.--are outside people because that is where they put them.

I say again what I said before (910808 1208) but you missed it:
>What you are resisting is a notion of suprapersonal control systems.
>But you don't have to assert that to talk about structures of social
>convention.

You said (910805.0900):

>The same phenomenon exists in every aspect of a society, not just in
>language. PCT tells us that all rules, conventions, laws, and so on (by
>which people actually live) must, in order to be effective, exist as
>reference signals at the appropriate level inside each person.

I agreed with this.

You said:

>They do
>not reside outside people, even when they're written down or present as
>physical constructions. They are not implemented by any mechanism outside
>individual human beings. There are no natural control systems outside the
>individual human being. Not even in a society.

I disagreed with the first sentence, while agreeing with the other two. I said that not every pattern or structure that exists in the world exists because some single superordinate control system is controlling it. I said that some patterns and structures exist because control systems agree among themselves about them and maintain them *cooperatively* by treating departure from them, by either themselves or others, as errors. The fundamental error, I suggested, was with respect to a perception of membership that mammals and perhaps other living control systems find extremely important. Things like "If you and I disagree about this convention that I have learned from others as one of many tokens of membership, then one of us is not a member, and I'm betting it's you, because I sure don't want it to be me. So let's reach agreement about either the convention or your status." I suggested that one benefit of such socially inherited and cooperatively maintained structures is that they enable greater degrees of coordinated action in the affairs of the moment. Language, structured social relations, and the like are not ends in themselves, but means. They are tools, implements for coordination. If people had no need to come to agreement about things and coordinate their actions there would be no need for any of this. Neither would they be people.

>All this goes to show that there are no "social reference levels."

No. It only argues that there are no social *control* *systems*. As I recall the discussion a while back, we don't know where the highest reference levels come from. I'm proposing one source for them.

>With respect to language, I'm trying to show how language can appear to
>be a thing, a universal force or rule, without actually being that.

I don't know what you mean by universal force or rule. Words like this lead me to wonder if you're again resisting something that I am not intending.

>My argument is that individuals are entirely responsible for such things,
>but that in their need to avoid direct conflict and in their natural
>resistance to disturbances, they seem to be under the control of
>something larger than themselves. In fact, they are: they have no choice
>but to go on being control systems.

I think this is just right, with some qualms as to what you mean by "need" and "natural," and with the caveat that individuals, particularly children, very often determine how to exercise their responsibilities by the example of others. They choose what is expected. To it I would add that people *cooperate* in avoiding direct conflict and in resisting disturbances, and that by way of doing so and in order better to do so they *collectively* create and maintain systems of social convention and expectations. For any child that is born, the *natural* world around her includes not only trees and stones and animals but also phonemes and gestural conventions and personal advertisements of relative social status. There exists a correlation of the phoneme sequence "dog" with a particular range of kinds of perceptions and expectations--barking, tail wagging, warm fur, doggie smell, panting. If she is (was) Achumawi the range is much wider, as I have noted. In either case, the correlation is a matter of convention.

The only control individuals are under is self-imposed. That does not make it any the less real. They are mistaken in attributing real authority to society, or a system, etc. But that does not make social sanctions any less real or affecting: people do resist error in matters of social convention, and that resistance does have the effect and often (by no means always) the intent of sanction. Like it or not, for example, academic success in the public schools is strongly correlated with conformity to white middle-class norms of comportment and language use.

If a person acts as if a certain tree is to be avoided, that does not make it so. He may know something about that tree, or he may be delusional. If the members of a community agree that a certain tree is to be avoided, and the avoidance and the agreement about avoidance are among the tokens by which one is known as a member of that community, and disconformity would among other things call in question one's probity and reliability in the community, etc., or invoke more serious sanction such as vengeance by members of the nkanga clan to whom the tree is sacred, that still does not make any difference in the tree or in what one could perceive if one went up to that tree and touched it or climbed it or cut it down, so long as no one else was aware. The prohibition is not a property of the tree. It is a property of social relations in that community. The tabu is no less real for all its physical unreality and perceptual nonexistence (absent other people). It is of a different sort of reality from physical reality (physics) or psychological reality (PCT of independent control systems). It is of what Durckheim long ago in distinction from these called social reality, and to understand it we need a PCT of interdependent, cooperative, but still autonomous control systems, which accounts for how and why control systems might donate some of their independence for the sake of social agreements that enhance their autonomy. Without that, language will remain inexplicable. So will the higher levels of the control hierarchy.

This is hard news when we want things to have a demonstrable relationship to what we can directly model now. Unfortunately, we have no choice but to take it all on at once.

Bill Powers (910811)

>TUNA BITING OFF WASHINGTON COAST

This headline is not ambiguous if spoken aloud. "Off" has a higher level of stress in the funny version, and there are other differences. This is then the same sort of thing as Joel's pairs of sentences that differed only by placement of commas, except that we have no conventions about punctuation marks to distinguish "biting off WC (from X)" from "biting off(shore from) WC."

>trying to see past the words to the silent perceptions
>they indicate. Think of the category "dogs," and then discard the word
>and look at the experience it means. THAT is a category.

Bill, I am beginning to wonder if there are in fact no category perceptions. Whatever the basis of the hierarchically nested semantics of words may be, could it be that we impose it on our nonverbal perceptions, rather than the other way around? This provides one sort of answer to the question, how could it be that the Achumawi category that we translate "dog" includes what we must also translate "horse" and "slave"? Instead of learning the culture-specific category-perceptions and the culture-specific words for them, we learn only the category-words and their synonym-like partial interchangeability with one another when we use them to refer to nonverbal perceptions of dogs, etc. The associated percepts are always exemplars, not some generalization of all possible exemplars.

You take up a different aspect of this in your next post, see below, re recursion.

I haven't yet read any of your later stuff that would explain how you came to split the 1973 sequence level into event and sequence, with relationship and category interposed. A flag is raised in my head about the rightness of those levels, and I gather in yours too.

From Bill Powers (910812.0300)

I too like the reordering. Another anecdote: an old Indian hunting trick for spotting game is to move one's head (and upper body) back and forth laterally so that objects at different distances appear to move relative to one another and so stand out. This would be the way to see Bill Cunningham's crane. If you stand perfectly still, that deer can't see you. Then it lowers its head to graze, and in doing so it spots you again because your configuration stands out against the background, and you can bet it is continuing to watch you.

>Now to return to the general "recursion" problem.

>I think we can identify this problem as one of verbal manufacture. It
>comes from our ability to try out arbitrary manipulations of symbols
>according to rules but without regard to perceptual or "factual"
>constraints.
. . .
>We have
>to look at experience to see whether the proposed meaning is imaginary,
>nonexistent, or part of what we call objective or directly-experienced
>reality.

I am making this sort of proposal about the category level, above. We can apply all the following words to a given individual: thing, animal, human, boy, Fred. We can apply all but the last two to another individual, substituting girl, Joyce. We can apply all but the last three to a third one, substituting dog, collie, Lassie. These partial synonymities have to do with definitional statements about what all collies or all dogs or all humans have in common, and those definitional statements correlate with perceptions of the identified features (white ruff, pointed ears and muzzle), but there is no collie-perception, one only perceives *that* collie, and those features that satisfy the definitional specificiations of what kinds of dogs are collies. The defining features are associated with the superordinate category-word, but as features and not as the category itself.

I think the logical positivists had the right intention about cleaning up language and making it accountable, they just lacked appropriate criteria for accountability, being caught up like everybody else at the time in S-R notions of objectivity. Your discourse about the need for verifying the relation of language to nonverbal perceptions sounds uncannily like the logical positivists. It is important to emphasize the distinction from them, because they got into a lot of difficulty and their program is generally regarded as a failure.

Bruce Nevin
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Date: Tue, 13 Aug 1991 17:58:09 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: reply to David
In-Reply-To: Message of Tue,
13 Aug 1991 16:08:20 EDT from <bnevin@CCB.BBN.COM>

From Bob Yates

I am a bit confused, Bruce, about your claim that have, had of the perfect tenses is just like the verb have meaning possession. Syntactically they certainly are not.

1) John has lived in Chicago for 6 years.
2) Has John lived in Chicago for 6 years.
3) John has lived in Chicago for 6 years, hasn't he?
4) John has not lived in Chicago for 6 years.
vs.
5) John has a new car.
6) Does John have a new car?
7) John has a new car, doesn't he?
8) John doesn't have a new car.

If they were the same verb, where did those DO forms come from in 5-8?

How would your analysis apply to the German perfect tenses which have a distinction between transitive and intransitive verbs and the type of

auxiliary they require?

Date: Wed, 14 Aug 1991 11:22:09 +0100
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Francis Heylighen <fheyligh@VNET3.VUB.AC.BE>
Subject: Re: Stabilized Images

>[from Gary Cziko 910813.1330]
>
>>Gary Cziko's reference to the vision experiments came immediately to mind, but
>>I couldn't supply the reference. I'm glad he did.
>
>I didn't quite. I believe Turchin refers to a 1960 Scientific American
>article. I don't have the reference here in my office now but I can post
>it when I get back from Durango, although I'd be surprised if Bill Powers
>were not familiar with this research.--Gary

I just have Turchin's book here by hand, and the original reference is:

R. Pritchard, "Stabilized Images on the retina", Scientific American 204, no. 41 (June 1961), p. 72-78.

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Christian Haider <haider@GMDZI.GMD.DE>
Subject: Don't get any email-bodies anymore
In-Reply-To: Your message of Tue,
13 Aug 91 10:55:20 EDT.
<9108131526.AA08232@exunido.irb.informatik.uni-dortmund.de>

Hello,

Since yesterday, I get emails from this group, but the text is gone!?!?!?!?!

Anybody the same, or is it just our mailer?

Bye,
Christian

Christian Haider
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Date: Wed, 14 Aug 1991 13:53:47 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: reply to Bob Yates

[From: Bruce Nevin (910814 1111)]

Bob Yates (Tue, 13 Aug 1991 17:58:09 CST)

The "do" questions are available in the unreduced form, but not after the reductions.

1) John has the state of his living in Chicago for 6 years.
2) Does John have the state of his living in Chicago for 6 years?

And so on. The restrictions (as always) are in the reductions. This is what is meant by an "unrestricted source": any restrictions are semantic, not grammatical, that is, arbitrary convention. It is this that makes Operator Grammar of particular interest to the CSG. Relations among words in the unrestricted sources presumably correlate well with relations among nonverbal perceptions, whereas this correspondence is obscured by arbitrary grammatical restrictions in the reduced forms more ordinarily encountered.

The interrogative in general in English has a performative source: I ask whether . . . or . . .

> I ask whether John has the state . . . or not
> I ask: has John the state . . . or not?
> Has John the state . . . or not?
>
> I ask whether John does have the state . . . or not
> I ask: does John have the state . . . or not?
> Does John have the state . . . or not?

If you go to the do-question, you can't then apply the reduction to the perfect. This is not a peculiarity of this construction, but a generalization about reductions: they apply when the conditions for them first arise (when an operator is entered on its arguments) or not at all. The reduction to have . . . -en must happen upon entry, and cannot happen after entry of "do" on "have." A great deal of apparent complexity in the grammar falls out from this simple generalization. I could send you an unpublished paper of mine "Unbounded dependencies in operator grammar" that discusses other ramifications of this at some length, if you like.

In parallel fashion, the reduction of "have the state" (or of the even more obligitarily reduced compound "have the Ving-state") must happen when it enters on the argument operator ("live" in the example). After

the reduction, "have" is no longer available to be argument of "do".

The "do" here appears to enter as a carrier for the argument-indicator -s or a tense morpheme (a) when a verb is separated from the operator-indicator or tense, as in the interrogative and negative, or (b) when a verb is zeroed but its operator-indicator of tense is not ("She left because he didn't"). (The tense affixes are reductions of higher operators expressing time relations, and they replace the operator-indicator.) However, this seeming auxiliary "do" can be derived from the same operator on (N,O) as in "Sorry, I don't do editing" (from something like "I do one's editing of things," where "I" and "edit" are the N and O arguments, respectively, of "do").

After the reduction to the perfect, the "have" of the perfect appears to be a separate, especially restricted form distinct from the ordinary "have". It is typical that reductions introduce restrictions. Conversely, the work of analysis was to find an unrestricted source for all such resultants.

Look in _GEMP_ for the discussion of have . . . -en, the interrogative, do, and the auxiliaries.

>How would your analysis apply to the German perfect tenses which have a
>distinction between transitive and intransitive verbs and the type of
>auxiliary they require?

It is precisely in their reductions that languages differ. It is particularly difficult to find unrestricted sources (i.e. where the only restrictions are semantic, not grammatical, a desideratum for CT) for the auxiliaries of English. In general, the unrestricted source sublanguages for different languages are very similar, except of course for vocabulary and differences in ranges of meaning for individual vocabulary items. I don't know what work has been done recently on German but could dig up some older stuff if you like.

Bruce Nevin
bn@bbn.com

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Date: Wed, 14 Aug 1991 10:37:50 BST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: mar@CS.ABER.AC.UK
Subject: transition&configuration

[From Marcos Rodrigues]

K.Lorenz in his book "The Foundations of Ethology" (Springer-Verlag, Wien, 1981) describing fish behaviour (p116):

"Still it was surprising to find that the reaction of _Haplochromis_ males to the dummies which elicited rival fighting did not depend at all on their similarities to the real rival. An elongated block of grey plasticine attached to a glass rod holder evoked intense fighting if it were moved in such a way as to imitate the tail beat of a rival - an easy thing to

accomplish by simply rotating the glass rod between the fingers..."

This is not surprising for us any more.

Marcos Rodrigues
mar@uk.ac.aber.cs
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Date: Tue, 13 Aug 1991 14:05:53 BST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: mar@CS.ABER.AC.UK
Subject: transition & configuration, emotions

[From Marcos Rodrigues]

Bill Powers [Aug 12] --

Reading your post I had a kind of gestalt perception: I've seen this distinction before (that transition comes before configuration). It's amazing how we go through reading books, papers, posts, etc. and don't realise some important things.
Gerald Edelman ("Neural Darwinism", Oxford University Press, 1989) describes object and auditory recognition in infants. His interest is "in the question of their capacity to integrate, categorise, and generalise without formal tuition."
It is in there, clearly described, that infants can recognise movements before they can recognise configurations.
Unfortunately, I've missed that at the time I read the book.

To all --

Re. to emotions, it is quite intriguing to me their existence in evolutionary terms. I'm not naive on this matter; I'm totally ignorant. My question is the classical one: "what for?"
I'll make some hypotheses.
1. Emotions are mechanisms used by evolution to increase the probability that the stronger animal has the largest number of offsprings.
So, in group living animals the weaker animals, scared of the leading male, are prevented from having offsprings.
2. Emotions are mechanisms used by evolution to increase the chances of survival (of the group). So emotions work as social control references, tying individuals together.
3. Emotions are nothing but learned things used for pseudo-control of other people's behaviour. It comes to my mind the "Kmer Rouge" (sic) in Cambodja. In order to create a killer army they used children, teaching them that killing people was right. They couldn't use adults, because they had the feeling that killing was wrong, and would feel miserable if they did it (they wouldn't do it the way their leaders wanted).
This third hypothesis is outrageous, but it seems to me that some emotions are learned.
4. none-of-the-above.

It's not clear to me where emotions fit into HCT. Don't we control emotions? I do.

Best,
Marcos Rodrigues
mar@uk.ac.aber.cs

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Date: Thu, 15 Aug 1991 11:25:20 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: emotions

[From: Bruce Nevin (910815 1122)]

One view is that emotions are programs for quick response to stereotypically recognized situations. The recognition may be quick and dirty but it is apt enough to have good survival value. I have a photocopy of an article on this somewhere around but not handy. Issue for us is that the stereotyping is off the mark more often than it is for a gibbon, say. I think we don't control the emotions (we can't) but rather what we do about them.

This message itself qualifies as a quick-and-dirty response, so it may well be off the mark in many respects. Fortunately, many CGSers are now talking to one another in Durango face to face and won't get to this right away. :-)

Bruce Nevin
bn@bbn.com

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Date: Thu, 15 Aug 1991 12:12:11 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: communication, 2 senses

[From: Bruce Nevin (910815 1208)]

Bill Powers (910811)

I know you won't get to this for a while. I imagine with some considerable envy the pleasure, excitement, and mutual engagement of the CSG gathering now under way in Durango, and look forward to hearing some of the spinoffs.

>I think that standard English is whatever other English speakers seem to
>grasp the first time, as you meant it. I can understand Vermont and
>Texas and Georgia, but not Sloppy and not Foreign, meaning truncated or
>masked by unfamiliar intonations, emphases, and phonemes. . . . When it
>takes a lot of effort to turn spoken (or written) language into meaning,
>the chances of taking the wrong meaning escalate -- you have to put too
>much interpretation into it. Hearing standard English is effortless,
>which is how it feels when control is easy and skillful on both ends of
>a communication. I'm sure it's the same way for monolingual speakers of
>any language that is spoken with the aim of being understood clearly.

>What I'm getting at isn't some version of chauvinism, but a possible way
>to understand where this idea of the "ideal" language comes from. It's
>not JUST a matter of consensus. It's a matter of making distinctions and
>avoiding unintended meanings. . . . In most human affairs, language is
>just a means to nonlingistic ends. . . .

There are two kinds of processes under the label "communication" that we need to distinguish carefully. One is about the error-free transmission of information. The syntactic and semantic structure of language has that function. This is the function you emphasize in the above discussion.

The other kind of process is about relationship. Communication in this sense is done nonverbally. This includes nonverbal aspects of our use of language: tone of voice, emphasis on certain words, choice of some words rather than others ("honey" rather than "bitch," say, or the pretendedly agent-less "impersonal" sublanguages of the sciences). Something from Ed Ford about "quality time" depends on this kind of communication by which we affirm and maintain relationship.

It is in this aspect that language differences become important not just as potential impediments to the error-free transmission of information (aspect 1) but crucially as indicators of relationship, such as membership in the same or different communities, relative social prestige, and so on.

Bill Labov has a measure he calls the "index of linguistic insecurity." This is derived from the disparity between the model of linguistic performance that people believe they follow and their actual performance. In studies in New York (replicated in other places), upwardly mobile (and downwardly vulnerable) middle-class speakers of English have the highest index of linguistic insecurity. They really believe that they are conforming to their norms (which are derived from traits of upper-class New York English of a somewhat earlier period). They "normalize" what they hear themselves saying to meet these expectations. When attention is called to it, as in a formal interview situation or when speaking to a person of high prestige, etc., they actually do conform to their norms. When the interview is interrupted by a telephone call, a visit by a neighbor, etc., there is greater disparity. When samples of speech are taken in informal situations, with attention on communication in the first sense and communication in the second sense is taken for granted, disparity is greatest.

This is evidence for some of the perceptions that people are controlling when they use language. Each human being has internalized the relevant norms and control is entirely internal. The norms (reference values) are held in common by all members of a community. Lower-class speakers know them, but make no effort to conform to them. They also know the lower-class norms to which they conform, and experience and impose social sanctions ("don't put on airs!") for disconformity just as surely as anyone else. Similarly for upper-class speakers.

Labov's work on the social motivation of sound change, to which I referred previously, demonstrates pretty strongly that regional and social diversity in language is increased by processes by which

individuals identify, mostly at puberty, with one group of people or another, communication in sense 2. Working against this is the need for intelligibility in order to communicate in sense 1.

People control for communication in both senses.

Bruce Nevin
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Date: Thu, 15 Aug 1991 23:45:55 -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: PCT and language

From: David Goldstein
To: Bruce Nevin, Bill Powers, others
Subject: language and PCT
Date: 08/14/91

The first issue was the general one of how to select a theory of language for PCT? Bruce, you gave some criteria which included the principles of: coverage, simplicity and universality. I have no problems with these principles. They seem to be the kind of generalizations which is found in books which compare different theories of something, for example, personality.

I have been thinking about this issue from the point of view of integrating theories of psychotherapy with PCT. One important principle I would add is: What theory of human beings is implied by the theory of language? A theory of language which is based on a theory of human beings which comes closer to PCT would be preferred other things being equal. Each theory of language needs to be examined with this question in mind. I don't think it is possible to have a theory of language which is independent of assumptions about how people work. What are these assumptions for Harris?

Diver makes his assumptions very clear. Kirsner (1977), a student of Diver discusses the basic assumptions in an article entitled: The Theory (Columbia University Working Papers in Linguistics, No. 4). (1) The communication orientation: human language is a particular instance of a device of communication. (2) The human factor orientation: human language is a particular instance of human behavior. Each of these orientations has implications for the theory of language which is developed and Diver's students use these two orienting attitudes in their linguistic analyses.

Bruce, the communication orientation is one which you discussed in your last post. You disntinguished between two senses of the word communication: one was the contribution to the message of semantic and syntactic aspects of language while the second was the contribution of the relationship between the people talking.

I like the communication orientation you are taking. However, I think that the two categories you are setting up are not independent or exhaustive of what is meant by communication. The way that Diver talks about it makes sense to me: words and grammatical signals as perceived contribute meanings, the situation as perceived by the person contribute meanings, and the message perception which is constructed is based on all the different meanings which the listener experiences. I am using the term meaning the same as Bill Powers, namely, nonverbal perceptions.

Given the set of meanings which are going on inside the listener from linguistic and nonlinguistic sources, the possible messages which are consistent with the meanings are narrowed down to a few in most cases. When I was studying Chomsky's approach to language, I remember spending a lot of time on sentences which can be interpreted in two ways, for example: Flying planes can be dangerous. In these cases the grammatical and lexical signals are not sufficient to know which message is being communicated. In a real life context, which provides other meanings, this sentence would not be perceived as ambiguous.

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Date: Fri, 16 Aug 1991 11:12:41 -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: communication, 2 senses

[from Joel Judd]

This is our chance--we come up with an imminently marketable CT scheme while everyone's away, and make a million on it...no one will ever know...

Had to ask a couple of questions about the latest language comments. I appreciate this discussion by the way. It's topic is the next chapter in my dissertation, except that I've got to be concerned with TWO (or more) languages in the same person.

>>What I'm getting at isn't some version of chauvinism, but a possible way
>>to understand where this idea of the "ideal" language comes from.
>just a means to nonlingistic ends. . . .

This appears in SLA literature under the terms "native speaker," "ideal speaker-hearer,"target language," "L1," target language norm," and probably a few more I can't remember right this moment. I had mentioned this before when there was a heated statistical discussion. What many in language learning have realized is that it's one thing to talk about a native speaker, it's another to actually find one; that is, ANY ONE speaker may conform to some of whatever notions we hold of a native speaker, but as likely as not won't conform to others. The examples from work such as Labov's seem to exemplify a PCT conception of language. I can't remember--is it possible to see what an individual did in his data?

>There are two kinds of processes under the label "communication" that we
>need to distinguish carefully. One is about the error-free transmission

>of information.
>The other kind of process is about relationship. Communication in this
>sense is done nonverbally.

I understand the distinction you make, but I don't know why it's necessary if language is viewed as part of the hierarchy. Communication as relationship would seem to fall under SYSTEMS concept, or what I think I've been mentally calling "language" at the SYSTEMS level of the hierarchy. Communication as transmission of information can be accomplished with lower levels. If I'm deathly ill in a foreign country, or in a particular neighborhood in this one, I don't think I'm going to be too concerned about my relationship to the doctor.

This relationsip issue IS, however, crucial in language learning. It's what Gardner and Lambert and Schumann have been harping on for years with their talk about acculturation and social psychological variables, etc., but they've been doing it from a behaviorist perspective using ever more complex multivariate statistics.The problem is that they view SLA as a linear system, with acculturation factors (culture shock, attitude, etc.) a cause of eventual language acquisition:

ACCULTURATION --------> L2 VERBAL INTERACTION ---------> ACQUIRED LANGUAGE FACTORS

>Labov's work on the social motivation of sound change, to which I
>referred previously, demonstrates pretty strongly that regional and
>social diversity in language is increased by processes by which
>individuals identify, mostly at puberty..

This is one more piece of evidence for puberty as some sort of cutoff for "successful" SLA. This process of identification which occurs at puberty has important ramifications for the hierarchy in general. I wish there were more to go on regarding develpmental aspects of the hierarchy in general.
Joel Judd
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Date: Fri, 16 Aug 1991 12:10:54 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: PCT and language

[From: Bruce Nevin (910816 1206)]

David Goldstein (08/14/91)

>One important principle I would add is: What theory of human beings is
>implied by the theory of language? . . . I don't think it is possible
>to have a theory of language which is independent of assumptions about
>how people work. What are these assumptions for Harris?

First cut: As mentioned, Bloomfield and others essentially gave up on psychology because it had little useful to say to linguists in the 1930s. Aside from a commitment to there being some sort of mechanistic explanation (explicitly identifying "mechanism," a form of materialism, with science), and identifying behaviorism as the best available

candidate, Bloomfield in _Language_ essentially encapsulated the psychological issues of meaning and communication and set them aside, turning to the testimony of the *form* of language. The latter turns out to have had more to tell psychologists than vice versa, especially in Harris's work. Formal linguistics, then, is explicitly not dependent upon a prior theory of how people work.

Second cut: this is not to say that Harris has no assumptions about how people work, only that, for reasons just acknowledged, these assumptions play no major role in his theory. Language is seen as a combination of social convention and (some hand-waving) the general requirements for information processing in the brain. The contribution of social convention is seen in the identification of phonemes and of morphemes, in the reductions, and in the detailed distributional ranges of vocabulary, which correlate to differences of meaning. (See first set of quotations at end of this post.) The dependencies of operators and arguments in the base sublanguage are of a sort that is characteristic of much that human beings do, and seem to follow from general information-processing requirements. (See second set of quotes.)

Control theory specifies the information-processing requirements with great precision, and provides a framework in which we can integrate them with the human propensity for "choosing what is expected," that is for setting some internal reference signals to values that are publically known and that others share. Individuals give up some independence so as to enable cooperation and thereby enhance their autonomy. Independence is absence of dependence. Autonomy is mastery in one's own realm. Autonomy requires knowledge of boundaries and interfaces with others and appropriate management of those boundaries and interfaces (leaving unspecified for now what precisely is meant by the familiar metaphors "boundary" and "interface").

>Kirsner (1977) . . . (1) The communication orientation: human language is a
>particular instance of a device of communication. (2) The human
>factor orientation: human language is a particular instance of
>human behavior.

I don't know what is meant by (2) but suspect that CT says it better than Diver? In any case, we are talking about (1) communication. I distinguished between (a) error-free transmission of information, which is the function of the structure of language, and (b) affirming, defining, and maintaining relationships. You identify the latter as

>the contribution [to the message] of the relationship between the
>people talking.

I do not think that there is a single entity "the message" to which these two factors and others contribute. Instead of there being a single message process, there are at least two processes. One is the transmission of information. The other is the negotiation of relationship--relative status, intimacy/distance, and such concerns that we share at least with all our mammalian cousins. As Bateson says, when his cat mews at him she's not saying "feed me" so much as she's saying on a more basic level "dependency! dependency!" She's communicating

about the relationship with him, which at the moment happens to concern means for reducing an error signal connected with hunger and food. It is because of the relationship that her error signal becomes an error signal for him. Without that relationship no amount of information transmission would have the effect of reducing the error for her.

Your concern with "contribution to the message" seems to arise from the problem of ambiguity, determining what the other person intends. Yes, the current character of the relationship does contribute to this. So does other contextual information. From knowledge of the occasions when the cat chooses to "lean on" her relationship with him, of her s eating habits and when she was last fed, and so on, Bateson could determine that she wanted food. The cat was not saying "feed me," she was saying "I have an error signal, and I am depending on you to identify what I need to reduce it and provide it to me." She may do things that might evoke memories of times when the same dependency has worked to reduce the same error in the past, like running over toward the food dish.

Processes of communicating about relationship are more basic and more continuously active than the intermittent processes of transmitting particular messages. Even in human intercourse involving language there is less informing and more fence-mending and "touching base" than perhaps we realize, because our attention goes to problems of determining the other's informational intentions. Informing raises the question "what are you trying to say?" This more global and pervasive kind of communicating raises questions like "what are you trying to do? What are you making us out to be? What do you think I am? Who do you think you are?" The answers are not commonly given in explicit, informative words. Nor are the questions normally articulated explicitly.

Returning to your concern with determining the intention when words are ambiguous:

>I . . . think that the two categories you are setting up are not
>independent or exhaustive of what is meant by communication. [For Diver]
>words and grammatical signals as perceived contribute meanings, the
>situation as perceived by the person contribute meanings, and the
>message perception which is constructed is based on all the different
>meanings which the listener experiences. I am using the term meaning the
>same as Bill Powers, namely, nonverbal perceptions.

>Given the set of meanings which are going on inside the listener
>from linguistic and nonlinguistic sources, the possible messages
>which are consistent with the meanings are narrowed down to a few
>in most cases.

I agree wholly with this. When word W comes in, it evokes a mesh of associations. Some of these are syntactic, associations with words that are operators on W and (if it is an operator word) arguments under W in memory or in imagination, plus their syntactic associations in turn. Each considerable subtree of operator-argument dependencies constitutes a proposition, an assertion, which is associated with remembered or imagined nonverbal perceptions. (I believe the association with nonverbal memories involves assertions, not single words, and our

somewhat murky ability to evoke images and the like from hearing or thinking about single words is actually mediated by propositions containing those words. This is of course arguable, and we may be able to figure out ways to test it. I would like to.) W typically is accompanied by other words, and only the intersection of their several sets of associations is kept active. Nonverbal perceptions in real time have their own associative-memory connections to other nonverbal perceptions (expectations, etc.) and to assertions about them. These include perceptions of relationship. As above, only the intersection of these many several sets of associations is kept active, and this intersection is increasingly narrow. Collectively, these many sources for associative memory are highly redundant. We can "lean on" other sources when words are ambiguous, or lost due to noise, or garbled because of the other person's awkward management of them. We ignore what doesn't fit the "consensus" as to what is going on, or "normalize" an irregularity as though the speaker had said it properly. We quickly normalize for dialect differences (e.g. "I wonder how that he could think that way"--Kentucky) so long as our relationship is such that we have a will to communicate and to understand, and we don't even notice that we have done so.

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Quotations.

1. Language as institutionalization

> Language is undoubtedly unique. But the individual processes that create langauge, as they were seen here, are not unique. The various constraints are not so entirely different from constraints that exist elsewhere in the world. Grammatical relation, such as being a subject or an object, is not something that is known anywhere else; but to have things depend on classes of other things and appear only if things of the other classes appear, is a kind of dependence that is not necessarily unique to language. Also, language is a demanding structure: some things are regarded as being in it and therefore right, and some things are not in it and are therefore wrong. But these demands, as we have just seen, can be understood as institutionalizations of a less demanding and a more naturally occurring use in the combinings of words. In other words, there are demands in language that are unique to language, but we have just seen that one can reach these demands by a process of institutionalization of custom, of convenience, of what makes sense. This does not mean that one can make language be simply whatever makes sense to the speaker. For langauge is public, and institutionalization. But it is important to know that the demands of language, the rules of grammar, are reachable as the end product of a process of useful institutionalization, from something that is not demanding and not unique. And of course, the process of institutionalization itself is by no means unique, being widely known--for better or for worse--in culture and in social organization.
>
> . . . The overall picture that we obtain is of a self-organizing system growing out of real life conditions in combining sound

sequences. Indeed, it could hardly be otherwise, since there is no external metalanguage in which to define the structure, and no external agent to have created it.

--Zellig Harris, _Language and Information_ 110-113.

2. Language as formal structure

This book presents a grammar of English, as an example of how language structure can be derived from--not merely described in--a particular mathematical system.

--Harris, _A Grammar of English on Mathematical Principles_, 1.

The central problem in this work was to find what objects and relations could effectively characterize with the least redundancy those combinations of words that occur as English sentences against those that do not: to find the simplest system adequate to the task, with as little as possible unused capacity in the apparatus of description. It dictates a minimum of multiple classification of words . . . and maximum derivability: the theory has one primitive relation (argument requirement) and not many derivational steps (reductions), although the chains of derivation for a given sentence may be long.

Minimizing redundancy is particularly important in discovering the structure of language because what is given empirically in language is redundancy . . . : not all sequences of words form sentences. As redundancies are eliminated from the theory, leaving the fewest constraints on the application of the relations provided by the theory to its objects, we see how and why the remaining redundancy is essential to the structure of language.

In a different way minimality also dictates what relation there should be here between form and meaning. Sentences as well as words have meaning, and it is necessary to state the meaning of a sentence in terms of the meanings of its words in addition to stating how it is constructed as a combination of words. The simplest treatment is to design the theory with a meaning attached to the primitives of the structure in a way that its structural components yield the meaning of the sentence at each step of construction. . . .

--ibid. 26-27

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Bruce Nevin
bn@bbn.com

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Date: Fri, 16 Aug 1991 14:02:42 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: Response to Joel

[From: Bruce Nevin (910816 1343)]

Joel Judd (Fri, 16 Aug 1991 11:12:41 -0500)

>is it possible to see what an individual did in [Labov's] data?

Depends on the study. Some were anonymous informants in routine encounters in department stores. There, the consistent choice of one value of a selected variable (r sound, say, or vowel height) over another by all members of a social class, as indicated by their patronage of one store rather than another, was the significant fact. (His studies were more subtle and sophisticated than this surprisingly simple expedient may make them sound.) In the paper on the sound change on Martha's Vineyard, or the one on ability of informants to distinguish supposedly merged vowels in Northumbria, while convinced there was no contrast ("Uses of the present to explain the past" or some such--I could look it up for you), performance of individuals is identified and separately tracked.

>I understand the distinction you make, but I don't know why it's necessary
>if language is viewed as part of the hierarchy.

My purpose in making the distinction between two aspects of communication was not to claim that one could not be accounted for in the control hierarchy. My purpose was to point out that Bill was talking about just one aspect (informing), and that the other (relating) is pre-eminently social in nature. In addition, the first depends upon prior agreement to socially-set values of reference signals for phonemic contrast, phonemic constituency of morphemes, word dependencies and reductions, and association of morphemes with nonverbal experiences. These are pre-set agreements or there can be no communication (informing), though of course there can be communication (relating) without language, as Bateson's cat shows.

>Communication as
>relationship would seem to fall under SYSTEMS concept, or what I think I've
>been mentally calling "language" at the SYSTEMS level of the hierarchy.

Does Bateson's cat have systems concepts or "language" at the systems level? I'm not sure what you mean by "language" in quotes here.

Bruce Nevin
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Date: Fri, 16 Aug 1991 15:06:56 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: categorical implications

[From: Bruce Nevin (910816 1432)]

I said in my post to David:

>I believe the association with
>nonverbal memories involves assertions, not single words, and our
>somewhat murky ability to evoke images and the like from hearing or
>thinking about single words is actually mediated by propositions

>containing those words.

I thought immediately that this parenthetical aside was an overstatement, and meant to get back to it, but didn't. I think it is arguably true for operators, but that the argument is hard to sustain for primitive N. That is, if I say or hear or read "dog" I get some specific associations with experiences with dogs, if I say "wag" or "run" or "whine" I don't get such specific associations, and if I say "a dog ran" or "a dog whined" or "a dog wagged his <mumble>" I get specific associations again; similarly for less specific words as arguments: "an animal ran," "an animal whined," or even "something ran (whined)".

The mapping from nonverbal perceptions to words is one-many from the point of view of a given word (dog). Bill has said that this is because words apply to the category level, which in turn apply to perceptions of individual exemplars. I think this begs the question and am not convinced there is a category level of nonverbal perception, but of course I have not yet seen the justification for its inclusion after publication of BCP in 1973. The mapping is also one-many from the point of view of any given nonverbal perception, even granting (arguendo) that there is a category level, e.g. from the perception of a dog or of the category "dog" to dog, animal, vertebrate, something, etc. The existence of taxonomies on the verbal side requires one of the following:

(a) There is a corresponding taxonomy on the category level

(b) Only the lowest level of a verbal taxonomy maps onto category perceptions, and the higher levels are accomplished by mappings of words to words, so that the mapping to nonverbal perceptions is only through the most basic nouns

(c) All perceptions that map onto "Lassie" also map onto "collie" and onto "dog" and "animal," etc. The taxonomy is an artefact of the multiple connections from nonverbal perceptions to words, where there are more connections to more general words, which are duplicates of connections to multiple words of the next level down of the taxonomy.

Whatever is right, and I am not at all clear how to test for it, I don't see a clear need for any category perceptions that intermediate between the lower-level perceptions and the words. Do we have category perceptions for which we have no words (yet)? Are they not represented by some other perception that serves as a symbol? Then in those cases we have a mapping from nonverbal perception to nonverbal symbol just as with language we have a mapping from nonverbal perception to words, no need for an intermediating category level.

Got to run catch the train. Have a good weekend!

Bruce
bn@bbn.com

==========

Date: Fri, 16 Aug 1991 15:18:27 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: The Bladder Slugs of Yik

[From: Bruce Nevin (910816 1512)]

Following is from the Linguist List: Vol-2-411 just received.
I'm sending it for your enjoyment.

Date: Wed, 14 Aug 1991 15:03:50 +0800 (SST)
From: A_DENCH@FENNEL.CC.UWA.OZ.AU
Subject: The Bladder Slugs of Yik

For the general entertainment of all you Metaphor fans
out there, I am posting a first year undergraduate
assignment which I conceived (maybe in a drug-crazed
state but more likely commuting) some three years ago.
Sadly it is often the only thing introductory linguistics
students remember from their year of Linguistics 100.

Should any of you be inclined to use this yourselves, be
my guest. The assignment follows lectures on temporal
metaphor for which the following sources provided the
main inspiration and examples:

Foley, W. (1980) "Functional grammar and cultural
anthropology" Canberra Anthropology, 3:67-85.

Geertz, C. (1975) "Person, time and conduct in Bali". in
"The Interpretation of Cultures" London: Hutchinson.
pp 360-411.

Klein, H. (1987) "The future precedes the past: time in
Toba" Word 38:173-185.

Lakoff, G. & M. Johnson (1980) "Metaphors we live by"
University of Chicago Press. (surprise, surprise).

And I think my inspiration for the Bladder Slugs
themselves was most likely James Tiptree's sci-fi novel
"Up the Walls of the World", which I read years ago
somewhere but have never seen since.

Read on and have fun!

Alan Dench
Department of Anthropology
University of Western Australia
Nedlands, WA 6009
A_DENCH@fennel.cc.uwa.oz.au

University of Western Australia
Department of Anthropology

Linguistics 100 (UWA)
Tutorial Assignment: Temporal Metaphors

You are Chief Linguist aboard the starship Benjamin Lee Whorf orbiting the gaseous giant Arcturus IV. Your most pressing task is to compile an analysis of the temporal system of the Yik language of the freefalling Bladder Slugs. Given the following translations from Yik, what metaphor(s) seem to prevail in the Yik temporal system? Feel free to draw diagrams and offer explanations.

a. I perceived a large glabbage upperday.

b. The time for implosion is just below us.

c. The pressure increases, the light is dimming,
I'm plummeting old.

d. How deep until we fall on dense times.

e. Three days above I consumed a large splodj.

f. In the rarified days of my youth, I set my life
on a helical path.

g. The foolish Yik lives like a falling space rock.

h. At darkest bottom, we all meet at the centre.

i. The aliens, who live for eternity high above the days
of our youth, believe the universe is expanding. But
according to the great physicist Alblort Einslug, it
is merely moving up into its own "past".

j. All lives converge. At impact we will share our
common destiny.

k. I hope our bladderlings will rise into the upper
reaches of the brightest past.

l. I believe the shadows of our downtime bladderlings
fell across us upperday.

Linguist List: Vol-2-407
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Date: Fri, 16 Aug 1991 16:14:00 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: Response to Joel
In-Reply-To: Message of Fri,
16 Aug 1991 14:02:42 EDT from <bnevin@CCB.BBN.COM>

As the Chomskyan on the list, I feel a need to enter this debate. One of the problems I have having is the limited definition of human language. How is human language different from other animal forms of communication? Or is the assumption that it is different wrong?

This two types of communication mentioned by Bruce are clearly qualities of animal communication systems, too. Bees have a very remarkable way of communicating where a source of food is. That clearly is informing.

>My purpose in making the distinction between two aspects of
>communication was not to claim that one could not be accounted for in
>the control hierarchy. My purpose was to point out that Bill was
>talking about just one aspect (informing), and that the other (relating)
>is pre-eminently social in nature. In addition, the first depends upon
>prior agreement to socially-set values of reference signals for phonemic
>contrast, phonemic constituency of morphemes, word dependencies and
>reductions, and association of morphemes with nonverbal experiences.
>These are pre-set agreements or there can be no communication
>(informing), though of course there can be communication (relating)
>without language, as Bateson's cat shows.

Interestingly enough, those pre-set agreement for bees have to be biological. Are we left in the position of saying that bees and humans are the same when it comes to langauge? If we reject that, what are the characteristics of human language which the bees lack?

Bruce, if I understand some of your last notes, input to the language learner is crucial? What are the qualities that input must have?

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Date: Fri, 16 Aug 1991 22:41:47 -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: PCT and language

From: David Goldstein
To: Bruce Nevin, Joel Judd, Bill Powers, Bob Yates, others
Subject: PCT and language
Date: 08/16/91

While the cat example is cute, what does it really show other than people are often able to figure out what another animal(person) wants from signs of distress in a particular situation. Parents of crying newborns are in the same position.

The PCT perspective is that language is just a means to nonlinguistic ends. Communication has taken place when one person is able to create in a second person some intended experience by means of spoken or written words. If the words result in the desired message within the sender then the words have a good chance of doing the same within the receiver. The sender will control the selection of meanings and thereby control the message perceived within him-/herself.

The communication orientation in Diver's approach results in the

linguist looking for signals and associated meanings (nonverbal perceptions). The message is a perception derived from the linguistic meanings and nonlinguistic meanings. In PCT terms, I think that the signal/meaning relationships may involve signals involving sequence level perceptions. This means that the message involves program, principle and system level perceptions.

I will try to obtain the best single reference for Diver's theory by writing to him. Then I will post it on CSGnet. You can then judge for yourself. I know that Diver's students have analyzed many different languages. I know that Diver has the same attitude towards model fitting data as Bill Powers. I know that Diver has the same questioning attitude towards traditional concepts in linguistics (for example the ideas of noun, verb, rules, etc..) as Bill has towards traditional concepts in psychology (stimulus, response, etc..). Bruce, you and some of the others are the linguists. Perhaps, Diver is worth looking into along with Harris and others.

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Date: Mon, 19 Aug 1991 08:19:19 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: SIAM conference next month

Is anyone planning to attend the SIAM conference in Minneapolis next month on (engineering) control theory and its applications? (Notice just appeared on the cybsys distribution.)

Bruce
bn@bbn.com

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Date: Mon, 19 Aug 1991 09:51:27 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

[From: Bruce Nevin (910819 0803)]

Bob Yates (Fri, 16 Aug 1991 16:14:00 CST)

>One of the problems I have [been] having is the limited definition of
>human language.

How do you see human language being defined here, and how is that definition too strongly limited, that is, how would you extend it?

>How is human language different from other animal forms
>of communication? Or is the assumption that it is different wrong?
. . .
>Interestingly enough, those pre-set agreement for bees have to be
>biological. Are we left in the position of saying that bees and humans
>are the same when it comes to langauge? If we reject that, what are the
>characteristics of human language which the bees lack?

>Bruce, if I understand some of your last notes, input to the language learner
>is crucial? What are the qualities that input must have?

These are important open questions best answered by making and testing models. I don't have the answers.

As is well known, children reared in isolation don't learn to speak. Clearly, input to the language learner is crucial. As I understand it, the Generativist view is that exposure to adult language is required for setting of parameters, and that in in such cases as feral children the hard-wired mechanisms for language are present but never get "tripped." It would be relatively easy to formulate this view in CT terms, and I again invite you to do so. That might make the distinctions between the two views (learning vs. parameter setting) susceptible of test. One question might be, if it's just a matter of setting parameters then why doesn't it happen faster, and on the basis of less exposure to adult language? I don't know if the experiment has been done of raising individual bees in isolation, assuming they can be brought to maturity in isolation and then introduced into a hive without being attacked as intruders.

Operator grammar also predicts biologically innate mechanisms for control of dependency-on-dependency, for control of reduction based on likelihood, and for control of the correlation of words and word-constructions with nonverbal perceptions in real time and in memory and imagination. Except for the last, these need not be different in kind from mechanisms required for perceptual control in general. It does seem to me that the use of some arbitrary perception as a kind of index to a set of other perceptions in memory is of a different order. Whether this is the category level, or a symbolizing system that can be meta to any perception to which one can consciously attend, is one of the questions that we have been debating. The category or symbol level of control is not unique to humans. If innate mechanisms of only these degrees of complexity can suffice in a CT account of language, then it seems to me that the burden of proof of more complex innate mechanisms rests with those who propose them. Or do they not suffice? What additional complexity must CT accomodate, in your view?

When we look at bees' communicative behavior from our category or symbolizing level, they appear to us to be controlling for categories or symbols. But are they? Remember that e. coli appears to control for motions in 3-dimensions along a nutrient gradient in a medium, when in fact it is only controlling for intensity of a particular electrochemical reaction at (I guess) its cell membrane). What might the bee be controlling for when it "debriefs" from a successful foraging flight? Direction of dance relative to direction of sun, number of steps/wags relative to distance . . . important questions to which I don't have the answers. But we can agree, I think, that this system is less complex and less flexible than human language, and that while it is quite plausible that the symbolic quality of bee communication is in the eye of the beholder only and that bees may accomplish it with no category level of control, such a conclusion is not plausible for human language.

Two points of clarification: (1) Dependency-on-dependency refers to the fact that operator-argument dependencies are defined not in terms of argument words as some sort of list but more abstractly in terms of the argument-requirement of argument words: the nth argument of an operator must be a word for which one requires some other word as its argument--an operator--or a word for which one requires no argument--a primitive noun. (2) Likelihood pertinent for reduction is usually likelihood of repeating the same word in a pair of positions defined in terms of operator-argument structure, not the situational likelihood that has concerned us so much.)

I would say too while I am open to whatever kinds of innate mechanisms may turn out to be required to account for language and may be experimentally demonstrated to exist, I do have an ideological predisposition in favor of greater rather than less freedom for individual control systems viz for individual people. This helps to motivate me to look for explanations of the complexity of language structure and language use in a combination of perceptual control and social convention that postulates the least possible in biologically innate mechanisms. So does Occam's Razor.

What innate mechanisms do you believe are involved, and how do you support those claims in a control-theoretic account?

From: David Goldstein (08/16/91)

>While the cat example is cute, what does it really show other
>than people are often able to figure out what another
>animal(person) wants from signs of distress in a particular
>situation. Parents of crying newborns are in the same position.

Well, the cat and the newborn do many other things to affirm and maintain the relationship so that in times of distress cries of distress do indeed elicit the other's efforts at figuring out how to assuage it. And that was the topic.

>The PCT perspective is that language is just a means to
>nonlinguistic ends. Communication has taken place when one person
>is able to create in a second person some intended experience by
>means of spoken or written words. If the words result in the
>desired message within the sender then the words have a good
>chance of doing the same within the receiver. The sender will
>control the selection of meanings and thereby control the message
>perceived within him-/herself.

We are in agreement on this, although I have qualms about equating "intended experience" with "message" just because (a) the latter term has linguistic connotations and the former is predominantly though not necessarily exclusively nonverbal and (b) the latter term is an information-theoretic buzzword that has no application here.

>The communication orientation in Diver's approach results in the
>linguist looking for signals and associated meanings (nonverbal
>perceptions). The message is a perception derived from the
>linguistic meanings and nonlinguistic meanings. In PCT terms, I

>think that the signal/meaning relationships may involve signals
>involving sequence level perceptions. This means that the message
>involves program, principle and system level perceptions.

We are in agreement on this as a general outline of a theory of language use. Operator grammar is a theory of that which is used. What has been missing is the connection to meanings and to language use, and that is why I have got so excited about control theory. I am wide open to learning what Diver has to say about language use. I had reservations about the glimpse you gave us of his theory of that which is used (the decision tree for English definite and indefinite articles, etc.). I will follow up the Diver reference as soon as able when you provide it. Joel has also recommended McWhinney.

Bill I think would want to say that there is only the perceptual control of language use, and there is no "that which is used" i.e. structure of language. We will probably continue to debate this as I learn more about perceptual control and as Bill learns more about language.

We have a hurricane coming. Everybody's leaving. Later.

Bruce Nevin
bn@bbn.com

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Date: Mon, 19 Aug 1991 15:19:12 -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: Post-meeting hello

[From Rick Marken (910819)]

Just want to be the first to welcome everyone back to CSGNet after the CSG meeting in Durango. Great meeting. Great place. Great times. It was nice of Bruce Nevin to keep the net going (nearly single-handedly) while we were away. I guess we got back just in time to see some real control problems in the old USSR. Ed - nota bene - the perils of leadership.

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)

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Date: Tue, 20 Aug 1991 07:54:43 -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: Aftermath

[From Bill Powers (910820)]

My mind is a blank. I think I sprang a leak at the CSG meeting and everything drained out. It will take me a while to think about discussing control theory without a little shudder of revulsion.

In the meantime, it occurred to me that the Meadians and the linguists have something to discuss. What is an instruction, that a person can self-instruct? Who generates the instruction (and how), and who receives it and translates it into a specific controlled variable (and how)? By "who" I mean any of the whos at any level inside a person.

Thank you all for messages of appreciation for the CSG meeting. I didn't do much about setting it up: Tom Bourbon and Mary took care of the endless worrying about logistics and creature comforts. As befits a meeting of PC theorists and practitioners, the organization, content, and conduct of the sessions came into being out of mutual support, courtesy, interest in the thoughts of others, and (dare I say this of a scientific meeting?) love.

To those who attended a CSG meeting for the first time: I can't imagine how we've been getting along without you. I trust we won't have to do so from now on. See you in Durango next year (July 29, 1992).

Best to all

Bill P.

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Date: Tue, 20 Aug 1991 11:07:21 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: linguists, Meadians, and instructions

[From: Bruce Nevin (910820 1106)]

Just back from a meeting. Want to interject that two Meadians very kindly paid me a visit before the CSG meeting, Chuck Tucker and Clark McPhail, here in Cambridge for Clark's daughter's wedding. So we have begun some dialog, I have read a couple of papers on the crowd demo and have seen it perform. Also got a chance to mouse around with some tracking demos. We had a marvellous morning's conversation which I very much enjoyed and appreciated.

Maybe Clark or Chuck or someone else who was at Durango can clarify Bill's reference to instructions. Guessing at it, I would say I think of it in terms of our telling ourselves stories. Stories often have structure that the experiences to which they correspond do not or need not have, and we use the former to make sense of the latter. By making sense of experience we instruct ourselves as to next actions instead of being at a loss and reorganizing at a lower level. By reformulating a story that doesn't fit or doesn't work we reorganize at a higher level. The stories we tell ourselves depend heavily on analogy and metaphor,

and are of course profusely illustrated. . . .

Bateson tells something that was a favorite of his friend McCulloch's, as I recall. Someone builds an extremely capable computer and programs it to dialogue in English.

Scientist: Do you compute that a machine will ever be designed that will think as humans do?

<blinking lights, whirring tapes--this dates from the 1940s or '50s>

Computer: That reminds me of a story . . .

On a different tack: has anyone done any model building using FORTH? It seems to me an ideal real-time programming environment, and is very compact and fast. I haven't stolen the time to become a proficient programmer, but it's on my wish list.

Bruce Nevin
bn@bbn.com

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Date: Tue, 20 Aug 1991 13:07:55 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: From the pres

from Ed Ford (910820.1315)

To all:

As the newly elected president of the Control Systems Group, I'd like to share some of the decisions that went on at our annual meeting at Durango. First, the next conference will be held at Durango from Wednesday, July 29th through Sunday, Aug. 2nd. Details will be posted in the spring of 1992.

Chuck Tucker was elected vice president (which means he automatically becomes president next year).

The annual dues of the CSG have been raised to $40 per member, except for students, who will continue to pay $5. The reason for the increase in dues is that it will not only pay for the Newsletter, but also for the Closed Loop (the digest of CSGnet discussions) edited and published by Greg Williams. Only paid members receive a copy of the Closed Loop. The last issue of Closed Loop (#3) comprised 36 pages and Greg said that he had enough material for about 36 more. So, if you want to receive future issues of Closed Loop (Greg presently plans four issues per year), send your dues (our fiscal year begins with our annual meeting) of $40 payable to Control Systems Group and mail to: Mary Powers, 73 Ridge Road, CR 510, Durango, CO 81301. Those who now receive the newsletter and have not paid their dues will eventually be dropped from our mailing list.

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

Date: Tue, 20 Aug 1991 16:32:06 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: limits of grammar

[Martin Taylor 910820 16:10]
(Bruce Nevin 910805 7:03)

Sorry for the delayed reply--I've been away for 2 weeks.

>
>If you distinguish between a description that accounts for the
>informational capacity of language from one that accounts for its
>communicative uses, the former is what I mean by grammar and the
>latter is I think what you are objecting that a grammar such as
>I describe cannot include. As to whether one has been created or
>not in the past 2000 years, please do look at references cited
>for operator grammar.

I take a grammar to be a procedure for determining whether a string belongs "correctly" to a language, or alternatively, a procedure for generating all strings that do belong to a language.

My claim is that in the 2000 years or so that people have been attempting to describe what people do when they string words together, the rules have become ever more complex, describe ever more of language behaviour, but in no way delimit the "correct" description of any natural language. (Even most grammars for artificial languages such as programming languages often turn out to have little ambiguous hidey-holes in them).

I do not believe that a grammar based on rules can exist for a natural language, though one based on some concepts of fuzziness and/or probability might be possible. I do believe that a grammar based on rules can, like Ptolomeic epicycles, come arbitrarily close to describing what people do with strings of words. I do not believe that either approach will tell us much about how people do what they do when they use language.

To me, grammar is a branch of mathematics. It is fun, and properly used, it can tell us a lot. But my interest is in psychology, so I want to go a little deeper than a surface description. Just as Newton's mathematical descriptions tell us pretty well where a ball will go, but don't tell us why, so "grammatical" descriptions of language can be (to date only) moderately good at telling us what words reasonably go together, but don't tell us what is going on when they are put together.

I am, to quote you, interested in the informational capacity of the language.

Martin Taylor

Date: Tue, 20 Aug 1991 17:03:13 EDT

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Martin Taylor <mmt@DRETOR.DCIEM.DND.CA>
Subject: Re: followup to Martin

[Martin Taylor 910820 16:45]
(Bruce Nevin910805 0803)

>
>My first reply was too quick. I think we agree that grammar (the
>skeleton of language, if you will) does not account for communication
>and other uses of language.

My response was also too quick, sent before reading your long response.
I think we agree a lot more closely than our words would lead others
to believe.

My working assumption is that all of the rigmarole of
>a minimal structure of operators, arguments, and
>dependencies, and their definition in terms of the dependency
>requirement of other words (which makes a mathematical treatment
>possible)
if they provide a good description, are a byproduct of
what is actually going on--the informationally efficient communication
of intention from one person to another. In the CSG context, I would
rephrase that as "a byproduct of what is actually going on--the maximization
of the stability of the two interlinked hierarchic control systems."
I think the two statements are the same.

What is inherent is, to me, not the operators etc., but the requirements
that say that these operators are an effective way of satisfying the
efficiency and stability requirements.

Traffic laws are like prescriptive grammars we are taught in school. They
codify behaviour that usually will result in no harm if followed, but do
not help in effective behaviour in all circumstances, especially when
confronted with someone breaking the rules. Analogy structures, perhaps
coupled with "logical" thought, help at those times, in language as in
traffic. "What could that mean" is analogous to "How do I avoid this
imminent accident." It's not really very analogous to "can I ride my
bike to the store instead of the school?" The former analogy deals with
the "flesh" of the behaviour whose skeleton is articulated by the rules,
whereas the latter deals with the behaviour of the flesh whose approximate
structure is determined by the skeleton.

Martin Taylor
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Date: Tue, 20 Aug 1991 17:48:11 EDT
From: Martin Taylor <mmt@DRETOR.DCIEM.DND.CA>
Subject: Re: Social rules

[Martin Taylor 910820 17:40]
(Bill Powers 910805 0900)

> There are no natural control systems outside the

>individual human being. Not even in a society.

I do not think the rationale and examples Bill uses to back up this statement hold water. The effects are as he describes them, but to go from that to the above is an ideological, not a logical move. What is a control system but something that responds to an applied reference signal by reducing the mismatch between its perception and that reference? What is Bill describing but the insertion of a reference signal by society into an individual? Does the same not apply to cooperating groups, which attempt to reduce error with respect to externally (or internally?) supplied reference signals) and which supply appropriate reference signals to the members of the group so as to achieve that error reduction?

Bill uses changes in social norms, and the fact that there are many communicating people, as arguments against the idea that there are control systems outside the individual. But control systems at all levels of the hierarchy are subject to changing reference signals. If they were not, why would there be any need for a control system? And did Bill not, even in his earliest work, acknowledge and deal with the proposition that the reference signals for most of the control systems come from many sources? Why is it different when those sources are outside the single human?

Surely there is a sense in which social norms and reference signals change in an uncontrolled way. That follows from the concept of evolution as the genesis of enhanced stability through the production of control systems. At the "top" level, there cannot be control. But there seems no rational reason for asserting that the ultimate level of the hierarchy of control systems must necessarily reside within a bag of skin.

Martin Taylor

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Date: Tue, 20 Aug 1991 18:28:11 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: Language; Shareware

[Martin Taylor 910829 18:20]
(Bill Powers 910806.0900)

> What happens to control systems that are taken
>out of action and then brought back in? Do they retain, somehow, the
>state they were in when turned off, so they can continue?

Anecdotal evidence:

As a kid, I had to learn a particlar Haydn sonata on the piano. I played it over and over, and came to hate it (and Haydn), and after the age of 10 I never went near it...until at the age of about 20 I rediscovered Haydn and played other sonatas. I finally tried the hated one, and I found that I played it, not as I had learned to play Haydn, but exactly as I remembered having played it ten years earlier, broken triplets and all. There seemed to be a specific learning of the sequences of the music, that had not been lost or overwritten by later learning (I could do those patterns fine in

other similar music).

Martin Taylor
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Date: Tue, 20 Aug 1991 18:59:17 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: language problems

[Martin Taylor 910820]
(I think August is 08?)
(Bruce Nevin (910991 1340))

>Is there a distinction between language and the speech or writing by
>which we know about it?
>
>On the face of it, examples like the transcript that Martin Taylor gave
>us would seem to suggest that there is. We carry around within us a
>model or standard of spoken English to which that string of words and
>others that people often produce do not conform. And as Martin pointed
>out, we "normalize" what we hear, reconstruct what we suppose we should
>have heard, and more often than not firmly believe that that is what we
>in fact did hear.

If I said that, I mis-wrote. What I intended to say was not that we normalize the speech, but that we understand it, AND UNDERSTAND IT TO BE NORMAL. If we have to tell another what someone said, we rarely use the same words, but reconstruct something having the same sense. That reconstruction may have a more literary form (I prefer "literary" to "normal", since that kind of form is more applicable to writing than to speech). But this does not mean that we literarize the original when hearing it. We simply understand it, and not as a different form of language that has to be (unconsciously perhaps) corrected.

It is (as Bruce has said elsewhere) difficult to transcribe exactly, simply because we are more used to understanding what people say than worrying about exactly the way they say it (unless we are lawyers or linguists).

Martin Taylor
==========
Date: Tue, 20 Aug 1991 19:24:44 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: linguistics; social control

[Martin Taylor 910820]
(Bill Powers (910810))
>
>
>It's not an oversimplification or even a simplification. It's a
>description. The system is only and exactly whatever the license clerk
>(and the rest) wants to do, no matter what. If you want to understand the

>system, you have to understand what the license clerk (etc.) wants to do,
>and why. There is no other place to look for it and nothing else to
>understand about it. It is not in the sky or in the air or in a hubcap or
>in the water supply (which you can drink or dump garbage in, as you
>choose). The system is people doing or trying to do whatever they want to
>do or want to try to do. This is how people work.
>
You could say almost the same thing about the muscle fibres in your arm.
What's the difference? After reading several of your posts (and Rick's)
on the lack of social control, I still fail utterly to see the difference
(from the viewpoint of the control system itself) between one that
controls for, say, transitions, and one that controls for social
interactions. The reference signals come from outside, and the control
system acts so as to reduce error.

I don't mean "lack of social control" so much as "lack of control outside
the individual." My original phrasing is, I think, accurate, but has
political connotations that I don't intend, if you take it outside the
CSG context.

Martin Taylor
==========
Date: Tue, 20 Aug 1991 19:35:31 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: Language (Is that all there is?)

[Martin Taylor 910820]
(Bill Powers, on David Goldstein (910810))
>
>Your post on Diver's theory of number is interesting: the diagram does
>have the form of a program. And I guess it works, although it doesn't
>seem to make allowance for the option "he put THE peas on the plate" (and
>the carrots somewhere else). Are these rules supposed to work ALL the
>time? Or if differentiation is not required, is it still OK to do? Is the
>meaning then the same whether or not differentiation is used? (I don't
>think "he put peas on the plate" means the same as "he put the peas on
>the plate." The first implies that there are still some left for the
>others; the second seems to mean that those were all of the peas.)
>
Divers diagram is interesting. Divers diagrams are interesting. The Divers
diagram is interesting. The divers diagrams are interesting.

Why can't my Korean-born wife, who studied English as her main topic in
school, is a professional psycholinguist, and has written academically in
English for 30 years--why can't she, I say, yet determine where to put
"the" "a" or null? I think the uses are more diverse than Diver's
diagram suggests. In fact, I was at a seminar a few years ago in which
the talker suggested that no-one had yet developed a satisfactory set
of rules or situation descriptions to indicate when "the" "a" or null
would be appropriate, although native speakers always(?) know.

Martin Taylor
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Date: Tue, 20 Aug 1991 19:59:43 EDT
From: Martin Taylor <mmt@DRETOR.DCIEM.DND.CA>
Subject: Re: Transition/Configuration Inversion

[Martin Taylor 910820 19:50]
(Bill Cunningham 910813 0958)
>
>I feel much more comfortable with the revised order. The previous
>seemed contrived, but I couldn't say why. I peddle HCT where I work
>with varying success. At least three colleagues have challenged the
>previous order with independent, but identical, war stories: When moving
>through jungle under very hostile conditions, all sensory organs are
>wide open. Survival depends on detecting an adversary before being detected.
>These survivers all report correctly perceiving the presence of an enemy
>before anything resembling configuration has emerged. They can't pin it
>down to any particular sensory input, but they report a clear perception
>of a transition (absence of enemy-->presence of enemy). This experience
>was vivid enough for them to remember it clearly 20 years later and to
>find immediate fault with HCT's previous order.
>
Is it totally outside the bounds of CT to suggest that the two orders
might co-exist? Bill's anecdotal evidence may be an example of the fact that
peripheral vision is specialized to detect motion and to attract attention
to moving things, whereas foveal vision is more specialized to detect
detail (configuration, I guess). Is it not reasonable to suppose that
for things viewed peripherally the configuration is built from the
motions (transitions) whereas the reverse is true for things viewed
foveally? Is this heresy?

Or hearsay? I'm not sure whether there would be any auditory analogues
of this, although it seems that the information content of speech is
roughly equally partitioned (independently) between the actual spectra
at any moment (configuration) and the temporal differences between
successive spectra at 10 msec frame rate or thereabouts (transition).

Come to think of it, I'm beginning to enjoy being a heretic. I don't
think Rick is going to like this note, though. Sorry about that. Someone
please bring me back to the faith!

Martin Taylor
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Date: Tue, 20 Aug 1991 20:07:01 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Eric Crump <LCERIC@UMCVMB.BITNET>
Subject: social control

I am not a CT person (save what I have picked up from this list during
the past couple of months), so what may be painfully obvious to most
is you is still slippery for me. For instance:

- - - - - - - - -

>choose). The system is people doing or trying to do whatever they want to
>do or want to try to do. This is how people work.

>
You could say almost the same thing about the muscle fibres in your arm.
What's the difference? After reading several of your posts (and Rick's)
on the lack of social control, I still fail utterly to see the difference
(from the viewpoint of the control system itself) between one that
controls for, say, transitions, and one that controls for social
interactions. The reference signals come from outside, and the control
system acts so as to reduce error.

- - - - - - - - - - - - -

I, too, am failing pretty miserably at understanding how control does
not exceed the individual. I agree that the license clerk (our resident
example) determines, to a great extent, the ease with which someone
can obtain a license. But the clerk's behavior is moderated by
a social system that--it seems to my primitive mind--is an entity
external to the clerk as an individual (although, granted, the
system is also internalized by the clerk, but that is its method
of operation). Left to his own devices, a clerk's possible responses
to a request for a license could range from compliance to defiance,
from landing over the license to throwing the typewriter over the
counter. The social system requires that, at most, the clerk can
express his inner rage by giving customers a cold stare and
sending people to the end of the line if at all possible. Other
individuals made those conventions which restrict individual
behavior, I know, but so many individuals made and abide by those
rules that they have become something else, a collective that is
an organism of sorts itself.

That's why I like the reference to muscle fibers. Each might be
an individual, but their movement is commanded by the rest of the
system--the brain, the nerves, the bones, the organs. If they
followed their individual desires, your body might ask them
to type a note to CSG-L and they might decide to throw your
head through the monitor screen. Who knows?

I think society is like a big clumsy old body, made of five billion
cells. People are individuals, and can act as individuals, but only
do (generally) within the boundaries defined by the system, which
is not more than the sum of its parts; it is different than the
sum of its parts.

I could be wrong.

Eric Crump
lceric@umcvmb

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Date: Wed, 21 Aug 1991 10:07:32 MEZ
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Peter Parzer <A6212DAN@AWIUNI11.BITNET>
Subject: Testing CT Models

From Peter Parzer

There is an aspect of Control Theory that I still do not understand.
CT Models are about the control of perceptions. I can not observe the
perceptions of a person. So how can I verify a CT Model with an experiment ?
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Date: Wed, 21 Aug 1991 07:46:46 -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: Instructions; Social Control Systems

[From Bill Powers (910821)]

Re: Instructions

The Meadians speak of "Instructing yourself to ...." rather than just "doing." The implication is that there is a reference signal cast in words. I have reservations about this concept because comparison requires like entities to be compared. On the other hand, a reference signal cast in words can be matched by a description cast in words (what else?), so that one can get the illusion of carrying out an intention if it is possible to find a description that matches the self-instructions. This can be done without overtly acting at all.

---

re: social control systems

I detect a revolt in the ranks. Splendid. The process of learning p. or h. control theory is one of understanding followed by conflict with older points of view which come to light when the context changes. The phone rings at three in the morning and a voice says "Now just a doggone minute ..."

What comes in through the senses? Not reference signals, but perceptions. Perceptions are reports on the (purported) current state of affairs. They are not prescriptive. Ahah, says the perceptual system, I discern that this car with me in it is headed toward a tree. The reference signals, oblivious to the current situation, say that the car is centered in its lane. The comparators in the brain must take in the reference signal's specification and the perceptual systems' report and make of them an error signal that leads to action that tends to reduce the error. Without reference signals, perceptual signals imply no behavior. If you wish to crash into the tree, you can actively maintain the perceptual signals as they are. The perceptual systems will continue faithfully reporting the current situation until the moment of impact.

Furthermore, what comes into the brain must always begin as a collection of elemental stimuli that excite sensory receptors to produce trains of impulses representing intensity. The rest of the nervous system lives, therefore, in a world comprised of intensity signals. Out of the behaviors of these signals and all the relationships the brain can construct on them, the rest of the world comes. As we gain experience with this world (even in the act of constructing it), we record sufficient of it to be able to select previous states and use them to create reference signals defining intended states. Structures higher in

the brain select and set reference signals for structures lower in the brain, as required for higher control processes old and new.

When you get to the top of this hierarchical structure, you are as far as it is possible to get from the sensory periphery. The highest reference signals can be derived only from recorded states of the highest perceptual signals, or from fixed genetic information, or from the random trial-and-error of reorganization. The only way for any higher entity to insert a reference signal into the comparators at the highest level would be to drill a hole in the skull and stick an electrode through it (or to reach in through the fourth dimension or a theological loophole). It is physically not possible for the environment to adjust reference signals at the highest level. It is therefore not possible for the environment to *determine* reference signals at any lower level.

There is only one way in which a reference signal can depend on an external event. That is for the external event to disturb a variable under control at some level in the hierarchy. When this happens, the corresponding control system at that level will alter the reference signals sent to lower systems in such a way as to counteract the effect of the disturbance on the controlled variable. Those lower-level reference signals will therefore appear to depend on the external event as long as the higher-level reference signal stays constant. However, it is generally not possible for someone in the external world to know just what OTHER controlled variables have been disturbed by the same event, and thus to understand all the adjustments that are being made internally to the brain. We can predict that the disturbance will be counteracted by SOME act of the system, but whenever there is more than one act that would serve (and there usually is), we can't predict WHICH act or combination of acts will be employed. Whatever act is chosen by the brain must satisfy the requirements of many control systems at many levels. Unless you have a complete map of another person's goals at all levels, you can't predict how a given disturbance will be resisted -- unless, like Skinner, you arrange the environment so that only one act can have the requisite effect. Of course all such predictions depend on the constancy of reference signals at levels higher than those involved in counteracting the disturbance.

So my objection to the idea of social control systems has nothing to do with abstract principles or philosophy or activism. It is simply a deduction from the apparent facts of our physical construction, coupled to a model of how the brain manages behavior. Human beings can act on each other only through the exchange of chemicals and physical forces and through altering the patterns of intensity signals at the periphery of the nervous system. They provide each other with experiences, but not with reference signals. I can DESCRIBE a reference condition to you ("Go jump off a cliff") but I can neither interpret the description to you in terms of specific target-experiences nor cause you to accept the meanings in the description as your own active reference signals.

This is, I presume, how all people work -- even those who work for "The System." Each person lives inside one brain. In this brain are that person's perceptions and that person's goals. Some of these perceptions represent the output acts of other people -- but never their perceptions or goals. So each person lives in a purposive system, and is surrounded

by other people known only through their shapes and their acts, and only inferred to be purposive.

From interacting with others, one comes to form concepts of systematic entities, system concepts. Each person does this independently and alone. As a result, the inner organization of each person takes into account the properties of others as they are visible through the acts of others. The concepts thus formed embody theories of human nature, theories about human interaction, concepts of what you're allowed to do and what you're forced to do. These concepts may have nothing to do with real human nature; they may be completely erroneous. Nevertheless, they determine what goals you will pursue in relation to other people and what means you will employ in pursuing them. They also determine the properties that you will exhibit from the standpoint of other people.

The interactions that develop among people organized in this way can be of any conceivable type. There can be negative feedback and positive feedback and open-loop relations. The entire social system can oscillate or run away, or lapse into quiescence. There can be direct physical conflict. There can be loners who shun company. People can develop different customs, languages, means of livelihood, attitudes toward law and religion, definitions of fun, and styles of family living. Anything is possible: there are no overriding rules and there is no overriding entity capable of enforcing any particular style of being.

Each person, of course, has needs and requirements. These must be met, and they play a large part in determining when a person will reorganize and stop reorganizing. Everyone has to eat, breathe, stay warm, play, think, and experience Good. So there are inner forces that are similar in all of us. But these forces are inside, not outside. The constraints they introduce work through reorganization, not though external direction.

The physical world also introduces constraints, but not purposive constraints. It is apparently true that energy and momentum are conserved, and so on. It is true that two bodies can't occupy the same space, at least if they are human. It's true that if there is less food than is required, only some people get to eat enough. And so on. Physics, chemistry, and biology create constraints within which all learning and interaction have to take place. But these constraints exist without purpose and they apply equally to all.

There is and can be no social control system because there is no place for it to exist and no organization external to human beings capable of carrying out its functions. Even people who think they are part of a social entity have different concepts of what it is, what its goals are, what it should be perceiving, and how it should act in specific circumstances. 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. Only he can decide, and that decision comes out of the way he is personally, individually, organized inside.

This is true of every single individual you will ever encounter in the

process of interacting with the social system, no matter how impressive the building in which the individual works or the equipment he or she choose to bring to bear on you. It is true even when people use force on you, even when they gang up on you. What they do comes out of themselves; they are responsible for doing what they do. Just as you are. Just as we all are. People may use a mythical concept of a System as an excuse, as a way of attributing cause elsewhere, as a way of unloading responsibility. But the responsibility for how you move your arms and legs, for the way you move your mouth and face to shape the sounds you utter, how you mobilize yourself for action, is yours and nobody else's. It is your responsibility not for any moral reason but simply because your purposes determine all these things and therefore you, as a whole behaving system, are causing them.

If no individual can correctly blame the external world for the purposes presently being effected by that individual, then there is no System, because the system is manned by individuals (and womanned). It is simply the way they interact in the physical world.

Best to all

Bill P.

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Date: Wed, 21 Aug 1991 10:05:10 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: control from the outside

[From: Bruce Nevin (910821 0703)]

Peter Parzer (Wed, 21 Aug 1991 10:07:32 MEZ)

>There is an aspect of Control Theory that I still do not understand.
>CT Models are about the control of perceptions. I can not observe the
>perceptions of a person. So how can I verify a CT Model with an experiment ?

Let me try an answer, as a test of my comprehension.

The method is The Test for the Controlled Quantity, or The Test for short, described in _Behavior: the Control of Perception_ chapters 16 and 4.

Suppose control system Cn on level n is controlling a summation of neural currents (rates of firing) input from other control systems on level n-1. It is comparing the input rate-of-firing with a reference rate-of-firing and outputting the difference as an error signal. This error signal serves as the reference signal for control systems on level n-1, which in turn control for error relative to it by passing the difference between it and input down to the next-lower level, and so on. At the lowest level, efforts of effector systems such as muscle fibers result in changes in the environment which are perceived by sensory systems on that lowest level, and the loop is closed. That sensory input is passed back up the hierarchy and eventually the result of change in the environment is reflected as a change in input to the

control system Cn on level n.

From the outside, all you can see and experimentally examine is the actions and effects of the effector systems. From these, you infer what the organism is controlling for, an environmental variable Ve. Based on this hypothesis, you introduce a disturbance in the environment that changes Ve and nothing else. If actions of the organism restore Ve to the state prior to disturbance, and continue to do so by actions that change correspondingly as you change Ve, then you can say that Ve is controlled. If not, not. If partially or sporadically, you don't say that control is partial or sporadic, you modify your hypothesis about what is being controlled: your guessed specification of Ve includes something that is controlled, but also one or more things that are not controlled by this organism at this time.

Try the coin game described in Chapter 16 of BCP (Powers 1973) to get a real understanding of this.

Eric Crump (Tue, 20 Aug 1991 20:07:01 CDT)

>choose). The system is people doing or trying to do whatever they want to
>do or want to try to do. This is how people work.
>
>You could say almost the same thing about the muscle fibres in your arm.
>What's the difference? After reading several of your posts (and Rick's)
>on the lack of social control, I still fail utterly to see the difference
>(from the viewpoint of the control system itself) between one that
>controls for, say, transitions, and one that controls for social
>interactions. The reference signals come from outside, and the control
>system acts so as to reduce error.

When a neural control system Cn on level n receives a reference signal from level n+1 and an input signal from level n-1, the comparator in it *must* compare the two and it *must* output the difference as an error signal. The response is not merely compulsory, it is mechanical, it is determined by laws of physics. Just as the far end of a seesaw must come up when you push down on the near end, the control system Cn must output the delta between the input and reference signals. There is no such mechanical linkage between an injunction or command, say, and a person responding to it by obeying, disobeying, ignoring, avoiding, countermanding, etc. The injunction stands as a disturbance, not as a reference signal. As with any other disturbance, the response depends upon reference values currently maintained within the person. The result may even have the appearance of control, as in a military training camp. But this is not control in the sense intended here, it is coercion. Conversely, there is no coercion in the relations of Cn to Cn+1 and Cn-1 within a person.

So far so good in articulating the standard CT view, I think.

Now, what if there were mechanisms for transmission of reference values from some hypothesized interpersonal or transpersonal level? Or what if individuals voluntarily set certain reference signals within themselves to socially-agreed values as their only or best means of controlling certain other perceptions that had higher priority to them? Or what if

together with the tidy neural circuitry postulated in CT there were also untidy, leaky, messy mechanisms such that perceptual control were not so deterministic and mechanically inevitable as we have made out?

As to the third question, BCP (Powers 1973) discusses memory in terms of RNA present at each synapse. It limits discussion to the synapses conducting the reference signal into the comparator from above. What are the implications of the fact that RNA and the mechanisms by which neural firing changes it are present at all points of a control system like Cn at level n. What are the implications of the presence, and pervasiveness, and motility of other neuropeptides in the system?

Candace Pert (chief of brain biochemistry at NIMH) has shown that neuropeptides function as "signal molecules" or "communication molecules" in living organisms. They are found in all living organisms from single-celled organisms to humans. There are 50 or 60 known, made directly from the DNA without an intervening enzyme, configured from a single polypeptide. In vertebrates, they link the nervous system, endocrine system and immune system into a "bi-directional information network" (this means transmitting, receiving, and monitoring and adapting to information flow). They appear to help integrate behavior by regulating mood. Neuropeptide receptors are clustered at primary points for sensory input. "Even before we register our perceptions, they are colored by emotion." (Quotes from 1986 conference in D.C.)

The limbic brain, which is rich in neuropeptides, extends in tentacle fashion into the brainstem and cortex much more than previously thought. In addition, neuropeptides and neuropeptide receptors are strategically located throughout the body. "The emotions are not just in the brain, they're in the body." Even certain hormones, such as insulin, have turned out to be part of the peptide system. One, CCK, affects gut action and forms a lining from the esophagus through the intestines, possibly relevant to the perception of "gut feelings". Receptor sites are highly specific. "An opiate peptide may act miles away from its origin." Placebo analgesia, triggered by suggestion, is an altered state dependent on neuropeptides. Conscious expectation ceases to suffice to maintain the analgesia if experimentors administer naloxone, which block the flow of endorphins. Acupuncture also depends on endorphins, evidently by drawing the substances from other sites. The presence of neuropeptides in the brainstem nuclei may help explain the effect of breathing practices on the alteration of consciousness and pain thresholds. (From: Brain/Mind Bulletin 11.4 1/20/86)

Benjamin Libet (UC SF) has been studying EEG correlates of conscious experience since the early 1960s. He has found that a distinct brainwave pattern which he calls the readiness potential (RP) occurs 350 milliseconds before the subjective experience of wanting to move, and another 150 milliseconds before actual movement. During the latter interval of 150 milliseconds, the movement (quick flexion of wrist or finger) could be vetoed or blocked by the participant.

At the moment they were aware of a conscious decision to act, Libet's participants noted the position of a moving target (accuracy calibrated in another setting). In one experiment, they were asked to note when they actually moved. They reported having moved slightly before any

actual physiological evidence of movement. This was a puzzle to him but seems to me to be a registering of the reference value for movement (transition) prior to conformity of sensation and effort control systems. The question remains: to what does the RP correlate, since conscious volition clearly follows it in time. Does conscious intention result from subconscious sensing of the RP or of whatever gives rise to it, as suggested by John Eccles of Max Planck Institute (in _The Behavioral and Brain Sciences_ (8:529-566) issue containing commentary on Libet's findings). Some other questions raised in that issue: Are participants failing to report an "anticipatory image" (as described by James in 1890) because not instructed to look for it? What of the will to veto an action--is there no RP preceding it? My guess: the volition to move the finger is not keyed to any reference signal set by a higher-order control system, but takes some other bodily state, whose experimentally observed sign is the RF, as an arbitrarily set reference signal. What role might such a default mechanism have in our choosing when to act? Might it have to do with what we perceive as emotion or mood, mediated by neuropeptides?

As to the first question, re mechanisms for social transmission of values of reference signals: what is the CT account of hypnosis? Suggestion often takes place not by setting values of reference signals at highest levels but by people setting values at lower levels in conformity to or congruity with those implicit in actions of the hypnotist. One of Milton Erickson's trance induction techniques involved modification of the routine handshake. In a handshake, people match one another's hand pressure, movement, proximity, probably other factors. Erickson would make this a vague and moving target, perhaps accompanied by other actions to "induce trance". (See Erickson's writings for description of his experimental work over many years.) Ernest Rossi, Erickson's longtime collaborator, talks about "everyday trance" states that people enter periodically during the day. This may be associated with the ~90-minute Basic Rest and Activity Cycle (BRAC), comparable to the REM cycle in sleep. Brain activit shifts hemispheric emphasis, and breathing shifts from one nostril to the other. During the ~10-minute crossover period from left to right hemisphere and parasympathetic dominance one may observe relaxation, pulse movements of arteries, reduced blinking, pupil dialtion, reddening of the eyes, tearing, slowing of respiration, sweating. People can influence shifts of cerebral dominance voluntarily by closing one nostril and breathing through the other (incidentally, and I trust not prejudicially, an ancient technique of pranayama yoga). Rossi lists "64 projects in search of a graduate student" in his contribution to _Handbook of states of consciousness_ (edited by Benjamin Wolman and Montague Ullman, NY: Van Nostrand).

Might reference signals within an organism be seen as "suggestions" from higher-order control systems to lower-order ones? Might neuropeptides mediate "suggestions" among peer control systems? Might people's willingness to "take the attitude of the other" (Meade) for the sake of cooperation and other social benefits account for interpersonal suggestion at the heart of the social order?

Lots of experimental and clinical evidence points to various kinds of communication or entrainment among people, e.g. synchronizing of

menstrual cycles. Some allergic people react to electrical fields surrounding common electrical devices. Symptoms include pain, immobility, rigidity, watering of eyes, confusion, crying, euphoria. While in the allergic state, sensitive people sometimes emit measurable levels of electromagnetic radiation that can affect elecronic equipment and other people (Cyril Smith of U. Salford, England, Ray Choy of Lister Hospital, London, reporting in _Clinical Ecology 4:9-102 1987). One woman went into convulsions when brought within 200 meters of overhead power lines--for example, every time the ambulance went under power lines on the way to the hospital. Different frequencies trigger reactions in different people, and different reactions in the same person. (Ray Choy, Allergy Unit, Lister Hospital, Chelsea Bridge Road, London SW1W 8RH, U.K., reported in Brain/Mind Bulletin 12.10 July 1987.)

When two people are attempting to "feel each other's presence" there is an increase in brainwave synchrony between the two hemispheres of the brain of each person, and between the brains of the two people, this regardless of whether they were acquainted previously or not. A person with less inter-hemispheric synchrony would come to resemble (in brain activity) a partner with more synchrony rather than vice versa. Hemispheric synchrony is associated with unusual states of focus, meditation, and the like. In a darkende Faraday cage, participants were instructed to close their eyes and try to "communicate by becoming aware of the other's presence and to signal [the experimentor] when you feel this has occurred." There was no talking or touching. The findings are taken as confirming Grinberg-Zylerbaum's 1981 theory about interaction of "neuronal fields" (I don't have the title of the book). Reported in _International Journal of Neuroscience 36:41-52. Grinberg-Zylerbaum is at Facultad de Psicologia, Universidad Nacional Autonoma de Mexico, Mexico City 04510 D.F.

I have a dozen other clips of articles that I would like to bring to bear on this, but I haven't time.

I do not think that any of us is in a position to be dogmatic with respect to the status or effect of a Durckheimian "social reality". We can say that hierarchical control theory as currently formulated has no place for social control except as something that individual living control systems create in some statistical or "self-organizing systemic" sense by their individual choices and actions. We cannot say that HCT is complete in its account, and we cannot rule out the likelihood that it will require emendation and perhaps even supplementation by one or more other, complementary models of what is going on in living organisms, such that neither model is adequate without the other. The contribution of control theory seems pretty secure.

Bruce Nevin
bn@bbn.com

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Date: Wed, 21 Aug 1991 08:17:12 -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: Re: Transition/Configuration Inversion

[From Rick Marken (910821)]

Martin Taylor (910820 19:50) says:

>Is it totally outside the bounds of CT to suggest that the two orders
>might co-exist?

No problem.

>Come to think of it, I'm beginning to enjoy being a heretic. I don't
>think Rick is going to like this note, though. Sorry about that. Someone
>please bring me back to the faith!

Just say 100 "hail Bill"'s and go to confession this friday (the control theory
sabbath).

Why do you think I wouldn't like the note Martin? Looks OK to me. Control theory is not a religion. The nature of the perceptual hierarchy -- in terms of the make up of the levels, their order or even whether there are levels at all -- is up for grabs. There has been very little research on this topic. We have found pretty convincing evidence for the existence of hierarchical control -- and we have modelled this behavior very accurately. But the nature of the levels is still a wide open area for RESEARCH (no faith required). The only dogma in control theory is that people are organized as input control systems. It's not really a dogma, of course, because we would be happy to abandon this dogma as soon as there was one bit of convincing evidence that human purposeful behavior (control) is not the result of the operation of a closed loop perceptual control system.

Bill Powers answered the posts regarding the existence of "extra-individual" social control systems very eloquently. I also agree with Bruce Nevin's comments on this topic.

Hasta Luego

Rick M.

****

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

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Date: Thu, 22 Aug 1991 10:01:58 MEZ
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Peter Parzer <A6212DAN@AWIUNI11.BITNET>
Subject: Re: control from the outside
In-Reply-To: Message of Wed,
              21 Aug 1991 10:05:10 EDT from <bnevin@CCB.BBN.COM>

From Peter Parzer (910822)

On Wed, 21 Aug 1991 10:05:10 EDT Bruce E. Nevin said:

>From the outside, all you can see and experimentally examine is the
>actions and effects of the effector systems. From these, you infer what
>the organism is controlling for, an environmental variable Ve. Based on
>this hypothesis, you introduce a disturbance in the environment that
>changes Ve and nothing else. If actions of the organism restore Ve to
>the state prior to disturbance, and continue to do so by actions that
>change correspondingly as you change Ve, then you can say that Ve is
>controlled. If not, not. If partially or sporadically, you don't say
>that control is partial or sporadic, you modify your hypothesis about
>what is being controlled: your guessed specification of Ve includes
>something that is controlled, but also one or more things that are not
>controlled by this organism at this time.

What I can observe in an experiment is the control of an environmental variable Ve. What I can NOT observe is the control of a perception. So why there is so much emphasis on the control of perception in CT ? Of course, if an environmental variable is controlled than it is plausible to assume that it has to be perceived. But I still do not understand why the Theory talks about the control of perception if the models that I can test empirically are about the control of environmental variables.

There is another problem. Can I really determine experimentally if an environmental variable is controlled ? Assume I introduce a disturbance to Ve and the behavior of the subject does not bring it back to the original value. Now I can say, that the subject does not control that variable. This assumes that the reference value stayed constant. But I can equally say that the subject does control the variable, assuming that the reference value changed. It seems to me that I have to make additional assumptions about the reference value in order to determine if a variable is controlled or not, assumptions that I can not check experimentally since I can not observe the reference values of a subject.

Peter Parzer
a6212dan@awiuni11.bitnet

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Date: Thu, 22 Aug 1991 12:20:18 +0200
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Oded Maler <Oded.Maler@IRISA.FR>
Subject: Re: Instructions; Social Control Systems

(From Oded Maler 22.8.91)

(concerning Bill's rejection of "social control")

Sorry, but I'm not convinced. Once I suggested that you limit the scope of your theory to creatures having a nervous system, but you insisted that this theory can work on finer granularity and explain the cell, although it

is clear that the notions of behavior, signals, "perception" etc., within the cell use quite
different principles. There is no reason I see, to refuse viewing society as a large organism with all that control stuff. True, the fact that it is realized by a collection complex, lossely-coupled, big-brained, language-speaking individuals may imply much more complex dynamics, and different ways of analysis, but *in principle* it is not different than seeing "control" in a floating soup of proteins inside the cell, or in a collection of neurons. These are just different time-scales, different distances and different topologies of information channels.
Somehow the rejection of the idea of social control seems more based on sentimental/ideological grounds, as if it implies some form of Fascism or Bolshevism (which it doesn't).

--Oded

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Date: Thu, 22 Aug 1991 08:04:36 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: variables and their perception

[From: Bruce Nevin (910822 0703)]

An important way of learning for me and I think generally is by varying the value of a reference signal "to see what happens." When I first encountered Bill's demo programs and Clark selected the one for "shape control" my first action was to move the mouse around to find out what was being controlled. I then isolated something like "keep the intersection at the midpoint between the projecting ends of the lines" so that I was controlling not for shape but for something very much like the vertical movement of the bar in the previous demo. This redefinition of the problem followed very quickly from a few circular movements with the mouse, to which the figure on the screen responded with vertical changes to the relationships among those three points. In this case, learning wasn't so much reorganization as initial organization. And it was very fast. Something about the importance of first impressions comes to mind, and the observation that when we are reorganizing (or organizing) we are most amenable to suggestion. I suspect that good hypnotic subjects are more easily put into reorganization than those who are more difficult to hypnotize. Interesting to think of ways to test that.

Peter Parzer (910822)

I am testing my control of CT concepts by pretending to be a spokesman though I am merely a particularly brazen newcomer. I here ask earnestly for correction if misconceptions of mine are giving you a misleading impression. Nonetheless I will persist in trying to control the concepts appropriately.

>What I can observe in an experiment is the control of an environmental
>variable Ve. What I can NOT observe is the control of a perception.

All you can possibly know about Ve is your perceptions of Ve. Talk

about Ve is shorthand for your perceptions. From your perceptions, you try to infer not Ve as Ding an sich (the thing in itself, apart from any perceptions of it--Kant) but the experimental participant's perceptions of Ve. By changing Ve as you perceive it, you assume of course that you are changing it as the other perceives it (but that may be worth verifying). If the other does not resist your changes but lets them stand, then the other is not controlling any perceptions affected by those changes. If the other does resist your changes so that some of the changes from her point of view are undone as they are occurring, then the other is controlling for those particular perceptions. Your disturbances must be slow enough for the other to exercise effective control.

I suggest reading about the coin experiment or the rubber band experiment in Chapter 16 of BCP and actually carrying them out with another person, both with you as experimenter and with you as "subject". If you don't have access to BCE I or another can describe these experiments.

Bruce Nevin
bn@bbn.com

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Date: Thu, 22 Aug 1991 09:27:36 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: responses to Martin

[From: Bruce Nevin (910821 1358, 9109822 0743)]

Martin Taylor 910820 16:10

Yes, I think there is a lot of congruity between our views, time will tell how much is illusory, due to ambiguity of words.

>I take a grammar to be a procedure for determining whether a string
>belongs "correctly" to a language, or alternatively, a procedure for
>generating all strings that do belong to a language.

I believe our difference is in what we take to be "a language". Most linguists take it to be the model to which the language user conforms in using language, and to which she firmly believes her language-using behavior conforms, being generally surprised (at least) to experience evidence of the discrepancies of actual performance. I believe you are taking the behavioral outputs of language use to be "a language". The difference is close to Chomsky's distinction between linguistic competence and linguistic performance. To these Dell Hymes almost 30 years ago added the notions of communicative competence and performance. For the sake of getting a drink of water it does not matter what detailed actions I perform so long as they meet the succesive goals of the program (pick up container, go to water cooler on 7th floor, fill container, drink, return with container to office). For the sake of reaching agreement about certain focalized perceptions it does not matter what detailed linguistic actions I perform and you perform so long as the goal of agreement is reached. (Of course the goal--drink of

water, agreement--may be modified, supplanted, deferred, etc. in the process.) If I perceive or assume (imagination connection) that you are "with me" I may leave out supporting explanations, steps of argumentation, etc., as well as repeated words, and I may abandon the current sentence unfinished and skip to the next one, and so on, producing all sorts of syntactic irregularity.

[Some of this is conventionalized in the social institution of language (you don't say the subject of the verb under "want" when it is the same as the subject of "want" itself, as in "John wants John to go"). Whether conventionally "obligatory" or not, the principle of omitting high-likelihood words is pervasive.]

The preceding paragraph is omissible because we have already reached agreement on the points it makes. However, for you these agreements concern the relevance of information theory, and for me they signal its irrelevance, since nothing of information theory except the omissibility of redundant words is pertinent. Over to you on that one.

> descriptions of language can be
> moderately good at telling us what words reasonably go together, but
> they don't tell us what is going on when they are put together.

No, you need CT for that. But CT needs descriptions of language in order to specify the socially-set reference values involved in people's use of language.

>if they provide a good description, are a byproduct of
>what is actually going on--the informationally efficient communication
>of intention from one person to another. In the CSG context, I would
>rephrase that as "a byproduct of what is actually going on--the maximization
>of the stability of the two interlinked hierarchic control systems."
>I think the two statements are the same.

My concern with this is that it sounds like a process in each individual in isolation and a process of agreements negotiated by pairs of such individuals. Crucially, there is a vast body of agreements embodied in language that people learn essentially without question for the sake of participating in ongoing communication processes with people as they are learning. Your discussion above omits the institutional dimension of language as a system of social conventions.

>What is inherent is, to me, not the operators etc., but the requirements
>that say that these operators are an effective way of satisfying the
>efficiency and stability requirements.

The informational requirements are one sort of constraint on what languages can be like. These constraints are not arbitrary, although one of them is the requirement that the elements of a given language (phonemes, morphemes) are socially pre-set and arbitrary. When a child learns a language from those who are using it, the actual state of the language as an evolving institution imposes arbitrary constraints satisfying this informational constraint--the social definition of phonemes and morphemes--and other constraints as to which reductions are obligatory, where reductions are almost general but cannot be applied in

certain circumstances, etc. An example of the former is the vexed question of gender with pronouns in English. An example of the latter: in English we can say something is more A than something else (where A is an adjective), but we cannot reduce any "more A than" to the comparative "A-er than" for every A, although my children and their peers say e.g. "that's funner" quite freely. (The problem here seems to be that "fun" is historically a noun, but in appositional use--"that's a fun game"--has come more and more to seem adjectival.) Language changes as its users arrive at different agreements about these conventions incidentally in the course of controlling for perceptions that I have just talked about in terms of constraints on what a language can be like.

Here I think I find myself proposing an agreement with Bill, that the structure of language, while fascinating, is ancillary to the study of the control of perceptions that gives rise to it. This can perhaps give better focus to my view that important among those controlled perceptions are perceptions of social relations such as membership and relative status.

>What I intended to say was not that we
>normalize the speech, but that we understand it, AND UNDERSTAND IT TO BE
>NORMAL.
>We simply understand it, and not as a different form of
>language that has to be (unconsciously perhaps) corrected.

I say again that we do not notice ways in which our linguistic outputs differ from our internal model of the language and are surprised by them when confronted with evidence. Our control of language outputs and inputs is oriented to this model, but also (and almost always with more urgency--higher gain) to perceptions of interpersonal relationship and the perceptions to which the particular words and constructions at hand refer (your "message" concept). We turn up the gain on explicitness and syntactic correctness when normal interpersonal feedback processes do not suffice to assure us of successful control of these two sorts of peceptions of communication. Writing is one example of this, and this in my opinion is the reason for the differences between literary and spoken styles.

>English for 30 years--why can't she, I say, yet determine where to put
>"the" "a" or null? I think the uses are more diverse than Diver's
>diagram suggests. In fact, I was at a seminar a few years ago in which
>the talker suggested that no-one had yet developed a satisfactory set
>of rules or situation descriptions to indicate when "the" "a" or null
>would be appropriate, although native speakers always(?) know.

The "a" is the default with count nouns, present unless zeroed under describable conditions. Many languages lack the definite article, and in many of those that have it (in particular in the Indo-European languages) it is a relatively recent development that has progressed in very different ways among them. It is a good example of the interaction of informational motivations (which Diver's decision tree attempts to capture) and arbitrary social convention. In operator grammar, it is a reduction from the indefinite noun "that" in appositional construction "that which is a N". I think I described this previously. The definite

article has much more restricted distribution in the Slavic languages. A favorite anecdote about Henry Hiz, Polish logician and Harris's colleague for many years: In a graduate seminar on transformational analysis, he told the students "In English, noun always takes definite article." These generalizations are terribly hard to get right, and getting them right is not at all the same as controlling them.

Bruce Nevin
bn@bbn.com

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Date: Thu, 22 Aug 1991 10:21:54 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Comments: Converted from PROFS to RFC822 format by PUMP V2.2X
From: "Bill CUNNINGHAM - ATCD-GI (804" <CUNNINGB%MON1@LEAV-EMH.ARMY.MIL>
Subject: auditory analogue

Bill Cunningham 910822.0950
(Martin Taylor 910820.1950)

Martin Taylor asks for an auditory analogue his peripheral/fovial vision description. The "cocktail party effect" is used to describe the nature of aircraft communications with many fighter pilots using the same net frequency. Many voices can be heard. Only two are important: wingman and ground controller. The radio automatic gain control adjusts sensitivity to the strongest signal which is usually either the wingman (closeby) or the controller (powerful transmitter). Thus, two voices stand out from the babble;
but the babble is never lost--providing an overall context. This principle was learned early in the game and the name (originating long before HCT was proposed) suggests a human automatic control) for at least intensity and probably other levels--although these were not available to whoever coined the phrase.

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Date: Thu, 22 Aug 1991 10:30:44 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: phonetic control experiments

[From: Bruce Nevin (910822 1001)]

Discussion a while back convinced me that acoustic perceptions and articulatory perceptions are both redundantly involved in controlling speech, that acoustic perceptions are easier to "decouple" for purposes of injecting disturbances and testing for control, and that (hopefully this is not wishful thinking) acoustic perceptions are controlled with higher gain than articulatory perceptions.

I had some thoughts on the train yesterday about experimental apparatus. I was wondering if one could distort speech in real time in such a way as to shift vowel formants and change perceived vowel quality. If so, one could send the variably distorted signal in real time to headphones of the participant and record that on one tape (or track) while

simultaneously recording the participant's undistorted output on another.

Perhaps the equipment could filter the range for F2 (2nd formant) of the front unrounded vowels i and e (roughly 2000-2600 Hz) and F3 (roughly 2800-3600 Hz for i and 2650-3400 Hz for e), and shift them. I can imagine possibly doing this by re-injecting sound 180 degrees out of phase at one end of the frequency range for a formant (cancellation) while injecting sound at a proportionally large extension of the range at the other end (extension). By varying the amount of cancellation and extension one would shift the frequency of that formant. Engineers among us: could this be done in real time? F1 shifts also, but not as much. I'll talk to some folks in the speech synthesis group here at BBN as well.

I am supposing that the speech output of the participant would become distorted (tape 2) in ways that exactly countered the disturbance: changing vowel quality so as to lower formants being raised, etc., and that the participant might even be unaware of this assuming the dialogue with the experimenter or other participant were engaging enough and the purpose of the experiment were not known.

For a portable experiment similar to Gary's of anchoring tongue tip to alveolar ridge behind the teeth: try speaking as a ventriloquist does, without lip movement. The compensatory articulations to get the acoustic effect close enough are most interesting. "The flowing of the river" comes out something like "the hlouing ozhs the rizher."

Bruce Nevin
bn@bbn.com

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Date: Thu, 22 Aug 1991 10:05:54 -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: LANGUAGE and language

[from Joel Judd]

Peter Parzer,

I might also add reference to Philip Runkel's 1990 book _Casting nets and testing specimens_. Even if it doesn't convince you to seriously consider CT research methodology, it should make you question how most of the alternatives have been used to date. Pp. 117-119 describe The Test Bruce mentioned, in a format you might find easier to understand. Be careful though about worrying about control of "environmental variables;" we control our perceptions of the environment. Determining what we are controlling FOR is what makes The Test at once simple and difficult. Your last point concerning the change of reference level seemed to be right on the mark--that's the risk one takes when dealing with human beings. After explaining The Test, Runkel points out,"Still, you never know when the person might reorganize the internal standards. If that happens, then you must start over" (p.118). The reason why the joy stick demos work is that the subject is willing to accept an internal standard the experimenter offers. That makes the whole process "easy," and the Test fairly clear.

It's when YOU must figure out the controlled variable things get interesting.

Bruce and Martin (910816; 21),

I was feeling bad because the local mail server's been screwed up this week; I wanted to reply earlier. But since the same topic came up again yesterday, I'll chime in.

Earlier Bruce asked
>I'm not sure what you mean by "language" in quotes here.
and then in the reply to Martin yesterday again discusses language use (linguistic performance) and social language purposes (where actual linguistic performance is somewhat irrelevant).

My reason for putting language in quotes before was that I see all (or most) of the distinctions made over the years as falling into different levels of the hierarchy. For myself, I have been getting into the habit of shelving 'language' into the SYSTEMS level. Doing so makes it difficult to talk about, as it is difficult to speak of any of our systems concepts. But it avoids problems such as equating language with grammar, or language with syntax, or whatever. At the systems level, language becomes an intimate part of our being, our self. It is part of what makes me ME, and how I intereact with and judge other people, other things, as well as myself. A given situation will see me instantiate this concept through principles, programs, configurations, etc. But I think there are enough labels for performance at other levels (eg. discourse turn-taking, grammar, allophony) to allow distinctions to be made. Such a view explains WHY
>the structure of language...is ancillary to the study of the control of perceptions that gives rise to >it
as Bruce mentions. The constant confusion among ideas about sources of language use and variability--social factors, psychological factors, linguistic factors...gets me dizzy.

A question. Bruce says

>I say again that we do not notice ways in which our linguistic outputs differ from our internal >model of the language and are surprised by them when confronted with evidence.

Is self correction not a part of this statement? That is, when I use a feminine article with the Spanish word problema, and then correct myself by using the masculine, am I not comparing what my model of Spanish is with the output? Or would you say I don't yet have the "model," or what? This kind of evidence is of course what Stephen Krashen nicely packaged in the "Monitor Model."
Joel Judd
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Date: Thu, 22 Aug 1991 11:21:33 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: phonetic control experiments

[Martin Taylor 910822 11:06]

(Bruce Nevin 910822 1001)
>I was wondering if one could distort speech in real time in such a way
>as to shift vowel formants and change perceived vowel quality. If so,
>one could send the variably distorted signal in real time to headphones
>of the participant and record that on one tape (or track) while
>simultaneously recording the participant's undistorted output on another.
>
>Perhaps the equipment could filter the range for F2 (2nd formant) of the
>front unrounded vowels i and e (roughly 2000-2600 Hz) and F3 (roughly
>2800-3600 Hz for i and 2650-3400 Hz for e), and shift them. I can
>imagine possibly doing this by re-injecting sound 180 degrees out of
>phase at one end of the frequency range for a formant (cancellation)
>while injecting sound at a proportionally large extension of the range
>at the other end (extension). By varying the amount of cancellation and
>extension one would shift the frequency of that formant. Engineers
>among us: could this be done in real time? F1 shifts also, but not as
>much. I'll talk to some folks in the speech synthesis group here at BBN
>as well.
>

Real-time has a very special connotation in CT. There is a delay period for one's own speech heard over earphones that leads to almost uncontrollable stuttering (though one can practice to overcome it). That period is in the neighbourhood of 200 msec. I would be surprised if lesser delays did not have lesser effects which have not been noted in the literature because no-one has looked for them. They would correspond to the loop delays of various perceptual control phenomena. So one would have to do the experiments with careful controls to differentiate the effects of the distortions from those of the delays inevitably introduced by (even analogue) processing. And the evaluation of speech quality is a notoriously treacherous exercise, in itself.

One could, I think, monitor control of the perceived formants in the manner Rick describes for the Mind Reader experiment (hope I remembered the name right--I think I didn't, but you know which I mean). One could resynthesize the speech using shifted formants and see whether the speaker controls to put the shifted formant back where the unprocessed one would have been. I have a feeling that a speaker might not be able to do this, because such shifts (along with fundamental frequency) are what happen in the helium speech of deep divers. The talker hears the Mickey Mouse voice, but does not (can not?) control it back to a normal sounding voice. But perhaps in helium speech the error is outside a control range. The experiment could be done with breathing other inert gas mixtures, perhaps. Maybe we could use a helium-krypton mixture to get a gas of any desired density near that of normal air (the inert gas replaces the atmospheric nitrogen, to clarify things for people who have not been associated with deep diving. It prevents people from getting the bends so easily on depressurization).

A side advantage of doing the experiment by changing the density of the breathing gas is that all other factors surrounding the speech would be unchanged, in contrast to the situation in which one has to use earphones and hears both the internally conducted sound and the (delayed) distorted speech.

Comments on Bruce's note on language later, when I have time to do it seriously.

Martin Taylor

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Date: Thu, 22 Aug 1991 11:14:00 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "HRLOT1::HANCOCK" <hancock%hrlot1.decnet@HQHSD.BROOKS.AF.MIL>
Subject: TIME (JOE)

[FROM TOM HANCOCK (910822 0915)]
[TO JOE LUBIN]

PER OUR DURANGO CONVERSATION, COULD YOU SEND ME THE 'TIME' REFERENCES. MY PARTICULAR INTEREST WOULD BE WITH TIMES ASSOCIATED WITH DEGREES OF RESONANCE.

IT SEEMS TO ME THAT A SYSTEM (A STUDENT FOR EXAMPLE) CONTROLLING FOR MEANING WOULD BE SENSING SOMETHING RELATED TO DEGREES RESONANCE. WHAT DO YOU THINK?

TOM HANCOCK
3300 WEST CAMELBACK RD.
GRAND CANYON UNIVERSITY
PHOENIX, AZ 85017

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Date: Thu, 22 Aug 1991 12:55:08 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: yes, and yes

[From: Bruce Nevin (910822 1257)]

Joel,

Yes, you're right, I should have said we often fail to notice many of our deviations from our internal model of language, and I should have linked that explicitly to a range of variation in the gain on control systems controlling for conformity to that model, as mentioned. Thanks for picking that up, and may your mailer be healthy!

Martin,

Yes, I like the gas mixture notion a lot, though it might be more expensive to set up and is surely less portable. Unfortunately, it shifts everything up or down in frequency, and it is the relationships among formants that define vowel quality. Both disturbance and control in the headphone method could be precisely measured by formant frequency. But I don't know any way around the time-lag problem. The filtering and reinjection could only increase the delay.

The scheme I sketched would also affect corresponding frequency bands of burst associated with consonant release, noise of sibilants and fricatives, and so on, but probably not noticeably since the dominant part of these is above F2.

Bruce

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Date: Thu, 22 Aug 1991 13:30:27 EDT
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Workload assessment

[Martin Taylor 910822 13:30]

One of the interests in this laboratory is the assessment of workload in a stressful environment such as a cockpit. Much of the workload is "perceptual". I use quotes because I am using the term in its non-CSG sense to mean the abstraction of the state of the environment from a wealth of external data, and not to mean the input to control systems. Some of the workload is in "behaviour" in response to the perceptual situation assessment, but it is thought that in the interesting cases the decision-making rather than the resulting behaviour is the important factor.

It seems to me that turning this around, and looking at it from the viewpoint of HCT, might be profitable. An elemental control system at any level of the hierarchy presumably consumes some "mental resources" in performing its function. It takes some "work" to transform the incoming data into a form appropriate for it to compare to its reference signal, and it takes some work to generate from the error signal new reference signals that can be used by lower control systems.

If it is appropriate to think of workload in terms of resources used by multiple control systems, some parallel with others in a level, some controlling others by providing reference signals, then how might one conceive a measure of instantaneous workload? How might one integrate workload over time, and compare it with some regenerated source of effort, if such exists (it must, if only in the form of food converted into ATP etc.)? What should be the effects of task demands that exceed the avaialble resources? Could we tell if some control systems de-activated while others reduced the precision of their control? Could we detect changes in gain or bandwidth of any individual control system?

Some workers in the area of workload identify the concept with "situational awareness", but the analysis of subjective scales of judgment relating to awareness and to work stress indicate that they are not quite the same. I think HCT may offer a way to conceive and analyze the problem in a much more fruitful way, but I do not see how to go about doing it.

Does the group, and especially Bill P., have any ideas?

Martin Taylor

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Date: Thu, 22 Aug 1991 13:46:44 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: yes, and yes

[Martin Taylor 910822 13:35]
(Bruce Nevin 910822 1257)
>
>
>Yes, I like the gas mixture notion a lot, though it might be more
>expensive to set up and is surely less portable. Unfortunately,
>it shifts everything up or down in frequency, and it is the
>relationships among formants that define vowel quality.

Actually, if it were as simple as shifting everything up or down in frequency, correction would be easy. Unfortunately for the diving community, it's more complex. The pitch of the fundamental is determined by the masses and forces of and affecting the vocal cords, whereas the resonances that are the formants are affected mainly by the speed of sound in the gas and the relative impedances of the cavity walls and the gas (slightly). The bandwidths of the formant resonances (especially the first) are not very different from the separation of the harmonics of the fundamental, and when you shift the relation between the fundamental frequency and the formant frequency, the harmonics are placed differently on the resonance curve. Automatic correction is difficult. I grant that the gas mixture idea allows only a one-dimensional environmental disturbance, whereas the electronic distortion gives you as many dimensions as you want, but that shouldn't affect the experiment in principle.

One of the most successful techniques of changing apparent voice quality was developed by Dr Melvyn Hunt at the National Research Council in Ottawa. Anyone who has heard Melvyn as the Berlin Philharmonic Orchestra "saying" "We were away a year ago" as the opening bars of Beethoven's Fifth Symphony will have an idea as to what can be accomplished in this regard. It's eerie.

Martin Taylor
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Date: Thu, 22 Aug 1991 13:13:00 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "HRLOT1::HANCOCK" <hancock%hrlot1.decnet@HQHSD.BROOKS.AF.MIL>
Subject: LEARNING CT--MODELLING

[From tom hancock (9108.22b)
Bill powers (9108.04a)

It didnt work out for us to chat in durango along the lines of
My 9108.03a communique, as i had intended.
So i hope to try without face to face conversation signals.

>recast the situation as a control process

If i assume that a student is controlling for meaning,
And if i assume that a likert rating of 5 (ex. "i am absolutely certain
That i understand") is the reference standard--rs, and if for each

Segment of learning the s rates certainty--pr, then rs - pr = error.
And k(0) = t/e, where t = processing time and e = error.

This should account for aspects of the initial learning situation,
And should begin to test if s is controlling for meaning.

My concern would be to model across several sessions. For instance
To treat feedback as a potential disturbance, and to continue the model into
Subsequent sessions/testings. And perhaps to model with universtiy
Students, the interaction of controlling for grades (robertson) and
Controlling for meaning. These two may account for the most dominant
Control systems in undergraduate learning. I intend to study your
'78 pscyh review in order to imagine how to do it.

>control theory doesn't adapt very well to experimental situations that
Have been set up on the assumption of a stimulus-response model.

I am learning that! But also i am ambitious. That is, i work in training
Environments, where learning objectives (tasks) need to be specified--that is
Specific 'responses' need to be seen--or later i may feel responsible for
A pilot crashing, or for a young teacher getting fired. Can you help me
To reorganize with this concern?

>one point of using control theory is to get away from statistical studies
In which experimenters are jubilant (typically) over correlations as low
As 0.8.

I admire the high standards. But i doubt the success of my progress in
Attempting to model higher level control systems, such as i am
Concerned with, if i absolutely must have higher correlations. For example,
If i intend to model (or help) a student who is controlling for meaning and
Grades, that student may also at the time be controlling for early dismissal
To go have a hamburger or she may be controlling for self worth.
There are multifarious other potential factors in real-life training
Environments; hence i am concerned that correlations may not be extremely
High.

>facts that are determined statistically are true only of a population
And are next to useless for predicting the performance of an individual..

I hope to use statistics on one individual to model and ultimately to
Drive the computer control of instruction for that individual.

>in your findings about the relationship between delay time and long-term
Objective response correctness, was it true of all subjects that longer
Delays...little meaning to a ct modeler, because he or she wouldn't know when
To apply the model to an individual..

No, the patterns i described (9108.03a) do not fit for all subjects.
I admit that this is somewhat enlightening. However, i am presently
Working with the hope that the particular model for an individual
And the associated response patterns for that individual can be used
In the student modelling in computer based instruction so that instructional
Feedback under computer control can be sensitive to that student's unique
Model. Do you have any thoughts along this line?

>learn the subject

Bill, thanks for your encouragement to focus my efforts on understanding
The basics of ct. I needed that! I am presently setting about to do it.
(it may be a while before i have big blocks of time again.)
I realize that i can respect your valuable time, by my doing the homework
First. I do hope to interact some on the way though.

>pct isn't just another drop in the bucket of human knowledge about behavior.
It's a new bucket.

Ct seems to me to be a golden bucket. I am hoping this bucket can be used
To hold water in my domains.

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Date: Thu, 22 Aug 1991 11:26:00 -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: Re: control from the outside

[From Rick Marken (9108222)]

Peter Parzer (910822) says:

>What I can observe in an experiment is the control of an environmental
>variable Ve. What I can NOT observe is the control of a perception.
>So why there is so much emphasis on the control of perception in
>CT ? Of course, if an environmental variable is controlled than it is
>plausible to assume that it has to be perceived. But I still do not
>understand why the Theory talks about the control of perception if
>the models that I can test empirically are about the control of
>environmental variables.

Actually, you just answered your own question. The models that you test
empirically are about the control of perceptual variables. The model
controls what it perceives. The model will not match the subject's behavior
if it is not perceiving approximately the same variable that the subject
perceives and controls. In one of my experiments I test whether a subject
is controlling the area or perimeter of a rectangle. These variables are
functions of other variables (line lengths) which are functions of other
variables (brightness patterns), etc. You could argue, I suppose, that
area and perimeter are physical variables and calling them perceptions
just adds an unnecessary layer of verbal description. But I think there are
many cases where it is clear that the controlled variable exists only in the
subject's perceptions -- the "five coin" demo mentioned by Bruce Nevin is an
example. The subject can perceive and control a pattern in the coins that
is not obvious to you, the observer. Is the pattern "really there"? I
think we get into philosophy a bit here. It is ok to say that what
we try to find, using control theory, are variables that we can perceive
and measure, which correspond to perceptual variables that are controlled by
the subject. But I think it is important to remember that people are actually
controlling what they perceive because it encourages us to understand behavior
from the point of view of the actor; actors are notorious for controlling
variables that can seem "imaginary" to an observer, leading to the frustrated

cry "what are you doing?"

>There is another problem. Can I really determine experimentally if an
>environmental variable is controlled ?

Yes. I do it all the time -- even when the reference for the variable
is changing arbitrarily. See my "Behavior in the first degree" paper in
Hershberger's "Volitional action" published by North-Holland

> Oded Maler (22.8.91) says:

>(concerning Bill's rejection of "social control")

>Sorry, but I'm not convinced.
> There is no reason I see, to refuse viewing society
>Somehow the rejection of the idea of social control seems more based on
>sentimental/ideological grounds, as if it implies some form of Fascism
>or Bolshevism (which it doesn't).

Actually, it strikes me that the idea of social control seems rather
ideological since there is virtually no evidence for the existence of
control organizations outside of the individuals participating in society.
The idea of external social control seems to me equivalent to the idea
of exvironmental control -- except that now the control is somehow exerted
by collections of living things rather than by inanimate objects (like
reinforcers). I think it will be as hard to convince people that there are
no social control systems as it is to convince them that there are no
environmental control systems (like the reinforcing contingencies of the
behaviorists). I guess one step toward convincing me that there are
social control systems is to point to what you think is an example of
the phenomenon of social control -- then model it and see if the
model behaves as expected. We already have models that show appaernt
social control (organized crowd behavior) "emerging" from the behavior
of interacting control systems. The models have no control systems outside
of (or made up of) groups of individuals.

Why do people want to believe in social control systems anyway? I suspect
it's another surrogate "higher level" control system up there in heaven
checking to see who's been naught or nice. Maybe when we find that external
control system we can finally tell which group was right about who's up there.

Best Regards

Rick M.

     ****

Richard S. Marken   USMail: 10459 Holman Ave
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213 336-6214 (day)
213 474-0313 (evening)
==========
Date: Thu, 22 Aug 1991 14:33:44 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: fooling with formants

[Bruce Nevin 910822 1403]

Martin Taylor 910822 13:35

I should have realized that changing gas density would have more complex effects. Do divers in fact compensate for changes in vowel quality by articulating differently?

One would need to track articulatory movements in this method, not a simple matter, and then correlate control measured in articulatory terms with disturbance measured in (average?) shift of formant frequencies. The correlation would have to be shown with respect to prior correlations of articulations to formant frequencies in normal atmosphere, not directly as in the headphone technique.

I should think these matters would affect the experiment?

I did talk with John Makhoul in the speech synthesis group here. The task is to separate the spectrum from the rest of the signal (pitch, etc.), distort it, and resynthesize. The hard part is writing an algorithm to do this dynamically and implementing it on a board for real-time processing. A Sun would not be fast enough. It sounded like he said a C30 board, but I don't know the nomenclature. Given that, the signal processing is straightforward, they have done it before and could do it again. Delay would be on the order of 50 milliseconds. He suggested some other organization in a position to get funds from NSF or the like might support the algorithm-on-a-board part and his group would be happy to work cooperatively with them. (BBN doesn't do NSF proposals because of size of grants, overhead charges, various parity issues with DARPA, and so on, as I found out some years ago when I tried to get something rolling.)

Bruce Nevin
bn@bbn.com

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Date: Thu, 22 Aug 1991 16:22:58 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: fooling with formants

[Martin Taylor 910822 16:15]
(Bruce Nevin 910822 1403)
>
>I did talk with John Makhoul in the speech synthesis group here. The
>task is to separate the spectrum from the rest of the signal (pitch,
>etc.), distort it, and resynthesize. The hard part is writing an
>algorithm to do this dynamically and implementing it on a board for
>real-time processing. A Sun would not be fast enough. It sounded like
>he said a C30 board, but I don't know the nomenclature. Given that, the

>signal processing is straightforward, they have done it before and could
>do it again. Delay would be on the order of 50 milliseconds.

I think 50 msec would be too long a delay. It is near the worst time delay
for the detection of auditory sequence. A "C30" is presumably a
TMS 320C30 (30 MFlop claimed) chip. We will shortly be taking delivery
of a set of 3 coupled boards, each having 3 of the beasts on it. Given
some programming support, we might be able to do some interesting
manipulations, since one of the characteristics of the set is that
it will be able to do simultaneous audio input and output on 2 (4?)
channels each way.

I'm not proposing to do the experiment, because of low time and (especially)
programmer resources. But someone else might like to, and arrangements
could be made for Canadians to use it, given the right kind of
cooperative arrangements.

I can't say whether divers partially compensate for the gas effects,
but if they do, it isn't enough to make their speech very intelligible to
someone with no training in listening to helium speech.

Martin Taylor
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Date: Thu, 22 Aug 1991 16:48:23 EDT
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: control from the outside

[Martin Taylor 910822 16:25]
(Rick Marken 910822 14:26)
>
>
>Actually, it strikes me that the idea of social control seems rather
>ideological since there is virtually no evidence for the existence of
>control organizations outside of the individuals participating in society.
>The idea of external social control seems to me equivalent to the idea
>of exvironmental control -- except that now the control is somehow exerted
>by collections of living things rather than by inanimate objects (like
>reinforcers). I think it will be as hard to convince people that there are
>no social control systems as it is to convince them that there are no
>environmental control systems (like the reinforcing contingencies of the
>behaviorists). I guess one step toward convincing me that there are
>social control systems is to point to what you think is an example of
>the phenomenon of social control -- then model it and see if the
>model behaves as expected. We already have models that show appaernt
>social control (organized crowd behavior) "emerging" from the behavior
>of interacting control systems. The models have no control systems outside
>of (or made up of) groups of individuals.
>
>Why do people want to believe in social control systems anyway? I suspect
>it's another surrogate "higher level" control system up there in heaven
>checking to see who's been naught or nice. Maybe when we find that external
>control system we can finally tell which group was right about who's up
there.
I can assure you that your last paragraph does not apply to me, as far as
I am aware.

The main reason that I find it hard to see why control systems do not exist outside of the individual skin bag is that all of the abstractions that apply to a control system in the hierarchy apply equally and in the same manner to the control systems I see existing within the cell and within a society. The skin bag seems to have no special significance for the hierarchy of control, so far as I can see. 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." Equally, the elemental control system is in a community of control systems at the same level of the hierarchy, about which it may know nothing except that sometimes the perceptions it controls don't come out the way it wants (because parallel control systems are using the same effectors to make their perceptions correct as well).

The more I think about it, stimulated by the counter-arguments from Bill, Rick, and Bruce, the more I find it hard to find a non-ideological reason for the claim that control systems can exist only within a single animal skin. Bill has made the best case so far, but I think even his eloquent argument fails, because it seems to me to hang on the inadmissibility of abstracting from sensory experience to create the perceptions being controlled at different levels, and that ability is a required ability for all levels of the hierarchy except the first.

I believe I have no ideological committment in this, except to the kind of beauty in theory that drove Einstein. I "prefer" a theory that accounts for phenomena in the society of animals (or people) in the same way that it accounts for phenomena in the society of cells. For all its admitted lacunae, HCT is a theory that has considerable beauty. I'd like its limitations to be intrinsic, and to be self-evident in its construction. So far, it still seems to me that the limitation is arbitrarily imposed rather than being inherent in the theory or the world to which it might be applied.

Martin Taylor

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Date: Thu, 22 Aug 1991 18:54:47 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: aphasia (really alexia)

[Martin Taylor 910822]
Long delayed comment on Marken 910620

I mentioned a case of alexia without agraphia--the stroke patient can write satisfactorily, but can't read. Marken commented that probably the patient could write equally well fast or slow. I don't know whether that was true, or would be true in all cases of alexia without agraphia, but there is a case on record of a parallel problem in phonemic alexia (problems with phonology and function words). The patient in this case

had been a voracious reader, as I understand it, and a speed reader. After the stroke, he had great difficulty reading slowly, but his speed reading was essentially unimpaired.

Ref. Andreewsky, Deloche, & Kossanyi. Chapter in Coltheart, Patterson and Marshall (Eds) Deep Dyslexia, London: Routledge and Kegan Paul, 1980.

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Date: Fri, 23 Aug 1991 06:04:20 -0600
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Social control; The Test

+[From Bill Powers (910822)]

Bruce Nevin (910821)

Your representation of "commands" from outside as disturbances works well: they represent changes of conditions with which the person must somehow cope. The platoon leader says "Advance!" This creates a situation requiring me either to expose myself to enemy fire or to explain to the platoon leader (and eventually the Provost Marshall) why I have concluded that it would be wiser to go the other way. I must also deal with my own goals regarding patriotism, cowardice, hesitation to do harm to others, organizational consequences of disobeying an order, helping my co-dogfaces, and so on. It's a problem -- but it's not control from outside. Even the Army admits that obeying orders is controlled by the individual. Otherwise there wouldn't be any mechanism for dealing with disobedience. In general, the law considers intent a necessary component of committing a crime. Intent without control means nothing.

As to the neuropeptide stuff, this brings in a kind of "science" with which I try to have as little to do as possible (even if the mainstream finds it adequate):

>Candace Pert (chief of brain biochemistry at NIMH) has shown that
>neuropeptides function as "signal molecules" or "communication
>molecules" in living organisms. They are found in all living organisms
>from single-celled organisms to humans. There are 50 or 60 known, made
>directly from the DNA without an intervening enzyme, configured from a
>single polypeptide. In vertebrates, they link the nervous system,
>endocrine system and immune system into a "bi-directional information
>network" (this means transmitting, receiving, and monitoring and
>adapting to information flow). They appear to help integrate behavior
>by regulating mood. Neuropeptide receptors are clustered at primary
>points for sensory input. "Even before we register our perceptions,
>they are colored by emotion." (Quotes from 1986 conference in D.C.)

What does "a bidirectional information network" mean? It could mean control systems or anything else. The KIND of "monitoring" and "adapting" going on completely determines what kind of system you mean: closed-loop, one-way deterministic or stochastic, oscillatory, passive equilibrium, mish-mash network, and so on. I can't interpret "They appear to help integrate behavior by regulating mood." What does she mean, "appear?" What does she mean, "help?" What does she mean, "integrate?" What does she mean, "regulating?" What does she mean, "mood?" This is arm-waving. Mood and chemistry may have something to do with each other, but who says

that chemistry regulates mood? I can think of some excellent arguments for saying that mood regulates chemistry. The brain (via the neurohypophysis) issues reference signals to all the major organ systems. Does this person have any inkling of what is entailed in the term "regulate?"

>"The emotions are not just in the brain, they're in the body."

Exactly what I've been saying for many years. Error signals in the brain result in resetting both behavioral and biochemical reference signals. The somatic effects of resetting major biochemical reference signals are experienced as the feeling-part of emotions. The emotion-name we give to a particular somatic feeling-state depends on the behavioral goal that initiated it: to flee, to attack, to undo, to give up, to let go of the lost, and so on. Fear, anger, guilt, depression, grief. These same somatic states occur during ordinary behavior, but are not given the names of emotions if we succeed in carrying out the goals immediately. Emotions are felt as such mainly when control is made impossible -- for instance, by inner conflict. Of course the biochemical aspect of behavior involves many chemical signal carriers, signal; amplifiers (enzymes) and other kinds of biochemical processes. How else could biochemical control systems work?

"Even before we register our perceptions, they are colored by what we want." (WTP, correcting Pert).

There are lots of little local relationships among biochemical processes in the body and I don't doubt that they are being competently observed, but so far nobody has started putting them together into a picture of a whole system. Biochemists have recognized negative feedback, but they still reject the idea of a reference signal. So they can't yet come up with a hierarchical control model.

Between the neuropeptide level of observation and observations of organized behavior or experience there are huge conceptual gaps which most researchers try to bridge with wild guesses and statistical generalizations. They're not going to get anywhere that way. They need a model of the whole system, even if they have to revise it later. Control theory could help.

>Benjamin Libet (UC SF) has been studying EEG correlates of conscious
>experience since the early 1960s.

I don't think that the EEG is going to tell us many details about how the brain works. It's about as effective as sticking an oscilloscope probe into the space between backplanes in a computer, and trying to figure out the program from the electrical fields that are picked up.

Reference signals are internally set prior to the behavior that matches perceptions to them. It isn't surprising that signals should be detected that precede the behavior that follows them -- at least, this observation is consistent with the control-system model.

Brain researchers generally seem surprised to find that brain activity correlates with conscious experience. This makes me wonder what they

thought the brain was for.

>Now, what if there were mechanisms for transmission of reference values
>from some hypothesized interpersonal or transpersonal level?

Even if such means existed, the external agency would have a problem with fighting the other systems in the brain already contributing to those same reference signals. The goal structure in an individual has evolved through a lifetime of learning and interacting with the world; everything interacts with everything. How would you react if something told you to give up liking linguistics? Your goals serve your needs, not those of others. Even your altruisms have been structured to satisfy your concept of the "right" way to help and accomodate others.

You can't change just ONE reference signal in the brain and expect anything but massive resistance to the change. This is a SYSTEM, not a collection of reactions.

>Or what if individuals voluntarily set certain reference signals within
>themselves to socially-agreed values as their only or best means of
>controlling certain other perceptions that had higher priority to them?

This is how I assume it works. The setting is done by the individual. I have never said that it is done without regard to happenings in the perceived external world or without regard to other reference settings in the same individual (see first paragraph of this post). I'm only saying that there is no way for an external agency to reach inside an intact individual and physically alter reference signals. Or no way that would work in a significant number of cases (psychosurgery?). Even if you could do this, a higher-level system would immediately restore that reference signal to its former setting, or a conflict would be generated, destroying control. Unless you broke something in trying to effect the change.

>Or what if together with the tidy neural circuitry postulated in CT
>there were also untidy, leaky, messy mechanisms such that perceptual
>control were not so deterministic and mechanically inevitable as we have
>made out?

That's fine, but first let's go as far as we can with explaining behavior using reasonably tidy neural circuitry. It may turn out that what looks untidy now will turn out to be realizable in neural circuitry -- for example, Joe Lubin's models of perceptual organization which I trust he will be telling us more about soon. When we have exhausted the possibilities and explanatory power of organized models, what is left over will have to be explained in some other way. But I don't think we have come anywhere near exhausting those possibilities -- we've hardly even tried them yet.

Reorganization is pretty untidy, isn't it? It's part of the model. And the circuitry doesn't have to be as neat as you suggest in order for control systems to operate perfectly well.

>Might reference signals within an organism be seen as "suggestions" from
>higher-order control systems to lower-order ones? Might neuropeptides
>mediate "suggestions" among peer control systems? Might people's
>willingness to "take the attitude of the other" (Meade) for the sake of
>cooperation and other social benefits account for interpersonal
>suggestion at the heart of the social order?

Maybe. Show me a model that works that way. I can't think of one.

If a higher-order system "suggests" reference signals to a lower one, how does the lower system decide whether or not to take the suggestion? What intelligence does it need to make that choice? If it rejects the suggestion, what is the source of the effective reference signal? What happens to the higher-order system when its suggestion is rejected? Does it give up trying to control its own input? A model of this process would allow us to deduce answers to all these questions.

I see "suggesting" as more like the mode in which one person attempts to influence reference signals in another person. Whole persons have all the levels of organization needed to deal with suggestions. I can't see any one subsystem having them.

>I do not think that any of us is in a position to be dogmatic with
>respect to the status or effect of a Durckheimian "social reality".

I hope I'm not being dogmatic. Dogma is stating conclusions without justification. I justify all my statements as clearly as I can, referring to the publicly-defined model from which I deduce them. As far as I can see, a "social reality" that has the same common existence for all people would be inconsistent with the control-theoretic model (as well as its epistemology). If this concept is consistent with some other model, then I suggest that the other model be presented and its properties be laid out. It would also be nice to see some tests, even if they are very simple, that the other model would have to pass to be accepted.

>We can say that hierarchical control theory as currently formulated has
>no place for social control except as something that individual living
>control systems create in some statistical or "self-organizing systemic"
>sense by their individual choices and actions.

That is true. What else is required to account for social phenomena? What phenomena of social living are not accounted for by the control-theoretic model?

With regard to Eric Crump's comment about muscles: muscle-control systems receive physical reference signals from higher in the nervous system. This makes them subordinate to the rest of the nervous system. The highest-level control systems do not receive physical reference signals from anywhere outside the nervous system: there are no paths that could carry such signals. The only route from outside carries perceptual signals, which report only the current external state of affairs -- not the state that is desired or intended. At the portals of the nervous system, we have sensors that deliver signals representing their own

states of stimulation. There is no added information entering those one-dimensional channels that could also indicate the "right" state of the signals.

Peter Parzer (910822) (two o'clock in the morning, hey? Or is that just your mainframe staying awake?)

>What I can observe in an experiment is the control of an environmental
>variable Ve. What I can NOT observe is the control of a perception. So
>why there is so much emphasis on the control of perception in CT ? Of
>course, if an environmental variable is controlled than it is plausible
>to assume that it has to be perceived. But I still do not understand why
>the Theory talks about the control of perception if the models that I
>can test empirically are about the control of environmental variables.

What we observe is, as you say, an environmental variable. We also observe the organism's actions and disturbances. When control exists, the actions of the organism make the variable resistant to arbitrary disturbance. We also observe that if the variable is shielded from the senses (where possible), this resistance no longer occurs. We see that the relationships between action and disturbance, and the joint effect on the controlled variable, can in simple cases be accurately predicted, even to the dynamics, by a working control-system model. This is our best behavioral evidence in support of adopting such a model.

In some cases we can trace the mechanisms of control closer to the nervous system. For example, we can interpose mirrors or prisms between a visually-controlled variable and the sensors in the eyes. The result is that the image on the retina continues to be controlled as before, but now the former controlled variable is shifted and is controlled about a new apparent reference level. This tells us that the control is not of the objective variable, but of its image on the retina.

In other cases we can trace the input chain to signals inside the nervous system. In the case of the known reflexes, we can even complete the loop, finding the perceptual signal, the reference signal and comparator, and the output function and effector. The control-system model can then be matched function by function and signal by signal to the morphology of the real system. Where quantitative measures are available, we can plug them into the control model; otherwise we can arrive at settings for parameters that make the model behave very similarly to the real system. We can, in fact, reproduce simple behaviors far better than is possible using any other model. Perhaps some less-prejudiced net members who attended the last CSG meeting and saw the Little Man pointing model with dynamics will comment on that claim.

Where we can do such tracing, the control-system model helps us to understand what we find. Instead of general terms like "modulate" and "inhibit" and "adapt," we have terms for quantitative relationships that can be used in simulations; we discover what these connections do, what part they play in the overall operation of the system. This converts data into knowledge.

We also observe variables being controlled where we can't trace the internal connections. Seeing the same relationships as we find in the better-known systems, and succeeding in fitting control-system models to the observed control processes, we naturally extrapolate and assume that a similar organization must be involved internally. There is more latitude for alternatives, however. We can't pin down just how the perceptual process is carried out, nor where and how comparison takes place, nor how the errors are converted into more detailed settings of reference signals for lower systems. So we just draw boxes that represent a typical or generic system and leave it to the future to tell us what is in the boxes and what the right detailed organization is. We must often be satisfied with a sufficient model rather than the correct model.

Whatever the model for control of abstract variables, we can always return to the fundamental level of definition and apply the tests that show that a control system is present and acting. These tests do not rely on assuming a model. We disturb the proposed variable and see that action ensues that opposes the effects of the disturbance. We delete the link from the variable to the senses, and show that control is lost. We solve the closed-loop equations and find that there is an offset in the input-output equation imposed by the behaving system (the reference level). All of this can be done strictly by using publicly-observable variables. Note that reference LEVELS are publicly observable; reference SIGNALS are part of a hypothetical model that explains the existence of reference levels.

From all of this, we can be sure that a control system is present and operating even if we don't know the details of its operation. We know at least the externally-observable counterpart of the hypothetical internal perceptual signal. We can strongly suspect that a perception is involved because we, the observers, are also perceiving the variable using presumably similar neural apparatus.

In short, we can build up a very strong circumstantial case for adopting the control-system model as a model of both the behaving system and the observer. We can't complete the argument, of course, until neural research finds how the various functions are actually carried out, by neural circuit-tracing and neurochemical analysis. The control-system model can suggest interpretations of detailed neural relationships that are found by direct inspection; in turn, findings that deviate from the generic model will tell us how the model must be modified, producing better interpretations as the neural and biochemical investigations continue.

We can also approach this problem from the inside, the private point of view. By examining experience critically, we can find perceptions of our own that seem to be under control. Perhaps we can even find classes of perceptions that are related in the manner expected under a hierarchical control model. These subjective analyses help us to find variables for more objective treatment, using the tests and the methods of simulation. The private and public approaches often meet in the middle: the model proves congruent to private experience, confirming it and suggesting new experiences of interest. We can't observe the perceptions of others -- but we can certainly observe our own (there is nothing else to observe).

>There is another problem. Can I really determine experimentally if an
>environmental variable is controlled ?

If by this you mean "determine objectively, beyond doubt, with complete certainty," the answer is No. It is always possible that Nature has come up with some invisible force that just happens to simulate an action that opposes a disturbance; it is always possible that the action that seems to oppose the disturbance is really generated at random and just happens, by luck, to combine with an independent disturbance to yield a constant state of the supposed controlled variable affected by both. It is possible that just as you block the sensing of the controlled variable, the random outputs or the independent disturbances change their patterns and lose this fortuitous equal-and-opposite relationship.

All we can say is that the model provides a plausible and economical explanation of why we see what we see. It seems much more likely that the control we observe is the product of a system organized this way than it is that we are seeing a highly unlikely confluence of independent events that have a systematic-seeming outcome but only by chance.

On the other side of this picture, it is very unlikely that our first guess about the nature of a complex controlled variable will turn out to be the best guess. We observe the variables involved in behavior from a prejudiced point of view: our own experience. What interests us may not be important to the system we are studying. We may see that a variable is stabilized against disturbance for a while, but that the next disturbance of a slightly-different type is not resisted at all. This shows that we have defined a variable that is related to the "true" controlled variable but is different from it in some regards. From the differences, we can deduce modifications of the definition. Only when the variable proves stable against every kind of disturbance that could possibly affect it have we found a truly plausible definition.

One thing is certain: we can't just start with some variable we observe in the environment, and assume that it is pertinent to the behavior of the test organism. I know that is the custom, but it is a mistake.

Last point:

>Assume I introduce a disturbance to Ve and the behavior of the subject
>does not bring it back to the original value. Now I can say, that the
>subject does not control that variable. This assumes that the reference
>value stayed constant. But I can equally say that the subject does
>control the variable, assuming that the reference value changed.

This is correct as well as insightful. Our external tests for controlled varables depend on the reference signal remaining reasonably constant -- in some cases, precisely constant. If we DO find a variable that passes the test, it is very likely a controlled variable; the reference signal must have remained constant during the entire test.

But reference signals do not always remain constant. In that case, we will not find a controlled variable. Remember that this is not a statistical test in the usual sense. We aren't interested in correlations

as low as 0.9. The relationship of the action to the disturbance, in terms of effects on the controlled variable, must be a quantitatively equal and opposite one. It must ALWAYS hold. ANY exception calls for a redefinition of the controlled variable.

In the Coin Game alluded to by Bruce Nevin, the Subject lays out four coins on a table so that some pattern the subject has in mind is contained in the layout. The Experimenter disturbs the arrangement. If the subject can no longer perceive the intended pattern, he or she moves one or more coins so that the pattern is again visible. If the Experimenter's move left the intended pattern still visible, the Subject just says "no error." The game is finished when the Experimenter can create disturbances that predictably call for corrective action, and disturbances that predictably result in "no error." The criteria can be adjusted as suits the players and their degree of skepticism.

An example of such a pattern is "at least one right angle." An experimenter unaccustomed to this test for the controlled variable might take half an hour to discover this pattern. Of course it is possible to devise patterns that are undiscoverable, if you like wasting time.

This relates to your point above as follows. It is impossible to discover what pattern the Subject is controlling if the Subject keeps changing the reference pattern during the game. In such a case, only the Subject and God know what the controlled variable is at any moment. Even in this kind of case, however, the Experimenter can go up a level and approximate a higher-level controlled variable: "Either you've got an extremely obscure pattern in mind, or you keep changing it every time I get close."

I concur with Bruce: playing this game will teach one a lot about how to find controlled variables, and the pitfalls of assuming that "insights" into what another person is doing have any relevance. The Test is all about eliminating wrong hypotheses. When you get systematic about doing that, you can guess very efficiently. If you get hung up on a clever hypothesis, you may take forever to find the right controlled variable.

It is also instructive to discover that a perfectly good verbal definition of a controlled variable that passes every test is not the one the subject used. You say "It's a zig-zag!" The subject says "No, it's an 'N' on its side." Of course, it's REALLY a "Z." In fact, it's the perception that the Subject intends to reproduce, not its description.

Oded Maler (910822)

>There is no reason I see, to refuse viewing society as a large organism
>with all that control stuff. True, the fact that it is realized by a
>collection complex, lossely-coupled, big-brained, language-speaking
>individuals may imply much more complex dynamics, and different ways of
>analysis, but *in principle* it is not different than seeing "control"
>in a floating soup of proteins inside the cell, or in a collection of
>neurons. These are just different time-scales, different distances and
>different topologies of information channels. Somehow the rejection of
>the idea of social control seems more based on sentimental/ideological

>grounds, as if it implies some form of Fascism or Bolshevism (which it
>doesn't).

My main reason is that society is NOT a large organism. I am very literal-minded. I do not think we can explain by using metaphors.

Another reason is that 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). It also has quantitative implications: the kind of control we see in living control systems requires loop gains of at least 5 to 10, and often entails FAR higher loop gains. Loose coupling can't achieve control of any interest.

If you are using the term control in a non-technical sense, meaning perhaps determine, or influence, or limit, or cause, or interact, then you do not mean what I mean by it. Systems in which the relationships are not those of negative feedback with high loop gain simply cannot achieve control of anything, living or dead, in the terms I mean. Control is a definite, specific, striking, overlooked phenomenon that is seen ONLY in the behavior of living systems (or their inventions). Phenomena of other kinds can be seen in many types of systems and among independent control systems. A different model is required in order to represent them. They are interesting, but they are not control. If you don't distinguish control from these other kinds of phenomena, then you have not yet recognized the existence of the phenomenon that control theory is about. I wouldn't blame you for that. Few people have. But this complicates our discussion because then we are not talking about the same thing.

Looking over my shoulder at the mail this morning, Mary said "It looks as if you've disturbed a lot of control systems."

Best to all

Bill P.

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Date: Fri, 23 Aug 1991 08:15:43 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: experiments with phonetic control

[Bruce Nevin 910823 0702]

(Martin Taylor 910822 16:15)

I may have garbled the message, one point was that BBN would do the algorithm and implementation if funds were available to pay them for it.

>I think 50 msec would be too long a delay. It is near the worst time delay
>for the detection of auditory sequence.

I don't understand. Auditory sequence would not be affected.

Articulatory gestures would be decoupled from auditory consequences by a ~50msec delay. The intended focus of experiment is on vowels, which are of relatively long duration. A potential problem, to be sure, is what happens to transients between stops and vowels if you shift a band of frequencies within which falls not only the target formant but also all or parts of the transients associated with adjacent stops. Perceived quality of sibilants might be affected too, though much of that is at higher frequencies than F2 and F3, and F4 is of limited import.

A physical analog would be so much more reasonable to work with! What we want to model is the interaction of control systems with the environment, which includes crucially the acoustic and other physical properties of the nose, mouth, and throat. How hard would it be to simulate those physical properties in software, so that control systems could "do whatever it takes" to approximate an idealized pronunciation model of words? (Approximate: depends on the the interactions between competing control systems and the gain on various control systems, which varies as noted previously.)

Still, demonstrating control in the way sketched could have far-reaching consequences for phonological theory. The growing consensus in the field has been that the "phonological component" works with elements defined mostly in articulatory terms. (This seems to be because of the variability noted above--can't pin down well-defined acoustic parameters by examining outputs, strange to say! Articulatory parameters are conceived as targets which are approximated.)

I would like to be able to demonstrate e.g. a speaker's lowering of the i of "pin" to something close to the e of "pen" (distorted audio in headphones sounding like normal i vs. e), hopefully without being aware of doing so. (Midwest speakers of English often lack this contrast, and there's a problem about intervening ey as in "bait," and that suggests some of the difficulties involved, but this will do to illustrate the idea.) Phonologists might object that the speaker was still working with articulatory targets, which are known to be rubbery. So having hopefully demonstrated control at least for ourselves, a second experiment might get a speaker to swap articulations of two vowels. After the shift described above (lowering i to near e), have the person read material that contains occurrences of e but no occurrences of i, and raise e to where i was. Then reintroduce (altered) i. I don't know if it is doable, but it would be fascinating and most shocking to phonologists if it were.

Unfortunately, I currently have no equipment, no money, and no pull. I only have ideas.

>I can't say whether divers partially compensate for the gas effects,
>but if they do, it isn't enough to make their speech very intelligible to
>someone with no training in listening to helium speech.

Do divers find speech production disconcerting for more than a brief period? Do they get the "loose steering linkage" feeling about their articulation, as though it were becoming wobbly and hard to control? I'll have to look for a source of helium and experiment. Anyone else in a position to try? It may be that articulatory parameters will turn out

to be sufficiently controlled that the acoustic experiment outlined above would not work as anticipated. It is hard to see how to introduce disturbances to *articulation* so as to demonstrate control such that the person continues to produce undistorted speech as perceived by himself. Physical appliances placed in the mouth can change the acoustic properties of the oral cavity and articulators are much more difficult to instrument than acoustic output is. It really would be much nicer if acoustic control rules. Fits with the social character of language, too, as Jakobson noted in originally proposing the purely acoustic definitions of distinctive features of Jakobson, Fant, and Halle which have since been abandoned as unworkable.

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Date: Fri, 23 Aug 1991 09:13:43 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: control vs. influence

[From: Bruce Nevin (910823 0732)]

Martin Taylor 910822 16:25

>and within a society. The skin bag seems to have no special significance
>for the hierarchy of control, so far as I can see. 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."

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.

[Aside: As to the mystery of top-level reference signals, I have suggested that they come from the stories we tell ourselves including in that theories, myths, and dogmas, so that the upper end is closed through the pseudo-universe of language and symbol just as the lower end is closed through the environment. This does not account for organisms without language or symbols but the present set of hierarchies says that any organism with a program level has a category level and perhaps the taxonomic and other connections among categories suffice. One way or another I suppose it could be claimed that interconnections among control systems could be made to generate reference signals at the highest level of control in an organism.]

The "readiness potential" work cited before suggests that where there is no established reference input (the signal for when to initiate the flip of a finger or wrist, in those experiments) some normally inconsequential event or body state may be used to generate the signal.

The description of the RP suggests a cascading effect of oscillating feedback which serves as ad hoc reference signal by crossing a threshold. A leading to rise and speak in the silence of a Quaker meeting often has a subjective quality that accords very well with this description.

The carrying out of familiar sequences and programs exemplifies perceptual control very well. The occasion for initiating one or another program or sequence is not always so clear. It appears to come out of a realm that is much more wet and leaky than the control hierarchy, a realm with which we associate emotion and empathy, intuition and impulse. For mechanism I have suggested the emerging understanding of neuropeptides, and I hope Candace Pert gets back to that after she finds a cure for AIDS in the autoimmune systemic connection of her neuropeptide research. The implications for interpersonal influence are considerable.

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.

Within the `bag of skin' you have hierarchical control, perhaps made a bit more mushy than some would like by mechanisms that can render some reference signals subject to influence.

Between `bags of skin' you have influence. Interpersonal and social influence is sometimes made more hierarchically controlling than is appropriate by interpersonal coercion and manipulation. These techniques always run into problems because they result in conflict within the `bag of skin' being coerced or manipulated. Chapter 17 of BCP describes this dilemma clearly.

Influence works best by suggestion. As long as it is not deceitful (which is a form of manipulation, and does not work when experience eventually gives rise to conflict), suggestion works very well. As Albert Schweizer said, "there are three ways to teach a child: The first is by example. The second is by example. And the third is by example."

Has no one in this group looked at hypnosis?

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Date: Fri, 23 Aug 1991 07:24:18 -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: Misc comments

[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.

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Date: Fri, 23 Aug 1991 10:28:36 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: Candace Pert

[From: Bruce Nevin (9123 0815)]

Bill Powers (910822)

Don't be too hard on Candace Pert. I'm reasonably sure her work has been misrepresented here. I haven't read any of her writings. She spoke at a conference involving a non-technical audience and the report I excerpted and paraphrased was a summary in a periodical with a largely non-technical audience. I agree, it is very unlikely that she has a model remotely approaching CT. But the point I want to make is that there are *mechanisms* other than neural fibers by which rates of firing at one location might *systematically* affect rates of firing at another. These mechanisms have to do with the locations of receptor sites for neuropeptides and means by which these chemicals may be propagated in the body. It is not clear that such mechanisms are properly speaking part of the environment of the strictly neural hierarchical control system, in the same way that the acidity of the stomach or the blood-sugar level or the acoustic properties of the oral cavity are part of its environment. They seem rather to be an extension of it, with somewhat different properties. All this is highly speculative, of course. So what else is new. But if we understood the findings of Pert and others a bit better we might accomodate them in suitable extensions of the present CT model. I doubt we are ready to undertake that. But the higher-level point is that control systems may be more amenable to suggestion at all levels than we presently realize. I can't prove that or model that or yet test that, but I would urge clemency for views that such a finding would support, regarding social influence (as distinct, yes, from social control).

Bruce Nevin
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Date: Fri, 23 Aug 1991 09:46:15 -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: yes, and yes

[from Joel Judd]

Bruce and Martin,

Not that I'm any fan of Krashen, but something that Bruce said might make sense for another kind of experiment:

> we often fail to notice
>many of our deviations from our internal model of language, and I
>should have linked that explicitly to a range of variation in the
>gain on control systems controlling for conformity to that model,
>as mentioned.

This factor of loop gain might be the way to take care of Krashen's "Monitor." I don't know how familiar Bruce is with this (I assume Martin is), but his whole argument is that people often correct themselves either just before or after an utterance (spoken or written) by using a system called the "monitor." This is an explicitly learned "conscious" system of linguistic rules which can act on the acquired "unconscious" linguistic system. Ignoring all the problems with this conception which have already been well-criticized, what about using self correction as a way of determining loop gain in a given language learner, or as evidence of the gain in the first place? It wouldn't help in cases like Martin's wife where one can't determine accepted use even with the opportunity to do so. But in cases where the speaker knows accepted use, but just needs the time/context to produce it, wouldn't this be one place to start? Or am I barking up the wrong side of the feedback loop?
Joel Judd
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Date: Fri, 23 Aug 1991 08:08:46 -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: Control of perception

[From Rick Marken (910823)]

In rereading my post (910822) to Parzer I realized that I went off course when I started talking about my "area/perimeter" study. The point of that study is that the same rectangle can be perceived in at least two ways -- as x*y (area) or 2(x+y) (perimeter). The same entity is out there in both cases. What is seen (perceived) depends on which function of the lines of the figure is computeed by the subject's perceptual system. Subject's (and the model) behave quite differently depending on whether they are controlling x*y or x+y. This difference in behavior depends entirely on which perceptual variable is being controlled. The subject's effect on x and y is always the same. However, when the subject is controlling x*y, the subject's response to a disturbance differs from what it is when s/he is controlling x+y. A subject can mentally switch from perceiving the rectangle in terms of area to perceiving it in terms of perimeter. This mental switch is easily detected in terms of the subject's response to disturbance. The important point, however, is that there is practical significance to understanding that it is perceptions, not environmental variables, that are actually controlled. In the "area/perimeter" case the alternative perceptions seem obvious. But in more realistic situations there are many possible mappings of an apparently objective

"environmental variable" into a perceptual variable.

Hasta Luego

Rick M.

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Date: Fri, 23 Aug 1991 14:01:54 -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: Re: control from the outside

[From Rick Marken (910823b)]

Martin Taylor (910822 16:25) says:

>The main reason that I find it hard to see why control systems do not
>exist outside of the individual skin bag is that all of the abstractions
>that apply to a control system in the hierarchy apply equally and
>in the same manner to the control systems I see existing within the cell
>and within a society.

Do you really see them? What you might see in the cell is control occuring -- maintaining a certain level of chemical concentration. You then imagine a control model that might produce such control -- even assigning functional roles to likely cell components. But in a society you see what appear to be the components of the model used to explain control -- this person functions as a perceiver, that one as a comparator, that one as an effector -- but you don't see any control being effected by these individuals as a group -- do you? If so, where is it. What is being controlled by the hierarchical control system made out of individuals? And, to the extent that you can identify a control, is it control that CANNOT be explained in terms of the operation of each individual control system that makes up the group?

>The more I think about it, stimulated by the counter-arguments from
>Bill, Rick, and Bruce, the more I find it hard to find a non-ideological
>reason for the claim that control systems can exist only within a single
>animal skin.

We (Bill and I for sure) never made that claim. We are just saying (I think) that there is no phenomenon of control that is explained by imagining that groups of people are an operating control system. We also see no evidence of a social control system external to groups of people -- whatever that system might consist of. If there were evidence of control carried out by groups of people and if this control could not be explained in terms of the individual control actions of people, then we would be happy to entertain

the proposal of a "multi-animal" model of control. I hate to get repetitious about this but control theory is a model of a phenomenon. We like the model, not because it is beutiful, elegant and developed by a wonderful guy. We (I) like it because it explains the phenomenon of control, a phenomenon that has been completely neglected by psychologists. What phenomenon of control does your proposed "multi-animal" control system explain?

As Bill Powers says:

>   Systems in which the relationships are
>not those of negative feedback with high loop gain simply cannot achieve
>control of anything, living or dead, in the terms I mean. Control is a
>definite, specific, striking, overlooked phenomenon that is seen ONLY in
>the behavior of living systems (or their inventions). Phenomena of other
>kinds can be seen in many types of systems and among independent control
>systems. A different model is required in order to represent them. They
>are interesting, but they are not control. If you don't distinguish
>control from these other kinds of phenomena, then you have not yet
>recognized the existence of the phenomenon that control theory is about.

Hasta Luego

Rick M.

****

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Date: Thu, 22 Aug 1991 01:31:28 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Social Control?

From Tom Bourbon [910822] --

   Just a brief note, to echo the remarks of Bruce Nevin [910821], Rick Marken [910821] and, especially, of Bill Powers [910821], who replied to Eric Crump [910820] and Martin Taylor [910820], concerning alleged "insertion" of reference signals into individuals, by "social systems."

    Bill was on the mark when he said the only way that could occur would be were the individuals who comprise the "system" to bore a hole into the skull of the unwitting controlee and somehow manage to stimulate all of the proper channels that could eventuate in a perceptual reference signal, which is, after all, a "request" for a perception. That is all the "brain" ever provides to the pathways that eventually reach muscles -- the brain does not send commands to the muscles. In spite of the massive literature to the contrary, there is

no convincing evidence that the brain commands anything, so it is a poor analogy for a social system that commands the behavior of individuals. Behavior is not the end result of a linear chain of command, wherever that chain is alleged to begin, whether in a "stimulus," a neural "command" or a social edict.

In a cooperating group, each individual adopts reference signals for her or his own perceptions. Each individual acts on the environment to achieve the perceptions requested in those reference signals. Living systems cooperate (a) when doing so allows each of them to achieve control of perceptions that neither system could control when acting alone, or (b) when they decide to do so for the sake of doing so -- which allows them to control for doing so. In no way does the "cooperating group" put reference signals into the head of any member of the group. All any member experiences are perceptions. Whether the perceptions are even disturbances depends onthe reference signals already adopted by the member.

If it were possible for a group to insert reference signals into the heads of others, do you really think control theorists would miss that trick? It would be infinitely more simple than all of this pounding of keys and flapping of tongues that we go through!

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Date: Mon, 26 Aug 1991 05:18:00 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: Wordprocessing

For Bill

As I understand it, you download then read and reply on your stand alone computer, before you upload. You must have a program which allows you to conveniently insert quotes, complete with > chevrons to signify the quoted passage.

I can see how to paste/copy from a second document in Wordperfect or some other
program, but can't see how to get the > easily.

How do you do it? Do you use a special mail reading program?

Thanks!

Dag Forssell
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Date: Mon, 26 Aug 1991 09:07:45 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: experimental linguistics, loop gain

[From: Bruce Nevin (910823 1108, 910826 0704)]

Joel Judd (Fri, 23 Aug 1991 09:46:15 -0500)

>might make
>sense for another kind of experiment: . . .
>what about using self correction as a way of
>determining loop gain in a given language learner, or as evidence of the
>gain in the first place?
>where the speaker knows accepted use, but just needs the time/context
>to produce it,

Please say more about what kind of experiment you have in mind.

When we know gain is high on a particular controlled perception ("I said `car,' not `gar'!") we can get some data about the ideal or model constituted by the reference signal(s) for that perception, data that might not otherwise be available or as precise. We can get a better idea about syntax and semantics from written prose than from speech, and in formal contexts in general gain is higher on control of many linguistic perceptions than it is in informal contexts. Once we know what constitutes tight control for a given perception, we can use the concept of loop gain to account for less tight control. We might be able to use it to help differentiate levels of control.

I don't think we lack for evidence that loop gain varies, given acceptance of a CT account of speech as behavioral output. However, we don't have that prior acceptance in the field of linguistics. So the fact of perceptual control must be demonstrated first.

It is generally accepted that careful and formal speech is a more "accurate" rendition of speakers' intentions. Bloomfield used to speak very clearly and carefully, in the expectation that his informants would be more likely to do likewise. He did this for example when speaking Menomini while living with monolingual Menomini families in Wisconsin. This accords in a way with the old rhetorician's advice to pronounce the "d" on the end of the word "and" clearly, and clearer articulation elsewhere would naturally follow. Countering this is the observation that people often maintain both the standard of speech to which they actually conform and another to which they feel they should and believe they do conform (cf. Labov's measure of linguistic insecurity). With appropriate caveats and precautions about interference of multiple standards, I believe that CT for the first time provides a clear explication of and principled motivation for our preference for careful and formal speech as being a more "accurate" representation of the language.

The amount of self-correction is a function not simply of loop gain but

of loop gain together with the number of errors that are subject to correction but nonetheless escaped it in normal production, so you have to consider not only loop gain but also the various contributors to error. Probably most error arises from competition between control systems.

Slips of the tongue are usually corrected. They have been used to demonstrate that certain morphological changes are phonologically dependent rather than lexically dependent. Thus the following from Stemberger (1983) shows that form of certain English suffixes, voiced or voiceless, is related to the voicing of the preceding phoneme rather than being associated with the intended lexical item:

> The infant tucks--touches the nipple.
> gooed up--goofed up

If the alternation between /-t/ of goofed and /-d/ of gooed were lexically determined, then we would hear "goot up" when the person elides the f of "goof". One might argue that the slip was lexical and that the word "goo" was substituted, but that argument does not hold in any obvious way for tuck/touch and for many other slips of the tongue. Indeed, it is the incongruity of seeming word combinations that makes many slips of the tongue (malapropisms) so funny.

There has been very little experimental work in linguistics outside of phonetics. An excellent example of an experimental study is Stemberger and Lewis (1986). They worked with Ewe, an African language in which reduplication "the ultimate in phonologically dependent allomorphy" is a common form of inflection. A few examples may illustrate the concept: si "escape" sisi "escaped," fia "burn" fafia "burned," dzra "sell" dzadzra "sold." Note that the first syllable is not a complete copy if the original begins with more than one consonant. (The affricate written dz is a single consonant, the i in fia is a y glide.)

Ewe speakers were presented pairs of monosyllabic Ewe words, one pair at a time on a CRT screen, to read silently. The first word of each pair was a verb, the second some other word, without regard to whether or not they made a meaningful phrase. Some of the pairs were followed by a cue for the person to say aloud the last pair read silently, transforming the verb (the first word) into its participle form, which is reduplicated. The three word pairs preceding always set a bias toward producing consonants in a certain order, and the target pair always had the consonants in the reverse order. The object was to induce slips of the tongue. An example of three bias pairs followed by a target pair:

> be du
> ba de
> bu da
> di bo <

The first three pairs have b plus vowel followed by d plus vowel, and the target pair (marked < here) has d plus vowel followed by b plus vowel. The reduplicated form for the target pair should be:

> didi bo

On a certain number of trials, the reader made an error where the wrong consonant was reduplicated, often also changing the consonant of the second word so that they were reversed:

> bibi do
> bibi bo

(The erroneous alternatives were all real words, all the available monosyllables being exploited in the Ewe lexicon.)

There were 20 errors in 304 target trials. In 4 cases (17%) the error appeared in just one of the two syllables of the reduplicated form, and it was in the first or "copy" syllable in each of these cases. They conclude from the statistics that "morphological accomodation to phonological errors is thus the rule in Ewe." A lexical analysis of reduplication "predicts that accomodation to phonological errors would be minimal," whereas "a phonologically dependent analysis of reduplication . . . predicts the relative frequencies of the three types of errors that we actually obtained in the experiment." They explain that the differentiation into three types of errors follows from parallel processing of the base word and its inflection at the same time, "with the inflection (necessarily) lagging behind a little. Phonological errors are assumed to fall on a continuum from eraly to late in phonological processing. Accomodation occurs if the phonological errors happen early in processing (and this seems to be the rule), but not when the error is late in processing (Stemberger 1985)."

> Early in the phonological processing of a reduplicated word, only a single syllable is present, the syllable corresponding to the base word. An early error thus appears on the base word and is copied into the reduplicated prefix. Later in phonological processing, two syllables are present. Since the error induction technique that we used primed errors on the first syllable of the word, late errors would appear on the first syllable, the syllable corresponding to the reduplicated prefix. No late errors were found on the stem only, since we were not trying to induce errors word-internally. These data thus support descriptions of the data in terms of temporal differences.

In CT terms, these "stages of phonological processing" can tell us about the different control systems involved and the relations among them. Perhaps it goes as follows, leaving unspecified for now whether I mean the imagination connection or something else when I say that a control system "is active" without yet controlling perception through changes in behavior:

1. The sequence for the stem (di) is active.

2. The sequence for the following word (bo) is active.

3. The sequence for higher operator(s) Op explicitly asserting the semantics of the participle is active. (This is analogous to reconstructed "in a state" reduced to -en/-ed for English, but may

perhaps be more readily available in Ewe.)

4. The reduplication of the stem is carried out as required under Op, which then being redundant can remain in zero (unpronounced) form.

5. The reduplicated stem is pronounced, and the following word is pronounced.

We might suppose that the control process involved in reduplication is invoked at the wrong time, after (2) instead of or in addition to in (4), to account for two types of error reported here. Or we could suggest that a universal tendency to maintain articulators in the same place over a span of syllables (assimilation) is involved here, as it must be involved in similar slips of the tongue in other languages. On the face of it, this universal tendency (the lenition processes referred to as assimilation) seems to be explicitly invoked in a language involving reduplication.

It appears that the control systems for words are made "active" in the course of constructing a sentence, perhaps with the imagination connection, certainly not with actual pronunciation of the words until the construction of the sentence is at least partly laid out. The facts of intonation and stress indicate that propositions (operator-argument dependency subtrees) are progressively constructed and reduced as higher operators enter, though that is too involved to get into here. Construction of a sentence that begins with reduced forms presumes some anticipatory processing before the first word is actually pronounced. Slips of the tongue and midstream repairs are important sources of data about these processes.

Stemberger and Lewis point out that the data they cite do not enable a choice between available alternative phonological explanations of reduplication, citing Stemberger & MacWhinney (1984) as reaching the similar conclusion that it is very difficult to choose between alternative formulations that are all phonologically dependent. Indeed, even a fully lexical account in which suppletive reduplicated forms are primitive, underived elements is consistent with the data. I think reexamining the issues in a CT framework and devising further experiments bearing on issues of perceptual control would clear that up. But before that we need to establish the fact of perceptual control in phonology and create tools for introducing metered disturbances to control of metered phonological outputs.

My questions about ways of disturbing vowel formants concern one approach to this. I spoke with my father this weekend, a retired radio engineer. He thought it should be possible to do something like the following: filter the selected frequency range, mix the audio with an audio or RF carrier, shift the frequency, remix it with a different carrier, filter the carrier to pick up the audio, and remix the altered audio with the original signal. He resisted my urgings to design this for me, saying he hadn't worked with steep-sided filters. Another problem I foresee is tracking the actual formants produced by the speaker, which are likely not to be so well behaved as I would like. The filter must be narrow enough to capture the target vowel for disturbance and must itself shift to follow the vowel as the person

shifts it, assuming success.

I don't anticipate a lot of problems with intra-cranial transmission of sound, since as I understand it the higher frequencies that we would be disturbing are greatly attenuated there. However, this points up a possible problem with using sideband technology to shift the whole signal up and down, the fact that pitch (the fundamental frequency) is transmitted fairly well through bone and tissue and provides an alternant sensory input that speakers might prefer if the headphone signal conflicts. TBD.

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Fromkin, V. A. 1971. The non-anomalous nature of anomalous utterances. _Language_ 47:27-52.

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Date: Mon, 26 Aug 1991 07:46:07 -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: Robert L. McFarland

[From Bill Powers (910826)] --

Dick Robertson has informed me that last Thursday (Aug. 22), Robert L. McFarland died. Bob McFarland was chief clinical psychogist during my days at the V.A. Research Hospital in Chicago (1953-1960), where Robert K. Clark founded the medical physics department and brought me in as his assistant. Clark and I worked with McFarland for 6 years to develop control theory into a workable model of human behavior. McFarland made many contacts for us in the world of psychology, and carried on clinical tests of control-theoretic ideas. In 1960 he co-authored our first publication. I left the VA Hospital in 1960, and had little to do with the two Bobs after that, so I will leave it to Dick Robertson to write a proper remembrance of Bob McFarland. Bob put a great deal of time and energy into helping control theory along -- 20 years before there was any chance of acceptance. We didn't get along well, a fact that was largely my fault, and I have never properly acknowledged his support and contributions to the initial effort to get control theory off the ground.

At least his name, like Bob Clark's, is firmly associated with the beginnings of PCT and HCT. I am sad that he is gone.

Bill P.

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Date: Mon, 26 Aug 1991 13:59:00 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@VMD.CSO.UIUC.EDU>
Subject: Dues

I just got caught up with some of the e-mail after my vacation and found that I had a question regarding the dues for next year. It was my understanding that the registration fee for the conference in Durango included $25 for next year's dues. Then, we apparently raised them to $40. Does that mean that conference attendees owe another $15? Should we send it to Mary? Or is this to take effect for the following year?

Thanks for clearing this up. I guess Ed as President is the one I am aksing.

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Date: Mon, 26 Aug 1991 16:18:04 EDT
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Misc comments

[Martin Taylor 910826 15:40]
(Bill Powers 910823 09:26)

>
>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.

I think you hooked on "might as well be ..." I did not intend to imply that a control system chooses its reference signal (but see below).

Yes, I accept that only perceptions arrive from the outside world. But those perceptions are transformed in various ways, and when they are used as input to a control system, the transformation must result in something commensurate with the reference signal for that control system. That means that somewhere there is a chain of inverse transformations, which permit each control system to provide for its subordinates (of which there are potentially many) exactly the kind of reference signal that matches the perceptual inputs to those subordinates.

Don't misinterpret this to mean that I see something other than neural impulses as the nature of the reference signal. It could hardly be anything else in the realm of discourse, although in cell chemistry it would be chemical concentration, and (I still think) in social structures it would be something else again--but I don't want to argue about that here.

Even if the reference signal is unidimensional, as often claimed, there is still a requirement to make sure its value corresponds to some abstraction from the incoming data for the control system using that signal. All the models that I have seen discussed show many higher-level control systems supplying referents for each lower-level one, and each higher-level system supplying referents for many lower-level ones. That implies that either the reference signal used by an elemental lower-level control system is an amalgam (average, peak, function) of the higher-level attempts to supply it with a reference, or it is a selection from among the higher-level ones. Whichever it is, the function that determines the reference used must reside somewhere. It might be in the wiring of the lower-level control system, or it might be in the software of the lower-level control system. But it CANNOT be in the higher-level control system, because that system does not have information about what other systems are trying to use the lower-level system at this moment (and possibly not about what other higher-level systems could possibly use it, either).

So, I conclude that there are two requirements that a control system must have, which are usually (in these discussions) glossed over:
(1) There must be some kind of transformational reduction of dimensionality that associates patterns of perceptual input with the reference signal (Rick has discussed low-level versions of this a few times), and
(2) There must be a function, whether fixed or mutable, that determines a one-dimensional reduction of the multiplicity of signals from higher-level control systems into a single reference signal that is compatible with the perceptual input transform.

If the function (2) is mutable, it is likely to be more so at higher levels of the hierarchy (and might be under attentional control). But who chooses any shifts in the relative influences of the different attempted references. I claim that it has to be at least associated with the system for which the references are provided, because that is

the only place in the whole hierarchy that has the particular convergence concerned. If you put the choice of change of function outside the control system being controlled, you are back to the homunculus--a parallel organization that does the work you haven't figured out how to make your primary system do.

OK. I don't mind being picked on, and I KNOW I don't fully understand your model. I think I have convinced myself that a high-dimensional system of parallel unidimensional control systems can ordinarily mimic a high-dimensional control system (the argument hinges on the structural stability of the control surface), but I am not too sure about it. I think I have convinced myself that a hierarchic organization of LINEAR control systems is equivalent to a one-level control system, but I am not too sure about that, either. And I certainly remain to be convinced about the requirement to restrict the top-level control system to residence within one skin bag.

Martin Taylor

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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

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Date: Mon, 26 Aug 1991 16:39:47 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: Social Control?

[Martin Taylor 910826 16:35]
(Tom Bourbon [910822])
> Behavior is not the end result of
>a linear chain of command, wherever that chain is alleged
>to begin, whether in a "stimulus," a neural "command" or a
>social edict.
> In a cooperating group, each individual adopts reference
>signals for her or his own perceptions. Each individual acts
>on the environment to achieve the perceptions requested in
>those reference signals. Living systems cooperate (a) when
>doing so allows each of them to achieve control of perceptions
>that neither system could control when acting alone, or (b) when
>they decide to do so for the sake of doing so -- which allows them
>to control for doing so. In no way does the "cooperating group"
>put reference signals into the head of any member of the group.
>All any member experiences are perceptions. Whether the
>perceptions are even disturbances depends onthe reference
>signals already adopted by the member.

I agree entirely. Substitute "elemental control system" for "individual" or "Living system" in the above, and I still agree.

It is a false comment on my (mis?)perception to say that I think of installing a single reference signal into an individual through a hole in the head. It is a false comment on the standard HCT model to say that an elemental control system is subject to a linear chain of command. (At least I hope it is a false comment; if it isn't, I withdraw my earlier appraisal of the beauty of HCT.)

Martin Taylor

PS. Rick Marken provided a more cogent comment that needs some contemplation before I answer it. He may yet convince me. None of the other comments I have yet seen on this question of social control systems seems to me to have come close to the mark (even Bill P.'s, eloquent though they have been).

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Date: Tue, 27 Aug 1991 07:45:22 -0600
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Inverse transformations

[From Bill Powers (910827)] --

Martin Taylor (910826) --

As I said before, it's harder to tell when someone agrees with you than it is to detect a disagreement. Sorry to put words in your mouth. I think that we're zeroing in on the difficulty (which is as much mine as yours).

>... there is a chain of inverse transformations, which permit each
>control system to provide for its subordinates (of which there are
>potentially many) exactly the kind of reference signal that matches the
>perceptual inputs to those subordinates.

You've put your hand on the weakest point in my model, but you haven't quite found the vulnerable spot to put your finger on it. This is NOT the weak spot:

>That implies that either the reference signal used by an elemental
>lower-level control system is an amalgam (average, peak, function) of
>the higher-level attempts to supply it with a reference, or it is a
>selection from among the higher-level ones. Whichever it is, the
>function that determines the reference used must reside somewhere.

The natural assumption is that one higher control system can set reference signals for many lower ones, but that many higher systems can't simultaneously send signals to one lower system if the higher-level control is to succeed. This isn't true, however, when we stick to control of one-dimensional variables at each level.

The simplest mode of combining higher-level outputs to produce one lower-level reference signal is through addition of excitatory and inhibitory signals where the signals converge on a single lower-order comparator. The sum is the effective reference signal. Clearly, the lower-level systems would have no trouble matching their respective perceptual signals to the net reference signal (assume they're working properly). The apparent problem arises from the fact that no one higher-level system can determine the effective reference signal for a lower-level system, so it can't determine the state of the perceptual signal it receives from that shared lower-level system.

This problem goes away when we realize that the higher-level perceptual signal's value is a function of perceptual signals from many lower-level systems, not just from one. This means that any particular value of the higher-level perceptual signal can be generated by many different combinations of values of the lower-level signals. Control at the higher level does not require just one unique set of lower-level values of perceptual signals. All it requires is that a combination exist that produces a perceptual signal with a magnitude matching the higher-level reference signal -- any of the infinite set of possible combinations. It isn't obvious that the problem now goes away, but it does.

Suppose that there are just two higher-level systems sending reference signals to two lower-level systems. One higher-level system perceives the sum of the lower-level perceptual signals; the other perceives the difference. Skipping over problems of stability, all we have to do now is to take the error signal from each higher level system and send copies of

it to the two lower-level comparators, WITH THE SIGN NEEDED TO PRESERVE NEGATIVE FEEDBACK IN EACH LOOP. The sum-controlling system sends its output to both comparators with the same sign; the difference-controlling system sends its output to the comparators with opposite signs (in the latter case, the lower system providing the positive perceptual input receives a positive reference signal, the other receives an inhibitory reference signal, assuming in all cases that the higher system's comparator subtracts the perceptul signal from the reference signal).

If you make the output functions of both higher-level systems into time-integrators, this arrangement is precisely the analog method for the simultaneous solution of two equations in two unknowns, using a "method of steep descent." No matter what the settings of the sum reference signal and the difference reference signal, at the higher level, the lower level effective reference signals and perceptual signals will come to that pair of values that simultaneously satisfies the sum reference signal and the difference reference signal at the higher level. (!).

(If I hadn't learned computing first on analogue computers, I never would have come across this astonishing bootstrap process. Just another of the lucky breaks that helped to put me on this track. This method for solving simultaneous equations (even nonlinear differential equations) is well-known in analogue computing -- it's the basic method used in simulations, the heart of this kind of computing.)

Of course the net reference signals entering the two lower-level comparators do not correspond to the outputs of either higher-level system. This doesn't matter, because the higher-level system does not perceive the individual lower-level perceptual signals. It perceives only its own function of BOTH signals.

Now expand this idea to many systems at both levels. Clearly, if there are fewer higher-level systems than lower-level systems, the net reference signals at the lower level will still leave unused degrees of freedom: the set of simultaneous equations is underdetermined. This means that the perceptual signals at the lower level are free to vary along trajectories that leave the higher-level signals undisturbed. In fact, if disturbances occur that tend to move them off these trajectories, they will be returned to these trajectories by the concerted error-correcting adjustments made by the higher-level systems. Only the component of the disturbances that tends to move the lower-level vector along a "trajectory of indifference" can still alter the lower-level perceptions without resistance.

So we have found that "chain of inverse transformations" that you were quite correctly looking for. The inverse transformations occur automatically because of the feedback processes; this is where that mysterious missing intelligence comes from. It is not in fact missing from the model; it is only hidden in the way higher-level and lower-level systems interact as they "descend steeply" toward the state that satisfies all higher-level reference signals at once.

In my Byte series of 1979 (?), in Part 3 I think, I laid out a model in which three higher-level systems control a set of three muscles to produce an x-force, a y-force, and a net muscle tone -- each dimension of

control involving all three muscles, and each operating independently of the others. The reason for the existence of muscle tone is revealed clearly by this model: it keeps net excitation greater than zero in all three lower-level systems! That is necessary because control is lost when net inhibition results -- muscles can't push.

Now, what IS the vulnerable spot in the model? It is in the change of type that occurs with each transition from one level to the next. If we stick to one-dimensional control systems in as much multiplicity as required, there is no problem with solving the simultaneous equations (in principle). But the nature of the change of type can introduce new considerations that I don't see a general way of introducing -- or at least I have only a very fuzzy notion of how to do it.

Consider the introduction of "events." Here are included such things as oscillators that produce actions like walking (the motor program people aren't really as wrong as I sometimes claim). How does the output function of an event-control system create an OSCILLATORY output out of a one-dimensional error signal indicating only some (unnamed) deviation of the perceived event-signal from the reference event-signal? The signals being compared are single signals; the reference state is a fixed value. Where do the required oscillations come from? This is the type-transition that is the core of the remaining difficulty: the lower-level system, a configuration-control system, seeks reference-perceptions designating momentary positions.

I suspect that the answer is sitting there somewhere in the vast realms of mathematics of which I know nothing. That doesn't do me much good -- but maybe it will do someone else some good, once that someone else grasps the nature of the problem (and also what the problem ISN'T).

Maybe the answer is simpler than I suspect it is. It could be that by sensing the right combination of integrals and derivatives of lower-level position signals, the event-system as a whole will naturally create the required oscillatory output when the whole feedback loop is considered properly. I haven't been able to figure out how to do that, however -- when matters get beyond a certain level of complexity, my mind goes on strike and declares itself boggled. I also get this urgent message to go take a nap.

I hope this is bringing us closer to mutual understanding.

Best

Bill P.

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Date: Tue, 27 Aug 1991 12:41:08 -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: CSGnet in Limping Mode

[from Gary Cziko]

This last weekend, the machine which manages CSGnet had its operating

system changed (along with oil and filter). As a result, there are a few bugs left to iron out, including the LISTSERV function on which CSGnet depends.

I have been told that the LISTSERV is currently "limping along" and therefore CSGnet is limping as well. CSGnet messages do seem to be getting out (otherwise I wouldn't bother trying to send this one), but perhaps not as quickly as usual and I light amount of traffic over the last few days leads me to suspect there may be a bunch of spooled messages somewhere that will be showered upon us all when some systems programmer comes up with the proper incantation.

I have been told that no messages to CSGnet will be lost during this time, although they will probably be delayed. If you want to be safe, you may wish to postpone sending messages until I announce that all is back to normal. If you do send messages now, be prepared for delays and make sure to keep a backup copy in case of loss. Commands to LISTSERV will also be hit and miss for a while.

I am sorry for any inconvience, but all this is quite clearly out of my control, and I'm sure glad it is.--Gary

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Date: Tue, 27 Aug 1991 13:54:43 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: Joel: training device

[From: Bruce Nevin (910827 1344)]

Joel Judd,

A device such as I have sketched for experiments in phonological control would be useful for second-language training even if it turns out that people fall back on kinesthetic and/or intracranial perceptions when audio in headphones is disturbed. They could be instructed to do whatever it takes to make the audio signal in the headphones sound normal to them. You could then shape the disturbance so that their outputs audible to you through the air sound normal to you. Then get them to attend to what they are doing to accomplish this. Perhaps gradually or in alternation phase in undisturbed audio (one ear vs the other?) so they can hear themselves doing it. Is there money for research leading to development of such a training device, which could also be used for experimental work? I am not in an institutional setting that supports my writing grant proposals of this sort, as mentioned earlier.

Bruce Nevin
bn@bbn.com

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Date: Tue, 27 Aug 1991 10:59:12 -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: Method of steepest descent

[From Rick Marken (910827)]

In his post this morning to Martin Taylor, Bill Powers (910827) describes how control of lower level inputs solves the problem of apportioning outputs to lower order control systems. In his description of two higher level systems controlling the sum and differerence of inputs from two lower level systems, Bill notes:

>If you make the output functions of both higher-level systems into time-
>integrators, this arrangement is precisely the analog method for the
>simultaneous solution of two equations in two unknowns, using a "method
>of steep descent." No matter what the settings of the sum reference
>signal and the difference reference signal, at the higher level, the
>lower level effective reference signals and perceptual signals will come
>to that pair of values that simultaneously satisfies the sum reference
>signal and the difference reference signal at the higher level. (!).
>satisfies all higher-level reference signals at once.

I just want to inject another quick advertisement for my own work. Bill mentioned his two level Byte model. I want to mention that my three-level spreadsheet control model, with four control systems at each level, was designed to show how this "method of steepest descent" works in an even larger hierarchy of control systems. My spreadsheet hierarchy demonstration is described in "Spreadsheet analysis of a hierarchical control system model of behavior", Behavioral Research Methods, Instruments and Computers, 22(4), 349-359 (1990).

Bill goes on:

>Now, what IS the vulnerable spot in the model? It is in the change of
>type that occurs with each transition from one level to the next. If we
>stick to one-dimensional control systems in as much multiplicity as
>required, there is no problem with solving the simultaneous equations (in
>principle). But the nature of the change of type can introduce new
>considerations that I don't see a general way of introducing -- or at
>least I have only a very fuzzy notion of how to do it.

My spreadsheet model does at least provide a moderate feasibility test of "method of steepest descent" for using variables of one type to control variables of another type at a higher level. Perceptions controlled at level 3 of the spreadsheet model seem to be of a different type from those controlled at levels 2 and 1. The perceptions controlled at level 3 are relationships (like A>B). The value of this perception is a logical type variable -- true or false. The lower level perceptions are quantities which are functions of other quantities. Nevertheless, the level three systems are able to set the appropriate

references for the level 2 and 1 quantity perceptions in order to make the relationships they control true or false (as requested by their own reference inputs. I agree with Bill that, when the higher level perceptions involve changes over time in lower level perceptions is difficult to think of how a unidimensional output can be sent to these lower order systems in order to get them to produce the required temporal variations input.

Regards

Rick M.

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

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Date: Tue, 27 Aug 1991 16:52:02 -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: McClelland Paper, 2nd revision

[from Gary Cziko 910827.1645]

Kent McClelland is sending me an electronic version of the second revision of his paper "Perceptual Control and Sociological Theory."

Since many of the "hardcore" CSG group already received a copy of this paper at the Durango meeting, I will NOT send this paper to anyone by default. Therefore, if you want a copy, please send me a PERSONAL note (NOT to CSGnet) to that effect.

Kent's paper is quite long at approximately 160 kilobytes, but in my opinion is it good to the last byte. It provides a very good introduction to perceptual control theory and is especially relevant to the current CSGnet discussion on social control.

Below is a note from Kent which gives more information about the paper.

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[from Kent McClelland]

This paper was presented at the August, 1991, meeting of the Control

Systems
Group at Durango, and it is an extension and revision of a draft distributed
earlier on the CSG net.

Roughly the first half of the paper consists of a general introduction to the
theory of human behavior as the control of perception. Near the beginning is
a fairly complete but very brief review of the literature on control theory.
This is followed by a relatively non-technical description of the structure
and operation of mechanical and human control systems.

The three diagrams referred to in the text cannot easily be transmitted via
CSG net, but the author will be happy to send paper copies of the figures to
anyone who requests them.

The second half of the paper offers some ideas on using control theory to
refine sociological conceptions of power, authority and influence, and the
conclusion of the paper deals more broadly with possible applications of
control theory to sociology.

As I indicated at the Durango meeting, this paper is due for yet another
revision. In particular, the control-theory definition of power offered in
the second half of the paper needs to be extended and elaborated. I would
welcome any suggestions about improvement of the text or elimination of
errors. I am also wondering where one might publish a manuscript like this
one.

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Assoc. Prof. of Sociology Home: 515-236-7002
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Grinnell, IA 50112-0810

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Date: Mon, 26 Aug 1991 14:29:08 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: business meeting notes

from Ed Ford (910826.1430)

Hugh and others: At the CSG business meeting in Durango (held
Friday night after the banquet), it was decided that the dues
be raised to $40 to offset costs of publishing and mailing the
Closed Loop. Since conference participants had already paid
$25 dues, it was decided that the balance of $15 would be
optional for those who attended. The dues (now payable) for
all others for 1992 is $40. Some conference members have
already paid the additional $15.

To those who attended the conference: As requested, everyone

was asked to send between a half page to a page of single spaced copier-ready comments covering what you presented for the next edition of the newsletter, due out in early October, to coincide with the next edition of Closed Loop. Closed Loop is only sent to paid up members. Make $40 dues payable to CSG and mail directly to Mary Powers, 73 Ridge Rd, CR 510, Durango, CO 81301. Mail material for newsletter to me at my address below.

Rick, Thanks for the software on intentionality. Will let you know when my son-in-law Eric has it up and running on my IBM PC.

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

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Date: Wed, 28 Aug 1991 07:26:41 -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: Kent's Paper; Auditory controlled variables

[From Bill Powers (910828)] --

Gary Cziko (910827) --

No apparent problems here, Gary. I haven't noticed any missing transmissions.

I read Kent McClelland's paper at the CSG meeting and I second your opinion. The analysis of power is an important contribution; I'm glad to hear that Kent is going to expand on that subject.

Kent wonders where to submit this paper. I suggest submitting it to the most important mainstream publication in sociology, and then publishing the rejecting referee reports on this net. Clark McPhail and Chuck Tucker might have more productive suggestions.

Bruce Nevin (910827) --

A truly ingenious suggestion for second-language teaching. This cries out for a much closer look at the assumptions of phonology through the Test. Do sound spectrograms truly show the production of four formants (pardon my ignorance here), or are we looking simply at the outputs of band-pass filters? If that latter assumption is true, there's no reason to think that the spectrograms show the auditory controlled variables that human beings actually perceive and control. Why not waveforms? Why not overlapping bands (the ear uses them)? Do sonograms force hypotheses on us? What is their frequency resolution? I realize that I have always assumed that they show what is actually produced -- but the visual result has obviously been pre-processed by some artificial perceptual function. Could the assumptions built into the apparatus be making the task more difficult?

It seems to me that I read somewhere about a PC board that produces

speech by emulating the properties of the vocal tract. Does anyone writing or listening know anything about it? I'm not suggesting controlling output, but such a board might enable us to model control of articulation and then look for those functions of frequencies/waveforms that represent the "natural degrees of freedom" of the articulation-controllers. The constraints introduced by the physical apparatus ought to be suggestive -- for example, I think it's impossible to combine simultaneously the articulations that produce a hard "g" and a "d" or "l", so whatever sound-pattern that combination would make can't be a controlled variable in any language -- the patterns would have to be sequenced, as in "Gdansk" or "glad." There must be many other combinations that are physically ruled out, although their sonograms could be combined. Do we learn what sounds are possible to reproduce by remembering those we have made while practicing articulations? Could speakers of a second language benefit from accumulating experiences of making sounds using new articulations? It seems to me that this would be something like learning the Dvorak typewriter keyboard or practicing scales in strange keys on the piano (I consider everything but the key of C to be strange).

I remember the late discovery that the difference between the "s" and the "z" sound is whether or not you keep your voice turned on. I had always assumed that the tip of my tongue sort of buzzed, because that's how it felt. I discovered this at the age of 20, when I tried out for an announcing job at a college radio station. I flunked the commercial test because I couldn't say "ladies" with a proper "z" at the end, and didn't know how to fix the problem. I realised that I always hissed the final s instead of buzzing it. Microphones really pick up on that. It took a long time to figure out what was wrong. Good thing, too. I might be reading commercials now instead of -- what is it I do now? Ah yes -- typing. No problem with articulation there -- at least I can spell most of the time.

Pardon me for loading down the poor sick Listserver with such trivia.

Bill P.

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Date: Wed, 28 Aug 1991 10:52:05 -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: Joel: training device

[from Joel Judd]

There've been some disturbances to my mail system lately which I haven't felt like counteracting. Yes, I think that an audio/headphone experiment of the type you describe is feasible, probably with some equipment already available here. Certainly more so than changing the gaseous environment! There's at least one faculty member here who has done work in L2 phonology--Gary's going to indoctrinate her in CT first, then talk about phonology. Anyway...

>Then get
>them to attend to what they are doing to accomplish this. Perhaps
>gradually or in alternation phase in undisturbed audio (one ear vs the

>other?) so they can hear themselves doing it.

Part of the trick, I think, in developing experiments along these lines will be to find something that demonstrates fairly unambiguously CONTROL, as Rick has pointed out several times. That's what I'm controlling for too--a demonstration of language as a control phenomena. Of course there'll always be those who can explain away what they see and hear according to what they want to believe. I'm just trying to find the best way to demonstrate the control of linguistic perception. In the case of phonology, a few published experiments have shown that adults (for whom L2-like phonology is supposed to be difficult if not impossible) can produce L2-like (as judged by native L2 speakers) utterances after specific training in producing such utterances. Of course, as in the experiment you outline, they are attending to those utterances--not conversing, not producing novel utterances, etc. This has been one of the accepted pieces of wisdom in SLA; one do almost any aspect of a L2 in a native-like fashion. But once placed in a "regular" setting, where the language is used to some communicative/relevant (to the speaker) end, and the phonology, morphology, syntax, etc. go to pot, as they concentrate on matching their reference signal(s)...
Joel Judd

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Date: Thu, 29 Aug 1991 09:17:28 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: disturbing phonological control

[From: Bruce Nevin (910828 0704)]

Bill Powers (910828)

Good to know our mail traffic is moving, although delayed.

Sound spectrograms are the output of a narrow-band filter swept through a range of frequencies from ~100 Hz to ~4K Hz. Each shift of the filter is coordinated with a shift of an electrical stylus one notch down a rotating cylinder of electrically sensitive paper. Each rotation of the cylinder (one per stylus/filter sweep) is coordinated with one playing of a 2.4 sec tape loop. Greater energy at time t and frequency f results in a darker mark at the coordinates (t, f) on the paper, where f is vertical and t is horizontal when you unroll the paper. (This is based on specs for the original at Bell Labs as reported in 1946 by Koenig, Dunn, and Lacy in an article reprinted in Ilse Lehiste's _Readings in Acoustic Phonetics_). Speech synthesis based on production of formants has been pretty successful, beginning with simply painting them on transparencies to be moved under a photosensitive playback device. Thus, whatever it is that humans control acoustically, it must be captured in the Fourier analysis into energy at different frequencies constituted by a representation as formants.

When you thought what you were controlling for in the contrast of z vs s was "tongue-tip buzzing" it was still laryngeal "buzzing" that you were in fact controlling, together with tongue articulation being more tense

for s, lax for z. The use of words in theories to focus attention can get in the way of control--your point, I take it.

In particular, your point is the excellent one that a representation as formants may not afford us the appropriate variables to control when we want to disturb what a human speaker is controlling. I don't know how to determine that except by experiment--and by keeping our minds doggedly open. This suggests need for sophisticated hardware such as a board with TMS 320C30 chips on it and a programming environment that supports writing and easily modifying programs for dynamic control of arbitrary variables in the digitized signal. Sounds like I do need to get proficient in FORTH (in my spare time). Can't keep going back to programmers in BBN Systems and Technology Devision (formerly BBN Labs) to say "I need another algorithm and program done." Too many grant cycles.

A "PC board that produces speech by emulating the properties of the vocal tract" would be better in obvious ways. I'll post a query to a couple of places.

>Why not overlapping bands (the ear uses them)?

Can someone clarify this, and maybe give a pointer to literature? Does this not specify partly-redundant means by which the ear does Fourier analysis of incoming complex wave forms? (Band n covers from 2040 Hz to 2050 Hz, band n+1 covers from 2045 Hz to 2055 Hz, etc.?) If so, activation of bands covering 2200-2450 Hz but not adjacent bands until the activation of bands covering 225-245 Hz below and 2900-3200 Hz above, would correspond very directly to the formants of the vowel i of "feet" for a particular speaker. The sound spectrogram represents contiguous, non-overlapping (narrow) bands. Do we gain anything besides redundancy from overlapping? How wide are the bands registered in the ear?

I don't know what sonagrams are supposed to represent. I don't know how to recognize complex waveforms except by Fourier analysis as sketched above.

I believe there are languages with velo-dental articulations (g͡d, etc.), though they may necessarily be fully voiced or "prevoiced" stops or even implosives (where the larynx descends with a kind of inverse voicing during stop closure). (English voiced stops are not fully voiced, voicing begins part-way through the oral closure phase.) Certainly there are labiovelars (gb, kw, etc.). But yes, a lot of constraints on what are possible speech sounds follow from articulatory limitations, and others follow from acoustic characteristics of the vocal tract, i.e. restrictions on what is audible and what acoustic distinctions are avaliable for exploitation as terms of contrast in a language.
Some of the latter have been difficult to demonstrate convincingly (influential article by Stephen Anderson "Why phonology isn't `natural'" in _Linguistic Inquiry_ a few years ago argues against many such proposed constraints). Work at Haskins Laboratories seems to me very amenable to restatement in CT terms, e.g.

Browman, Catherine and Louis Goldstein. 1989. Articulatory gestures as phonological units. _Phonology_ 6:201-251 (1989)

Saltzman, E., P. E. Rubin, L. Goldstein, and C. P. Browman. 1987.
Task-dynamic modeling of interarticulator coordination. _Journal of the Acoustic Society of America_ (JASA) 82:S15.

I have read the first of these. Of special interest in the bibliography:

Stevens, K. N. 1972. The quantal nature of speech: evidence from articulatory-acoustic data. In E. E. David and P. B. Denes (eds.) _Human communications: a unified view. New York: McGraw-Hill. 51-66.

Stevens, K. N. 1989. On the quantal nature of speech. JPh 17:3-45.

These have been on my "to read" list for some time now.

Bruce Nevin
bn@bbn.com

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Date: Thu, 29 Aug 1991 07:58:03 -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: f(formant) control (re-send of right file)

[From Bill Powers (910829)]

Bruce Nevin (910829) --

FORTH is known in the programming fraternity as a "write-only" language. I once did a lot of programming in it, and they were right. It's just lovely at first, because you make up FORTH "words" that accomplish nifty subroutines, and they're immediately available for use interactively.You can try out your routines as you go. Also it produces fast-running programs. But six months later when you look at what you wrote it has mysteriously turned into gobbldegook -- you can spend hours trying to figure out what you meant. Nobody else, of course, can read your programs.

I recommend C or Pascal, which are at least understandable to someone else (if you don't get too cute). I'm just getting back up to speed in C, translating some old Pascal programs, and while it's as annoying as ever (it lets you do wierd wrong things without complaining) it is also very fast-running. Even Quick Basic or Turbo Basic is now a reasonable (and easy) language, compiling to reasonably fast run-time code. But of course a compiler for any of these languages has to exist on your machine.

The reason I would like to see a hardware model of the articulation-based speech generator is to see how COMBINATIONS of formants change with the easiest natural movements of tongue, lips, and jaw (the natural degrees of freedom I mentioned). I suspect that these functions, not the formants themselves, will prove closer to the actual controlled variables. Your very clear explanation satisfies me that sonograms are true spectra (I hope they don't do them with tape loops any more!). So we need to look for functions of the formants that are resistant to disturbance. I'll

hold your coat while you figure out how to disturb them.

Best

Bill P.

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Date: Thu, 29 Aug 1991 13:45:57 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: variables to tinker with

[From: Bruce Nevin (910829 1225)]

Bill Powers (910829)

>FORTH is known in the programming fraternity as a "write-only" language.
>. . . six months later when you look at what you wrote it has
>mysteriously turned into gobbldegook -- you can spend hours trying to
>figure out what you meant. Nobody else, of course, can read your
>programs.

C is also often called a write-only language too, with the same criticisms about unreadability and obscurity.

This is a function of good programming practice, which it is easier to violate in some languages (C, FORTH) than in others (ADA, other allegedly "straitjacket" languages).

I have no doubt you are very well aware of the issues. I have no intention of lecturing, but for expository purposes it needs to be stated that no language is "self-documenting" and that bad programs can be written in any language. The difference between a hacker and a programmer is said to lie in design. The coding should be a translation of the design, with easy portability to another language as one consequence. In structured programming design, one uses just a very few control-structure constructs. One should use words with mnemonic value. And one should provide prolific documentation of the meanings and uses of all words and constructs, with the comments in the code and the description of intent and implementation in a design document correlated with each other. This applies to C and to Pascal and to any other language.

I gather that it is especially easy to abuse FORTH for the same reason that it is especially easy to prototype an application quickly, namely
>you make up FORTH "words" that accomplish nifty
>subroutines, and they're immediately available for use interactively.
>You can try out your routines as you go.
And you don't have to wait through the code
compile link run debug compile etc. loop, it's more wysiwyg than batch mode.

Common philosophy, which I believe in, is that when you have finished prototyping and you know what you want (have relevant reference values) then you should throw the prototype away and do it right. No reason

that couldn't be done in FORTH, C, or any other desired language.

Glad you're familiar with FORTH, in case I actually do get time to get into this.

> functions of the formants that are resistant to disturbance.

I just attended a talk by a French man just coming off a postgrad program at U Ariz and returning to Paris, Emmanuel Dupoux. He described work concerning units of speech perception. He reviewed experiments suggesting people perceive syllables first, then segments. He presented results that called this in question, without ratifying a segment-first theory. He proposed some sort of "semi-syllables" as the primary units in speech processing.

The general methodology was to demonstrate differences of response time recognizing various targets in spoken lists of words (e.g. segment b, CV sequence ba, CVC sequence bal, etc. in French words balance vs balcon). In some cases people could be pressed to recognize segments and the longer recognition time in more complex syllable structures (the syllable effect) went away. A lexical effect was also found comparing words with nonsense syllables, but only for simple monosyllables (for obvious reasons). I can reconstruct more detail from notes if there is interest.

My conclusion on questioning Dupoux is that the elements he is looking for in his "semi-syllables" are the CV and VC transitions. Surprise, surprise. It is from these that we determine the adjacent C configurations. But some reservations about the adjacent V configurations that I'll get back to.

Thus, the initial part of a vowel formant after a b curves to point toward 0 Hz for F1 and about 600 Hz for F2. If the following vowel is i, F2 rises steeply to about 2400 Hz and F1 to about 240 Hz. If the following vowel is a, F1 rises to about 600 Hz and F2 to about 1800 Hz. These targets for departure from b approximate the formant frequencies for u. The targets for departure from d approximate the formant frequencies for a front, unrounded vowel, say, the e of red. There is of course a direct articulatory correlation.

The big problem with synthesized speech, and a principle reason it sounds artificial, is what are somewhat perversely called coarticulation effects. I say perversely, because the term suggests the control of behavioral outputs. These effects are the consequence of the interaction of different articulators and of the transition from one articulation to another for any given articulator. They are part of the environmental effects for the control system.

The CV transitions should I suppose be called coarticulation effects also, but they are not because they are in fact the only cues we have in the acoustic signal for distinguishing one stop consonant from another--the silence for t is indistinguishable from the silence for k or for p.

One problem in my mind with the scheme for shifting vowel formants is that it might do strange things to consonantal transients. If the

shifts are relatively slight (i to e or u to o as proposed) this might not be a difficulty. But in the long run we would like to tinker with consonant articulations as well. I see no way to do that without programming a chip and working with the digitized signal.

We would like to think of the vowel formants as steady state until the next consonant transition, but they are in fact often not so well behaved in normal speech ("coarticulation effects"). So even for vowels the filtering and mixing scheme my father suggested might just be too broad and inflexible a brush.

Hence the reservations mentioned. In DuPoux' review of the literature and of his own work no one did any experiments where the target for recognition was just a vowel. I am not so easily convinced that we perceive vowels as configurations after perceiving the CV transitions. "Aha" has no formant transitions, only the transition of voicing on and off (with a voiceless vowel a in the middle for the h).

Someone suggested that the CT model might admit of both orders (transition first or configuration first) in different parts of the hierarchy. Is this a case where the two orders are very close together in the hierarchy? That the control sub-hierarchy for recognizing the b of balance goes from the transition to the configuration, while the parallel subhierarchy for recognizing the a goes from the a to the transition? This suggests a vowel-first (or more generally syllable-nucleus-first) perceptual process. That would accord well with what is called dependency phonology and would nicely parallel the perception of lexical dependency in operator grammar.

Open questions.

Lunch is over. Back to work.

Bruce
bn@bbn.com

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Date: Thu, 29 Aug 1991 13:22:30 -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: CSGnet Status

[from Gary Cziko 910829]

Although the LISTSERV@UIUCVMD on which CSGnet depends is still limping badly and "is not to be relied on," messages still seem to be getting through, at least to me. However, I have received some returned mail from such dependable nodes as those manned by Rick Marken and Bruce Nevin and so apparently all mail is not getting through to all people. I still have no firm date on when things will be back to normal.

I suggest that we go on as if things were normal (although perhaps at a slightly less frantic pace), keeping backup copies of mailed messages. If you feel you missed a message or two, make a note of the date and you can always get the log file for that week (assuming THAT is working normally).

Also note that for some strange reason, whenever I as listowner send commands to LISTSERV, LISTSERV thinks that I am POWERS Bill while in fact I am most certainly not. CZIKO@UIUCVMD is me and not Bill, not matter what LISTSER says. If anyone out there knows how I can convince LISTSERV of this, please let me know.--Gary

Gary A. Cziko
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FAX: (217) 244-0538
Internet: g-cziko@uiuc.edu
Bitnet: cziko@uiucvmd

Date: Thu, 29 Aug 1991 17:10:37 EDT
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: disturbing phonological control

[Martin Taylor 910829 16:30]

Bruce Nevin, Bill Powers, etc.

A little auditory tutorial:

The ear does not do a Fourier analysis at all, although it does permit the hearer to distinguish among different frequencies. Just what it does do is a research question, but one agreed "fact" is that its resolution in time is good at high frequencies, and its resolution in frequency is good at low frequencies (you can't have both together, for fundamental mathematical reasons). Fourier analysis (and most versions of LPC=Linear Predictive Coding) gives the same resolution in time and frequency at all frequencies.

When you talk of the ear analyzing in overlapping frequency bands, you probably are implicitly talking about the individual sensory cells (hair cells) that detect the motion of the basilar membranes, which vibrates in response to the incoming sound. Each hair cell has a particular pattern of response as a finction of frequency. There is a frequency to which it is most sensitive, and its sensitivity drops very rapidly as the frequency deviates from that central frequency, although it retains some sensitivity even to widely deviant frequencies. There are interactions among hair cells, or at least among the responses from neighbouring hair cells fairly low in the nervous system, such that one that responds strongly may suppress the response from a less sensitive neighbour. There are about 30,000 of these hair-cell channels, and to a first approximation each is around 1/6 octave wide, so there its lots of overlapping among their frequency responses.

When you go a little further up the auditory system, you have a related pattern of sensitivities that is often referred to as "critical bands." A critical band is usually determined by either physiological or psychophysical experiments that determine how much one tone masks another of a nearby frequency. If the two tones are in non-overlapping critical bands, they do not interfere with each other; of course, the

bands not being all-or-none, such a situation does not actually occur, and there is always some cross-masking. So the critical band is often measured in terms of an equivalent rectangular band that would accept the same amount of signal from a wide-band noise as does the actual critical band.

The measurement of critical bands is somewhat affected by the experimental technique. I have a long-standing difference of opinion on this with one of my colleagues. I believe that measurements of a critical band that determine how far apart two tones must be before they fail to augment one another give results about 1/2 the width of measurements that determine how close two sounds must be before they interfere with one another. But in any case, critical bands tend to be around, say, 0.1 - 0.2 of their centre frequency (from memory--don't take the factor as gospel), but are constant at around 80Hz or so for centre frequencies below 500 Hz.

Various people have attempted to develop auditory models, either as computational tests of ideas as to how the auditory system works, or as front-ends for speech recognition systems. We have here a model developed by Roy Patterson at the MRC Applied Psychology Unit in Cambridge, England. I think it is the most realistic model available today, and we are porting it to a set of processing boards based around the TMS 320C30 chip (3 per board, 3 boards) that are being developed for us. The idea is to do real-time production of what Roy calls the "stabilized auditory image," a 2-D presentation that is claimed to show all that a person can hear, and to hide all that a person cannot hear. We wish then to use this image as a front-end to a recognition system, and to use it as a canonical form that could be distorted and turned back into speech.

Formants:

The basis of a formant is very simple, but the analysis of the set of formants is not. A formant, whether in a voice or a musical instrument, is simply the effect of a resonant filter, usually a cavity, on a sound. For example, a pure buzz is represented in time by a series of zero-width impulses. In frequency space, it is a set of lines equally spaced in frequency. When such a buzz is passed through a resonant cavity, the height of the lines near the centre frequency of the resonance is increased relative to the height of the lines well away from the resonant frequency. The time waveform ceases to be an impulse train and becomes a regular pattern of some repeated waveform.

In the human voice, formants are caused by the several cavities in the path from the vocal cords to the outer air. Some of these cavities are under voluntary control, some (like the nasal passage) are not. Most of the control is effected by movements of the tongue, but aspects are controlled by the lips and the jaw opening, and perhaps a few other things. These resonances are not acoustically independent, since the cavities are coupled, and are not simple, because the flesh is to some extent elastic. Sound probably bounces around within the skull and sinuses as well, affecting the formant structure. I don't know the physical details, but the result on analysis is quite complex.

Programs for tracking formants exist, but are still a research topic, because they are never infallible. The better ones work on an a prior assertion that there exists a certain number of formants, and that the frequency of a formant does not vary dramatically up and down over very short times on the order of 10-20 msec. Changes are assumed to be smooth. When you look at a spectrogram, or, much better, the stabilized auditory image, you don't always see the same number of formants. Sometimes you seem to see four, sometimes six or three. But we are taught that there are four (or sometimes that there are five) of which the first three (having the lowest central frequency) vary under voluntary control to form the continuant sounds of language, and the first two are all that really matter. Synthesis programs usually control the first two formants and keep the others constant. I think that may be one reason they don't sound natural, because you can certainly see the upper formants shifting, whether they are under voluntary control or not.

Sorry for the long non-control didaction, but I really think that the digital version of the proposed experiment is more difficult than you realize, and your discussions of the nature of the speech signal seemed to need some comment.

Martin Taylor

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Date: Fri, 30 Aug 1991 00:07:00 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: Instruction

I have been a passive member fo this group for about three months and am quite interested in the topic. Not that I understand a whole lot of what you are saying. One of the topics I have followed with great interest is "social control." There seems to be many different opinions on the subject. I am a doctoral student in curriculum and instruction (9 hours and a dissertation to finish) and wonder how "instruction" might be related to social control. I'm not completely sure of what would be a good definition of instruction. How does it (if at all) control our perceptions? I think I might understand control
theory better if it were related to a topic in which I knew something about. Instruction must exert some kind of control, I would love to hear control theory as it relates to instruction. I would also like to hear a definition of instruction from the control theory angle.

Thanks a lot...
Jack DeGolyer [JHDCI@UNO.EDU]

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Date: Thu, 29 Aug 1991 17:33:27 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: variables to tinker with

[Martin Taylor 910829 17:15]
(Bruce Nevin 910829 1225)

>I just attended a talk by a French man just coming off a postgrad
>program at U Ariz and returning to Paris, Emmanuel Dupoux. He described
>work concerning units of speech perception. He reviewed experiments
>suggesting people perceive syllables first, then segments. He presented
>results that called this in question, without ratifying a segment-first
>theory. He proposed some sort of "semi-syllables" as the primary units
>in speech processing.
>

Yes, demisyllables are one very useful unit in speech recognition (and
synthesis). I once had, but have somewhere mislaid, a Bell Labs list
of some 1600 (I think) demisyllables of English. I wonder what DuPoux
had that was new?

>My conclusion on questioning Dupoux is that the elements he is looking
>for in his "semi-syllables" are the CV and VC transitions. Surprise,
>surprise. It is from these that we determine the adjacent C
>configurations. But some reservations about the adjacent V
>configurations that I'll get back to.
>

Yes, the transitions are important. Some 15 or so years ago Louis Pols
did some experiments to see just how much of the transition mattered
in the recognition of the consonant (and, I think, the vowel). He masked
X msec of the start of a CV transition or the end of a VC (and perhaps
vice-versa as well?), and found that consonant information persisted
well into what might be thought of as the steady state part of the vowel.
I don't remember the details--it was a long time ago. The basic result
was that the consonant holds enough information to identify the vowel,
and vice-versa.

>
>
>The CV transitions should I suppose be called coarticulation effects
>also, but they are not because they are in fact the only cues we have in
>the acoustic signal for distinguishing one stop consonant from another--
>the silence for t is indistinguishable from the silence for k or for p.

But the bursts are not indistinguishable, at least not in French. Pierre
Alinat, at Thomson-CSF, has studied the evolution of the burst, and finds
quite different centre frequencies, bandwidths, and movements of the
centre frequency (and bandwidth?) for the different stop consonants. I
would be surprised if the results were different in English, since the
release mechanisms for the different stops are quite different.

>Someone suggested that the CT model might admit of both orders
>(transition first or configuration first) in different parts of the
>hierarchy. Is this a case where the two orders are very close together
>in the hierarchy? That the control sub-hierarchy for recognizing the b
>of balance goes from the transition to the configuration, while the
>parallel subhierarchy for recognizing the a goes from the a to the
>transition? This suggests a vowel-first (or more generally
>syllable-nucleus-first) perceptual process. That would accord well

>with what is called dependency phonology and would nicely parallel the
>perception of lexical dependency in operator grammar.
>

That was me. I pointed out that the transitions convey information that
is independent of that conveyed by the steady states (configurations?).
In a principal components analysis of the filter-bank analysis of speech,
the components that represent spectral time-differences look very much
like those that represent spectra, and are interleaved with them in
importance. I take that to mean that any recognition scheme should
treat both configuration and transition as being of equal and parallel
importance.

Martin Taylor

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Date: Fri, 30 Aug 1991 11:03:09 CET
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Chuang Gu <cgu%cernapo.cern.ch@VMD.CSO.UIUC.EDU>
Subject: Re: CSGnet Status

I failed to signoff CSG-L. Please help me.

cgu@cernapo.cern.ch

thanks a lot.

Chuang GU

==========

Date: Fri, 30 Aug 1991 09:02:21 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: AN ANNOUNCEMENT FOR A MEETING

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, 30 Aug 1991 07:53:18 -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: Auditory stuff

[From Bill Powers (910830)]

Bruce Nevin (910829) --

I still think that FORTH is the ungreatest among the ungreat, probably because I don't speak German and like to have verbs up front. Deciphering 1 valve dup pi - swap > if . (I'm a lot out of practice so that's probably meaningless but not untypical) is not my idea of a language one can learn to read swiftly. The HP35 calculator had about the maximum stack depth I am comfortable with. I don't love C or Turbo Pascal, either, but at least I can tell sense from nonsense without deep thinking. I agree with you about comments. Mine are substandard.

Re: articulation and coarticulation effects.

>One problem in my mind with the scheme for shifting vowel formants
>is that it might do strange things to consonantal transients.

In figuring out how to disturb heard speech, it isn't important to isolate dimensions of control and disturb them one at a time. All that matters is that the disturbance alter the controlled variables in directions that can be counteracted by feasible changes in articulation. The disturbances can be quite small: the object is not to overcome the person's corrective changes in articulation, but to allow them to succeed. I'd like to see what happens to sonograms when a person's lips and jaw are fixed and the tongue alone moves, and so on -- these changes would be easy to counteract using natural movements.

One way to disturb formants would be to use the old loudspeaker-in-the-mouth trick, with the person not providing the voicing (as in speech aids for laryngectomy victims). Feedback from an external microphone through a tuned filter could effectively alter the cavity resonances of the mouth (this might even work WITH voicing). Don't forget the wonders of the analog world. You can do things in real time with analog devices that would require a Cray to do in digital form.

Re: syllables vs. segments:

>Is this a case where the two orders are very close together
>in the hierarchy?

I'm not sure what "close together" would mean. Ordering in the hierarchy is a matter of dependency: what has to be controlled in order to control what else; what perception is built out of what other perceptions. If you mean by "close together" the absence of intervening levels, I would probably agree.

My tests would go like this: it is possible to alter a segment without altering a syllable? If not, segments are of a higher level. Is it necessary to alter a syllable in order to alter a segment? If so, segments are of a higher level. Can syllables be altered without altering a segment? If so, segments are a higher-level invariant drawn from many alternative lower-level syllables.

>... the control sub-hierarchy for recognizing the b
>of balance goes from the transition to the configuration, while the
>parallel subhierarchy for recognizing the a goes from the a to the
>transition?

That would be messy, and what would control which ordering of levels would be used? I think you're talking about event-perception, where it isn't so much ordering per se (sequence level) as temporal pattern (event level) that is recognized. I don't know. Your thoughts on these matters are exploring virgin territory.

Gary Cziko (910829) --

I'm not Gary Cziko, either. I am actually a virus that has got into the Listserver. I can make it do anything I want. I control the horizontal. I control the vertical. I control everything. You're next.

Martin Taylor (910829) --

Fascinating information; thanks for a most informative tutorial.

>Just what [the ear] does do is a research question, but one agreed
>"fact" is that its resolution in time is good at high frequencies, and
>its resolution in frequency is good at low frequencies (you can't have
>both together, for fundamental mathematical reasons). Fourier analysis
>(and most versions of LPC=Linear Predictive Coding) gives the same
>resolution in time and frequency at all frequencies.

This sounds like the operation of a bank of tuned active filters, which would not behave like either Fourier analysis or LPC (from the little I know of the latter). Tuned filters can be realized using a combination of leading and lagging negative feedback, coupled with an adjustable amount of positive feedback that adjusts the "Q" of the circuit. As the positive feedback is increased, the filter resonates more and more (the output amplitude increases) to the same input and its bandwidth (as a percent of the center frequency) decreases. If the bandwith remains a constant fraction of the center frequency, the time-resolution of a high-frequency filter naturally is good because the "Q" determines the amplitude-squared loss PER CYCLE, and cycles take less time at high frequencies. Time resolution depends on how fast the output amplitude of a filter can follow input amplitude changes, and that in turn depends on Q and center frequency. I suppose you know all this.

An active filter with too much positive feedback turns into an oscillator: tinnitis.

A bank of tuned filters has a nice property, nervous-system-wise, in that it turns a complex temporal waveform into a spatial map. You end up with a set of parallel signals which can then be processed just as if they were an image (one-dimensional).

>When you talk of the ear analyzing in overlapping frequency bands, you
>probably are implicitly talking about the individual sensory cells (hair
>cells) that detect the motion of the basilar membranes, which vibrates
>in response to the incoming sound.

This tuning is too broad to permit us to get, say, one signal per note of the standard scale. There must be another layer that sharpens the tuning. Maybe this layer has adjustable tuning (through reorganization) -- it would almost have to to fit the various kinds of musical scales that people learn to hear as "natural" (Western, India-n, Eastern 5-tone).

The auditory system, it seems to me, is unique in the brain because it manages to represent frequencies by the use of neural signals that are themselves frequencies of firing. Audible sound frequencies cover the range over which neurons can fire. At the lowest levels, the frequency of a perceptual sound signal IS the frequency of firing, isn't it? But at some point, some level, this has to change: a given perceptual signal must represent *the fact that* a given frequency is present, and the frequency of firing of the neuron must then represent the amplitude of the signal at that frequency -- the envelope. I think this would happen at the level where my hypothetical bank of active tuned filters is. The output of the filter would not be a sound frequency any more, but a neural signal whose amplitude (frequency) represents the amplitude of response of the filter. The signals at and above this level would vary in frequency at a rate far lower than the individual cycles of a sound-wave, because now they represent something ABOUT that sound wave, an abstraction concerning its form.

This is confusing because there is a switch from one-to-one correspondence between sound-wave crests and neural blips to an envelope-type response -- but the coding is still in terms of frequency of firing.

So when we hear a trill on a piano or in a voice, the perception must be that of a rapidly-varying frequency of firing of a neural signal in which there are many neural impulses per cycle of the trill. This is a much higher-level perception. It's interesting that our ability to perceive cyclical variations at this higher level cuts off when the cyclical variations rise to the speed of the LOWEST normal frequencies of neural firing (10-15 per second or so). This is very, but maybe not completely, confusing.

Too much more in your post to comment on by sending-time. I should soon have a logon at Ft. Lewis College (Roger Peters, joint appointment in psychology and computer science, says "no problem"). This will eliminate my long-distance costs. Then I will have to made a decision: do I spend all day on the net, or (1) preserve my marriage, (2) get some programming done, and (3) keep entropy away from the house and yard?

I have an A/D converter and will get a microphone. I want to try emulating that tuned-filter idea. Also I have a hunch that my adaptive transfer-function algorithm might do something interesting to sound waveforms. Shut up and send this, Powers.

Best

Bill P.

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Date: Fri, 30 Aug 1991 11:35:17 -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: CSGnet Status

Gary (910829)

>I suggest that we go on as if things were normal

Ignoring error signals can lead to deep-seated psychological problems, can't it?

>Also note that for some strange reason, whenever I as listowner send
>commands to LISTSERV, LISTSERV thinks that I am POWERS Bill while in fact I
>am most certainly not. CZIKO@UIUCVMD is me and not Bill, not matter what
>LISTSER says. If anyone out there knows how I can convince LISTSERV of
>this, please let me know.

Sounds like LISTSERV is controlling for something else...

Joel Judd

==========

Date: Fri, 30 Aug 1991 11:52:19 -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: Auditory stuff

Bill (910830)

>I should soon
>have a logon at Ft. Lewis College (Roger Peters, joint appointment in
>psychology and computer science, says "no problem"). This will eliminate
>my long-distance costs. Then I will have to made a decision: do I spend
>all day on the net, or (1) preserve my marriage, (2) get some programming
>done, and (3) keep entropy away from the house and yard?

Have you talked to a guy named Ed Ford? I've heard he's good with these kinds of decisions and works from a CT framework as well. In fact, I believe he's on the net.

Joel Judd

==========

Date: Fri, 30 Aug 1991 13:43:04 -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: Paper: PCT and Ed. Research

[from Gary Cziko 910830.1330]

In a few days I will be ready to disseminate a draft of a paper of mine. Its title is "Human Behavior as the Control of Perception: Implications for Educational Research". Compared to other works recently made available to CSGnetters, it is relatively short at only 67 kilobytes in length.

In 1989 I published a somewhat controversial article in _Educational Researcher_ with the somewhat overblown title "Unpredictability and Indeterminism in Human Behavior: Arguments and Implications for Educational Research." In 1990 _Educational Researcher_ published a critique of my essay by Lehrer, Serlin and Amundson but due to the confusion of a change in editorship I was provided no opportunity to respond at that time. This draft is a belated rejoinder to Lehrer et al.

When I wrote my original 1989 essay I had no knowledge of perceptual control theory and many of the issues I raised take an entirely different light when seen from the PCT perspective. Since I cannot expect readers of _Educational Researcher_ to know anything about PCT, I have provided a concise introduction to PCT, much of which is similar to what appears in Chapter 5 of my book in progress which I have already made available to CSGnetters. Since considerable time has elapsed since the publication of my 1989 article and Lehrer et al.'s critique, I have had to review my original arguments and their criticisms. Therefore, the draft stands on its own fairly well with knowledge of the preceding two papers helpful but not necessary.

For PCT oldtimers, there is probably not much in the way of exciting new insights in my paper (although they may find of interest how PCT makes problems of quantum physics and chaos seem much less relevant for behavioral research), but it is just this group of people who can provide me with useful comments and criticisms for which I will be very grateful. Newcomers to PCT should find the draft of more interest. Since _Educational Researcher_ is probably the most important journal for

theoretical discussions of educational research, I find quite appealing the prospect of publishing an article in this journal on PCT.

According to the convention for distributing papers to CSGnetters, I will send a copy by default to all those on the hardcore list of:

Bourbon, Delprato, Ford, Forssell, Goldstein, Harnden, Hershberger, Joslyn, Lubin, Malcolm, Marken, McClelland, McPhail, Nevin, Powers, Roberts, Rodrigues, Sabah, Talmon, Taylor, and Tucker.

If you are on this list and do NOT want a copy, please let me know via a personal message within a day or two. You should also send me a personal message if you are not hardcore but would still like to receive a copy. Finally, let me know if you would like harden or soften your core status on CSGnet.--Gary

References

Cziko, G. A. (1989). Unpredictability and indeterminism in human behavior: Arguments and implications for educational research. _Educational Researcher_, _18_(3), 17-25.

Lehrer, Richard., Serlin, Ronald C., & Amundson, Ronald. (1990). Knowledge or certainty? A reply to Cziko. _Educational Researcher_, _19_(6), 16-19.

==========

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Date: Fri, 30 Aug 1991 15:02:51 -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: pp.

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

==========

Date: Fri, 30 Aug 1991 19:06:23 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: more sound stuff

[From: Bruce Nevin (910830 1904)]

(Martin Taylor 910829 16:30)

An excellent, informative, and very thought-provoking post--Thanks, Martin! I will spend some time this weekend looking at hard copy and the few references I have at home.

(Martin Taylor 910829 17:15)

I have no handout and no reading list from Dupoux, but his second, revised abstract
cited references which I take it must be familiar names to those active in the field. The two abstracts:

> CROSS-LINGUISTIC EFFECTS IN SPEECH PROCESSING
>
> EMMANUEL DUPOUX
> Cognitive Science Department, University of Arizona
> dupoux@arizvm1
>
>
> BBN, 4th floor large conference room
> 10 Moulton St, Cambridge MA, 02138
> Thursday August 29th, 1990, 10:30 AM
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>Speeded segment detection experiments are used to compare and contrast
>various languages: French, Spanish, Catalan, English, Japanese, etc.
>The aim is to establish the role of the syllable or other units during
>the early stages of speech comprehension as a function of the
>phonological characteristics of the language.
>
>[Revised abstract ...]
>
>Recent studies have outlined 1) the role of "coarse grained"
>constituants (like syllables) in speech processing (Mehler, 1981;
>Mehler et al , 1991) and 2) the fact that the units used to process speech
>might depend on the phonological properties of the language
>(Cutler et al, 1987, 1989, 1991). We review the empirical
>evidence that favours the syllable as a processing unit
>and present some more data that qualifies a strong
>version of the syllabic hypothesis. It is found that
>the perceptual system can use information that spans
>over a range smaller than a syllable, maybe corresponding
>to semi-syllables. It is also argued that the "coarse grained"
>constituants are only build at a later stage and that they
>correspond rather closely to what phonologists describe as
>"surface syllables".

Don't know what to say about bursts without data except yes of course, should have thought of bursts. For voiced stops perhaps there is enough of formants during the voiced phase, certainly for nasals there is.

Bill Powers (910830)

Yes, analog tools look better and better, else we must pull just the right combinations out of the digitized signal, and that looks increasingly to be forbiddingly complex even for a single, stably repeated signal, much less dynamically in real time.

>In figuring out how to disturb heard speech, it isn't important to
>isolate dimensions of control and disturb them one at a time. All that
>matters is that the disturbance alter the controlled variables in
>directions that can be counteracted by feasible changes in articulation.

I guess I'm looking ahead, past demonstrating the reality of control to using control to isolate the perceptual parameters under control. That, I think, will be revolutionary in phonology.

>The disturbances can be quite small: the object is not to overcome the
>person's corrective changes in articulation, but to allow them to
>succeed.

I do know that.

>I'd like to see what happens to sonograms when a person's lips
>and jaw are fixed and the tongue alone moves, and so on -- these changes
>would be easy to counteract using natural movements.

I don't know how sonagrams are made. I do know there is a fair amount of software and hardware for speech analysis available. I think I mentioned a survey of these by Read, Buder, and Kent in _Journal of Speech and Hearing Research_ 33:363-374 (June 1990). I have a reprint that Charles Read sent me, and could send a photocopy on request. I intend to talk with Makhoul here at BBN after the holiday weekend.

>One way to disturb formants would be to use the old loudspeaker-in-the-
>mouth trick, with the person not providing the voicing (as in speech aids
>for laryngectomy victims). Feedback from an external microphone through a
>tuned filter could effectively alter the cavity resonances of the mouth
>(this might even work WITH voicing). Don't forget the wonders of the
>analog world. You can do things in real time with analog devices that
>would require a Cray to do in digital form.

Please educate me on this. I tried placing a vibrator against the outside of my throat and simply breathing through the articulated oral passage, but clearly that is not enough vibration, or enough of the right sort. The larynx actually interrupts the air stream, of course. I am wide open to suggestions about analog devices, especially ones that any reader might reasonably be able to replicate.

>>Is this a case where the two orders are very close together
>>in the hierarchy?

>I'm not sure what "close together" would mean. Ordering in the hierarchy
>is a matter of dependency: what has to be controlled in order to control
>what else; what perception is built out of what other perceptions.

That is exactly what I mean. Let me give some of Dupoux's examples in

somewhat greater detail.

When instructed to press a button whenever participants hear b, the length of time it takes them to respond is a function of the complexity of the syllable of which b is the initial segment. This indicates that something following the b has to be controlled in order to control the b segment. The main "something" appears to be the transients (burst plus upcurve of formants) that come between the b and the following vowel.

Length of time for recognition in one study increased roughly linearly for syllable complexity (CV, CVC, CCV, CCVC, CVCC, and finally disyllabic CVCV). These data suggest syllable as basic unit of perception. French ba is recognized faster in balance (CV-CVC) than in balcon (CVC-CV), but bal as target is recognized faster in balcon than in balance. The effect is even stronger in Spanish than in French. In another study, a consonant was recognized faster in a given syllable-structure position if the accompanying words in the list were biased (80%) to having C in that structural position. Thus, with a bias to first C of CC cluster being in syllable onset of second syllable, the /k/ phoneme of tacron (in onset of second syllable) was recognized faster than the /k/ phoneme in tactic, and the reverse with a bias to the first C of the cluster being in the coda of the first syllable, as in tactic. (English is problematic because in proposed pairs of words a consonant in this cluster position is always ambiguous as to syllable structure, some call them ambisyllabic, in one word of the pair. There is also a problem of stress and vowel reduction. An example is climax : climate, where the m of the latter is ambisyllabic and the a is reduced.)

If pressed to respond as quickly as possible, people can recognize the segment without any differentiation as to the complexity of the rest of the syllable that it initiates.

I surmise that this is in part a consequence of parallel processing, and that we normally wait for the "votes" of segment, semisyllable, and syllable recognition processes to come in before responding that we recognize an initial segment. I get this from two facts. If pressed, we can respond on the basis of segment recognition alone. For monosyllabic CVC words vs nonsense syllables, there is a lexical effect, where word recognition comes in before syllable processing.

I observed that a vowel alone was not a target in any of these studies. I wonder whether vowel recognition is dependent upon syllable structure. I guessed that vowel recognition goes from configuration to transition. Stop recognition seems to go from transition to configuration. But syllable recognition involves both consonants and vowels, and that is why I said the two were "very close together in the hierarchy".

But I see that it is an open question, what syllable recognition might be based on. People at this talk were concerned about the number of syllable-recognizers required, many more than the 40 or so for English phonemes. But we know we can be quite profligate with neural control systems. So I'm up in the air on this one.

Got to run for the train. Late night tonight. I'm likely to be more

taciturn starting next week, as the heat will be on with some work I've been neglecting more than a little.

Martin: an article of possible interest: New Scientist 29 June 1991 p. 57, review of _Natural theories of mind, evolution, development and simulation of everyday mindreading_, edited by Andrew Whiten.

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Date: Fri, 30 Aug 1991 20:53:22 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: priorities

from Ed Ford (910830.2055)

Bill (910830)

>Then I will have to make a decision: do I spend all day on the net, or
>(1) preserve my marriage, (2) get some programming done, and (3) keep
>entropy away from the house and yard?

Bill, several months ago you stated that you were questioning my concept of priorities, especially at system concepts level, within the PCT framework. I was delighted (purely from an instructional point of view, naturally) to learn of your conflict and your attempt to deal with just that kind of experience, that is, establishing and then evaluating your priorities. I know what your No. 1 priority is, that's obvious. I've seen you with her on too many occasions. But you better take a good look at the view from your house, you might not see those mountains in a few years.

Joel, thanks for the plug.

Gary, looking forward to your paper.

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Date: Sat, 31 Aug 1991 20:35:16 EDT
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Auditory stuff

[Martin Taylor 910831 20:05]
(Bill Powers (910830))
>>Just what [the ear] does do is a research question, but one agreed
>>"fact" is that its resolution in time is good at high frequencies, and
>>its resolution in frequency is good at low frequencies (you can't have
>>both together, for fundamental mathematical reasons). Fourier analysis
>>(and most versions of LPC=Linear Predictive Coding) gives the same
>>resolution in time and frequency at all frequencies.
>
>This sounds like the operation of a bank of tuned active filters, which
>would not behave like either Fourier analysis or LPC (from the little I
>know of the latter). Tuned filters can be realized using a combination of

Yes, actually that seems to be what happens, but the interesting thing is that the active filter loop seems to include the basilar membrane itself. When von Bekesy measured the frequency response of basilar membrane in vitro, he got much wider tuning curves than we now find to happen using in vivo measures. The active mechanical resonance of the basilar membrane now seems to account for the precision of tuning that is evidenced in the hair cell responses, at least in large part.

>
>An active filter with too much positive feedback turns into an
>oscillator: tinnitis.
>

I'm not sure this is what tinnitus is. But there is acoustic emission from some (most? all?) ears, discovered by Pat Zurek (if I remember rightly) about 10-15 years ago. I imagine that this is a side-effect of an active filter with too high feedback. As a purely personal guess, I think there is more than one source of tinnitus, one of them being the death of a hair cell or some other individual frequency channel, which releases neighbouring channels from some inhibition, and this may, as you say, increase the effective loop gain into the oscillatory region.

>A bank of tuned filters has a nice property, nervous-system-wise, in that
>it turns a complex temporal waveform into a spatial map. You end up with
>a set of parallel signals which can then be processed just as if they
>were an image (one-dimensional).
>
>>When you talk of the ear analyzing in overlapping frequency bands, you
>>probably are implicitly talking about the individual sensory cells (hair
>>cells) that detect the motion of the basilar membranes, which vibrates
>>in response to the incoming sound.
>
>This tuning is too broad to permit us to get, say, one signal per note of
>the standard scale. There must be another layer that sharpens the tuning.
>Maybe this layer has adjustable tuning (through reorganization) -- it
>would almost have to to fit the various kinds of musical scales that
>people learn to hear as "natural" (Western, India-n, Eastern 5-tone).
>

No, there's another way of looking at this. Consider colour discrimination in the visual system. There are only three tuned visual channels, each having a bandwidth around half an octave (we see about an octave). The red and green channels have peaks separated by only a small amount. Our colour discrimination
is based on the relationship among the outputs of these channels. Similarly in sound. We can discriminate about a 1% variation in frequency, more or less,
presumably based on the relative outputs of many individual channels responsive
to the particular frequency. Very seldom in the nervous system are signals found in just one channel. Distributed representations are much more the norm.

I think there is one more fact that is relevant, and that is that the absolute

sensitivity of the different hair-cell channels differ by several tens of decibels, and that each has a range of perhaps 20 dB (from memory) over which its output changes appreciably. For the deeper acoustic system, there is a lot to analyze before we can get to the notion of a "note".

>The auditory system, it seems to me, is unique in the brain because it
>manages to represent frequencies by the use of neural signals that are
>themselves frequencies of firing. Audible sound frequencies cover the
>range over which neurons can fire. At the lowest levels, the frequency of
>a perceptual sound signal IS the frequency of firing, isn't it? But at

Not exactly--but it is apparently true that the probability that a particular neural channel will fire is a function of the phase of the acoustic waveform. A particular nerve may fire on average every (say) fifth period of the wave, but each time it fires, it will be at about the same phase. Its neighbour may also be firing about every fifth period, but not necessarily the same periods. If you take enough of them, the total number of firings looks kind of like the acoustic wave, so in a gross way, what you say is correct.

>some point, some level, this has to change: a given perceptual signal
>must represent *the fact that* a given frequency is present, and the
>frequency of firing of the neuron must then represent the amplitude of
>the signal at that frequency -- the envelope. I think this would happen

Interaural phase effects are maximal at about 800 Hz, and useful up to around 1500 Hz (maybe a little higher), but coherent phase in neural firing can be seen up to about 4 kHz. It's not clear that firing rate is the signal for intensity, so much as how many neural channels are firing at a high rate, although it is true that a given channel (over its range of sensitivity) will fire more rapidly for a stronger signal.

>at the level where my hypothetical bank of active tuned filters is. The
>output of the filter would not be a sound frequency any more, but a
>neural signal whose amplitude (frequency) represents the amplitude of
>response of the filter. The signals at and above this level would vary in
>frequency at a rate far lower than the individual cycles of a sound-wave,
>because now they represent something ABOUT that sound wave, an
>abstraction concerning its form.

The Patterson model, which is supposed to represent physiological facts pretty well, shows more than envelope effects at all frequencies that we normally study. I guess that envelope effects would be most inportant over 4kHz, but at those frequencies we have very poor frequency discrimination, anyway.

The actual sensory systems are very complex. Their functions may not be so complex--indeed it would be very strange if there were not some principle of uniformity and replication of simple components. But it is hard sometimes to see the functions when the details are so mixed up. Auditory physiologists and psychologists have been able to tease out an awful lot of detail, and I'm certainly no expert on it, but all that detail may well be hiding a set of simple principles.

Martin Taylor
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Date: Sat, 31 Aug 1991 21:01:57 EDT
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Dimensionality

[Martin Taylor 910831 20:40]

An issue that has raised its head like the Loch Ness Monster from a placid surface is that of dimensionality.

The underlying premise of HCT is that one can behave in such a way as to produce perceptions desired. This implies that behavioural controls can affect whatever sensory inputs are available in any way necessary. But there are only a few hundred muscles (assuming that the individual muscle fibres have effects determined only by the sum of their actions on the single tendon by which the muscle is attached), and there are tens of thousands
of auditory channels and millions of visual channels (reduced to one million or so in the optic nerve). There is a certain amount of correlation among the auditory fibres, because of their overlapping sensitivities, but among the retinal receptors the intrinsic correlations are much lower. Any correlations among their outputs are based on relations among things in the visual environment, not on their phsyiological connections (not much, anyway). 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?

It seems to me absolutely necessary that there be open-loop reduction of data in the perceptual input section of the overall control system. This has to be done on the basis of the internal statistics of the perceptual data, and (perhaps) on the basis of averaged success of control operations. Alternately, control theory applies only to a very tiny proportion of perception.

I made this same argument in 1973 in respect of J.G.Taylor's behavioural theory of perception, for the same reasons. He also had claimed that what was perceived was limited to the things for which control behaviour had been learned. My claim then was that this could not be true at the level of the sensor systems, because of the same dimensionality problem. 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.

My central thesis in commenting on J.G.'s theory was that lateral inhibition among sensory fibres would almost inevitably lead to a kind of principal components reduction of the data. We now know that this is more or less so, and principal components analysis is the theoretically optimum way of reducing dimensionality with minimum loss of information. It may even be enough to account for the orders of magnitude reduction that we need if HCT is to apply to a substantial part of what we perceive.

But not all the effects in perception can be controlled.

Martin Taylor.
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Date: Sat, 31 Aug 1991 21:13:48 EDT
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From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: more sound stuff

[Martin Taylor 910831 21:10]
(Bruce Nevin 910830 1904)

> French ba is recognized faster in balance (CV-CVC) than in
>balcon (CVC-CV), but bal as target is recognized faster in balcon than
>in balance. The effect is even stronger in Spanish than in French. In
>another study, a consonant was recognized faster in a given
>syllable-structure position if the accompanying words in the list were
>biased (80%) to having C in that structural position. Thus, with a bias
>to first C of CC cluster being in syllable onset of second syllable, the
>/k/ phoneme of tacron (in onset of second syllable) was recognized
>faster than the /k/ phoneme in tactic, and the reverse with a bias to
>the first C of the cluster being in the coda of the first syllable, as
>in tactic. (English is problematic because in proposed pairs of words a
>consonant in this cluster position is always ambiguous as to syllable
>structure, some call them ambisyllabic, in one word of the pair. There
>is also a problem of stress and vowel reduction. An example is climax :
>climate, where the m of the latter is ambisyllabic and the a is
>reduced.)

I don't remember the details, but I heard Anne Cutler give a talk on this last year, in which she pointed out that the effect was different for native speakers of English and of French. 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.

Martin Taylor