9201 Done

Date: Wed Jan 01, 1992 5:46 am PST
Subject: Xmas present(s!?!?) for the Net

From Greg Williams

OK, Gary, you asked for it. Here is the "Fred and Bill" story PLUS supporting addenda (half of a correspondence with Dennis Delprato; Dennis might wish to post the other half on the Net). I apologize only slightly for the length, since it equals only about three of the average daily Powers posts!

I think MCIMail's Santa Claus wears a business suit and has behavior-mod-trained elves. The $100 was labeled "promotional," which I suggest means that they want to "hook" cheapies like me. The old your-first-five-bucks-are-free-on-the-slot-machines scam. Well, it might work!

THE FABLE OF THE RADIO

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

Distributed at the Control Systems Group Meeting, Durango, CO, August 1991

Note: This is a work of fiction. Any similarity between characters in this story and real individuals, living or dead, is purely contingent, whatever that means.

Once upon a time, a well-off little boy named Fred bought a radio with money his parents gave him (usually, but not always) when he took out the trash. Why Fred wanted a radio isn't important. What's important is that it didn't take him very long to figure out how to work the radio. When he turned the knob marked "ON-VOLUME" clockwise until there was a click, he sometimes could hear soft sounds from the radio. He discovered that the sounds got louder if he kept turning the "ON-VOLUME" knob clockwise, as far as it would go without a lot of force. If he then turned the knob counterclockwise, the sounds got softer. He also discovered that whether the radio made sounds at all (other than a hissing which also got louder when he moved the "ON-VOLUME" knob clockwise, and softer when he moved the knob counterclockwise) depended on the position of another knob, marked "TUNING."

Fred was happy with his radio -- at least he stayed near it much of the time, moved both knobs occasionally (according to a schedule which his parents decided was sometimes "essentially random" and sometimes correlated with the sounds produced by the radio before and/or after the knobs were moved), and evidenced facial expressions and limb movements correlated with the intensity and frequency patternings of the sounds coming from the radio. Soon Fred could get his radio to make sounds that wouldn't result in his parents leaving the room; this occurred more frequently as allowance-paying day approached.

By and by, Fred's friends got radios, too, and Fred discovered that he could work those radios in the "same" (Fred's word) way as he worked his own; in truth, there were some differences, which Fred said were "of no consequence," between working his radio and working his friends' radios -- for example, some of the latter had "ON" knobs, rather than "ON-VOLUME" knobs, and some made louder sounds when their "ON-VOLUME" knobs were turned counterclockwise, rather than clockwise. Still, it took Fred only a little while (with a bit of screaming from his friends' parents) to be able to work all of the radios equally well. Fred exclaimed to his parents, "I really know how to work radios well!" His parents agreed with him, not because of his claim, but because they saw him working the various radios in ways which, to them, could be classified, if not as "good," then at least as "correct."

Alas, one day about six weeks after he got his radio, Fred could not work it. He moved the knobs as he had before, but the radio made no sounds at all. Fred moved them again. No sounds. And again. Still no sounds. Gradually, Fred moved the knobs less and less frequently. (However, whenever he came home from working a friend's radio, he moved the knobs on his own radio quite frequently for awhile, even though the radio produced no sounds.) But eventually, none of his friends' radios could be worked, either, and Fred didn't move the knobs on his radio at all -- the radio just sat silently in his room. Exactly 83 days after the last time he moved the knobs (on a day not noticeably different to his parents than those before or after it), Fred threw the radio out with the trash, muttering obscenities. (His parents, hearing the cursing, washed Fred's mouth out with soap. Fred kept on cursing, and his parents kept on using soap "to deal with his inappropriate verbal behavior." Fred confided to his closest pals, but not to his parents, that he had discovered he "liked" the taste of soap.)

Now, it happened that Fred's radio was found at the local dump by Bill, a street-smart kid who appreciated gadgets -- the more complicated, the better. Bill had never seen the insides of a radio before, and he was delighted when he pried off the back and gazed upon the maze of wires and little objects interconnected inside. "Wow!" he exclaimed. "I wonder how it works?" Bill took the radio home, where he hid it from his father, who would probably try to prevent Bill from "breaking it by messing with its guts" enough to find out how it worked. Excitedly, Bill went to the public library and began looking for books on radios. He found a book with pictures of a radio being taken apart and put back together, in steps, but the radio in the book wasn't the same kind as the radio he had found, and he wanted to know how radios work in general, not just how the one he found or the one in the book could be repaired (which generally involved replacing "defective" parts with little understanding of how those parts worked). Then he came across a book titled BASIC ELECTRONICS: RADIO CIRCUITRY, VOLUME 1 and perked up; even though he didn't know what "electronics" meant, the words "basic" and "radio" so close together seemed encouraging. To make a long story short, Bill read that book (and also VOLUME 2 and VOLUME 3), spent hours looking at the construction of the radio he had found, and finally announced to his father that he had learned how radios work. (To which his father replied "so what?" but Bill didn't let that bother him.) Bill was so happy about knowing how radios work, he told his father about the radio he had found, currently hidden under a heap of broken concrete blocks. "Give it to me!" demanded his father. Bill got the radio and meekly gave it to his father. Of course, when his father tried to work it, he found that it was no use -- Bill hadn't changed anything inside the radio, and it still wouldn't make a sound. "Bah! What good is it?" Bill's father shouted, as he threw the radio down. As his father walked away, Bill calmly walked over to the radio and took off its back. He speculated about why the radio didn't work. "It probably needs a new battery," he thought, and then he saw that one wire to the RF-coil was loose, probably due to his father's anger.

Bill took $3 from under his father's mattress, wrote out an I.O.U. (with interest) and stuffed it under the mattress, and headed for the local Radio Shack store with the radio in hand. On the way, as fate would have it, his path crossed that of Fred, who warily approached Bill, eyeing his non-designer jeans as if in disgust (or so Bill supposed). For a reason which Fred himself, to this day, says he "cannot explain -- apparently, random variability," Fred began yelling "That's my radio! That's my radio! That's my radio!" over and over. Bill dropped the radio and took off running. When Fred recovered his composure, he did not stoop to pick up the radio; rather, he kept on walking and never so much as looked back at the radio lying on the sidewalk. But he emitted, almost inaudibly, "I'll fix that peon!" and his course changed slightly from its direction prior to encountering Bill.

From a hidden vantage point up the block, Bill saw Fred walk away -- strangely, without the radio. As soon Fred disappeared in the distance, Bill hurried back to the radio, grabbed it, and hurried to Radio Shack. On the way, he got to thinking about Fred's claim that the radio was Fred's. Well, maybe it was... maybe it had ended up at the dump by some crazy mistake or weird misunderstanding... and maybe getting it back in good working order would make Fred feel better... and maybe Fred, who looked pretty upscale, might thank Bill and introduce him to one of those fancy uptown girls. It would be worth a try, and $3.

The Radio Shack clerk didn't seem to know an RF-coil from his rear end, but he did show Bill how to use the soldering gun which he kept hot for repairing speaker leads and such. Bill paid for a new (overpriced) battery and moved the radio's knobs gingerly. The resulting chorus of "Louie, Louie" resounded throughout the store! The clerk yelled, "Get outside with that noise!" Bill complied. And just then, Fred appeared! With two cops!! Before Bill had a chance to run, one of the policemen grabbed him around the waist and threw him to the ground. Bill, who had never been in such a situation before, knew exactly what to do: he tried not to move a muscle (which was only partly successful; his left eye started to twitch uncontrollably). The radio continued to produce loud sounds (something about a "very last chance sale" at Harry's Carpet Barn) while the other policeman searched Bill for crack and Fred, with a strange look (at least it seemed strange to Bill), grabbed the radio.

It all turned out better than Bill expected. No, he didn't get introduced by Fred to any uptown girls; he never even spoke to Fred. Fred was immediately and rather mysteriously influenced by the (sound of the? sight of the? sound and sight of the?) again-working radio, and the again-working radio was immediately and rather mysteriously influenced by Fred (or Fred's muscles?). The upshot was that Fred scampered away with the radio, rapturously twiddling its knobs in an incredibly sophisticated way (or so it seemed to Bill). With nobody around to press charges, the policemen had to release Bill with a warning against "fooling around with somebody else's property in ways they wouldn't approve of."

Bill thought that, as they parted company, Fred had seemed very happy. Bill was happy that he had helped Fred become so happy. And Bill was also happy because he knew how radios work, even if he didn't know as well as Fred how to work radios (although he suspected that he could figure out how to work radios as well as, and maybe even better than Fred, if he wanted). In fact, for the rest of his life, Bill remained very happy. (He even married an uptown girl, but that's another story).

For six weeks after the fateful meeting of Fred and Bill, Fred told his parents (with an annoyingly high frequency) that he was "very happy." They believed Fred, not because he said so, but because he took out the trash so regularly, even on the day after allowance-paying day, during every one of the six weeks. (And after those six weeks? Well, that's another story, too.)

February 25, 1991

Dear Dennis,

Our correspondence on mechanism, etc., has continued to occupy my thoughts, and I've finally reached some reasonably coherent (I hope) conclusions. Your comments on the following would be much appreciated.

The stated aims of behaviorist (and interbehaviorist?) psychologists are prediction and control of behavior. In a recent article in THE PSYCHOLOGICAL RECORD, R.D. Zettle contrasts these aims, as part of a "contextualistic" world view, with the explanatory aims of cognitive psychologists having a "mechanistic" world view. I claim that those behaviorist psychologists (led by Skinner) who have adopted a strictly empiricist methodology CANNOT use their "functionalistic" theories to predict and control any behaviors other than those ALREADY investigated experimentally. Because such theories merely restate (summarily and/or economically) experimental results, they are NOT extrapolative -- they can say nothing about the outcomes of experiments dissimilar to those already performed. I need hardly add that being able to predict and control behavior because it duplicates previous experimental conditions is hardly remarkable.

What sort of theories ARE extrapolative? Extrapolative theories MUST incorporate hypothetical constructs at levels "below" the level of the phenomena to be predicted and controlled. They must be what Bill Powers has come to term "generative models" (following H. Maturana). This is because incorporation of lower-level constructs which "go beyond" the functional relationships between phenomena previously found experimentally DOESN'T just SUMMARIZE those relationships -- it specifies how (I would say "mechanistically," but what it really amounts to is DEDUCTIVELY) the phenomena are related to each other IN TERMS OF LOWER-LEVEL PHENOMENA (which are NOT the phenomena to be predicted and controlled), thus allowing EXTRAPOLATION to novel experimental situations. For example, such a model can EXPLICITLY indicate what "other things remaining equal" means at the phenomenal level (it means that any changes don't significantly affect the lower-level constructs which determine the phenomenal-level functional relationships). And it can EXPLICITLY indicate under what conditions at the phenomenal level a functional relationship at the phenomenal level breaks down, by relating the breakdown to lower-level constructs. (The lower-level constructs must not be just "intervening variables" completely expressible at the phenomenal level -- if that were so, they would provide no additional "independent" information for extrapolation.)

A generative model at the level of physiology is necessary for genuine (that is, extrapolative) prediction and control of psychological phenomena. In turn, a model at the level of biochemistry is necessary for genuine prediction and control of physiological phenomena. And so on. This does NOT mean that a high-level science "reduces" to the lowest-level science, but only that if you want to predict and control phenomena (again, in an extrapolative way, and not just summarize previous experimental results), then you must make models at the next level (of phenomena) down.

In the case at hand, such models will, in general, give rise to "predictions" which accurately reflect functional relationships between psychological phenomena already investigated experimentally (such as "response" rates on particular "reinforcement" schedules), and ALSO give rise to (genuine) predictions which reflect functional relationships between never-before-experimentally-investigated psychological phenomena with an accuracy depending on the "worth" (to the investigators) of the models. Note that contextualism (pragmatism) is operative here, as well as for the empiricists. In both cases, a "good" theory can be used to predict and control behavior accurately. The difference is that a wider (potentially MUCH wider!) realm of psychological phenomena is predicted by models at the physiological level.

Empirical theories at the level of the phenomena to be predicted and controlled cannot get beyond the existing data, and to claim that NEW functional relationships between phenomena can be predicted and controlled by them is TOTALLY unfounded. One cannot say that because it has been found that rats consistently do thus-and-so on intermittent reinforcement schedules of a certain type, then humans should be expected to behave a certain way when intermittently reinforced. In the absence of experimental data on humans, the empiricist is (covertly) appealing to some sort of uniformity (of physiology across species!) claim; and given experimental data on humans, there is no REASON to make the predictive leap from rats to humans! In short, a consistent pure empiricist must say "I don't know" when asked to predict the outcome of a future experiment UNLIKE those already performed. However, some not-quite-pure empiricists (including Skinner-the-publicist) think that they can GENERALIZE from existing data to predict outcomes of somewhat dissimilar future experiments. I submit that this amounts to the inductive fallacy, which tends to be more fallacious with more extreme extrapolations. Empirical generalizations (ad hoc "laws") have no a priori limitations (or, for that matter, a priori justifications); they are only constrained by a posteriori data. A serious problem here is the temptation to OVERgeneralize in the absence of data-constraints... especially when seeking funding! Lower-level models incorporate a priori DEDUCTIVE limitations on their predictions. A lot could be said (which I won't) about how these deductive limitations help to direct the search for new experiments capable of falsifying the models; on the other hand, generalizations don't provide such direction.

What I think of as explanations of phenomena are always given in terms of phenomena at lower levels. To me, an explanation is a generative model. An explanation of goings-on at level n is a descriptions of goings-on at level n-1. As you have said, an explanation IS a description -- but the important point is that it is a description at the next lower level relative to the level being explained. But I'm not claiming that modellers want to explain, while empiricists want to predict and control. The fact is, modellers (when their models are "good") CAN predict and control (at the next higher level from their models), but empiricists CANNOT (at the same level as their models) (unless predicting and controlling ONLY repeats of PREVIOUS experiments count).

In sum, then, my notion is that, contrary to the view generally held by scientists, genuine (extrapolative, rather than summarizing) prediction and control of phenomena at level n can be achieved only by theories couched in terms of level n-1. This implies that empiricist theories in psychology can be used to (genuinely) predict and control (and explain, as I use the term) sociological phenomena, NOT psychological phenomena. Empiricist theories in physiology are required to (genuinely) predict and control psychological phenomena.

As a footnote, to forestall possible confusion, I don't claim that scientific disciplines as conventionally constituted actually correspond to phenomenal levels as I have discussed them above. Many contemporary physicists work with models at one phenomenal level to predict/control/explain phenomena at a higher level. Nevertheless, the phenomena they are trying to predict/control/explain are physical phenomena, and hence they are physicists. The limits of a discipline, I think, are set by the kinds of phenomena being predicted/controlled/ explained by its practitioners. What this says for psychology is that its practitioners should predict/control/explain psychological phenomena via models using physiological constructs. As long as investigators find it useful to predict/control/explain psychological phenomena, psychology won't disappear or be "reduced" to physiology.

And finally, I want to mention that in my training as a mechanical engineer at Case Tech and M.I.T. (mainline engineering schools, I submit), various professors made a point of distinguishing between empirical relationships and (n-1)-models, and claimed that the latter (when "good") are more valuable, because they contain more information than the former (thus allowing extrapolation beyond previous experimental conditions), and "because they say 'why' the phenomena are related as they are [that is, deductively]" (allowing LIMITATIONS of the extrapolations to be PREDICTED, something which not even "generalizations" from empirical data -- ad hoc "laws" justified a posteriori by fitting new data -- can do). These predicting-and-controlling engineers can see the advantages of explanatory models, even if the "behavioral engineers" don't!

Thanks for hearing out this paean to modelling!

Best wishes,

Greg Williams
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40328
606-332-7606

cc: W.T. Powers
W.D. Williams

R. Marken
T. Bourbon

August 1, 1991

Hi Dennis,

I'm sorry this reply to your letter of May 31st took so long. We've been busy building a house, in addition to our more usual undertakings. I'm taking the liberty of forwarding copies of this letter AND YOURS to Bill Powers, Tom Bourbon, and Bill Williams, since they expressed their interest in the ongoing discussion. Please send copies of your reply to this letter to them. Thanks! Maybe they will jump in sooner or later...

I am happy to hear that the "mechanism" business was a language problem, not a fundamental difference between us. For me, a mechanism is always instantiated in physics, but the mechanism itself is a FORMAL notion (example: "amplification").

You say that Skinner was a theorist, despite his atheoretical posturing. To the extent that his theoretical terms were limited to the psychological (or "molar behavioral," I suppose) level, I claim that his theories are NOT explanatory with regard to psychological phenomena. Such theorizing can be, in my terms, "extrapolative" ONLY if it is combined (perhaps covertly) with postulates containing theoretical terms at the functional level "below" the psychological level. It seems to me that the operant construct as applied to any particular experiment can only be used as an after-the-fact, ad hoc description of what already happened, and that the operant construct as generalized across experiments must smuggle in lower-level concepts (accounting for certain sorts of uniformity through time WITHIN THE ORGANISM, at least) to be explanatory in the extrapolative sense.

I am not as much interested in efficiently training an animal as in understanding why it is possible to train the animal at all in particular ways. Yes, I realize that funders want "practical" science, and I understand that the behavior modification people do their best to comply with the funders. But extrapolative explanation can be much more efficient than empirically based prediction and control. My mother predicted and controlled the behavior of her living room for years without being able to explain how the circuit worked. That was fine until it broke -- then she had to hire an electrician to fix it! I make a living reporting on horticultural experiments, most of which are written up with a conclusion to the effect that "we did A and then B happened, but when we did C, D happened." How much better it would be to know the underlying mechanism (I assume I can use the word safely now) which EXPLAINS (that is, allows to be DEDUCED) why A DIDN'T result in D, and perhaps even why E will result in F! At any rate, I go right ahead and report the (nonextrapolatable) results, since nobody has done the extrapolatable work. (Here, Skinner has a point. Maybe extrapolative explanation is too difficult and/or too impractical. But maybe not, if somebody like Bill Powers makes a few good guesses about the underlying mechanism.) I won't quibble about the operant conditioning procedures, except to the extent that I quibble about the horticultural procedures when the researchers suggest that their results can be utilized under somewhat different conditions (the "somewhat" depends on the particulars; say, with tomatoes instead of peppers). HOW do they know the limits on generalizing their results? They DON'T (in advance of using tomatoes), because they don't have an underlying causal model, only limited correlations. Nor does Skinner when he suggests that operant procedures are unexceptional in their applicability.

"Generality is the plea of the scoundrel," I say. The explanatory worth of a theory depends on its ability to predict the LIMITS of a procedure. Control theory can explain (rightly or wrongly is another question) why operant procedures of certain types are inefficient when free-feeding is permitted (the error signal for food remains small, etc.). I don't see how a purely psychological-level theory can explain that.

In your recasting of my argument, point "b" should have read: "Extrapolative theories use principles at one level to EXPLAIN (and thereby to aid prediction and control of) events at the next higher level." My emphasis is on explaining, not on predicting and controlling. Skinner emphasized the latter. So do you, when you tout operant procedures because of their "efficiency." (But we don't live by white bread alone. On the other hand, curiosity nearly killed Wile E. Coyote.) The activities are somewhat independent, in that you can explain without necessarily predicting and controlling, and vice versa. But what I call genuine explaining can aid prediction and control, whereas I don't think empirically based predicting and controlling can DIRECTLY aid explanation (they can, INDIRECTLY, though, by suggesting theories at the level below the phenomena -- we'll always need experimentalists, but I object to experimentalists who say we don't need extrapolative theorizing).

With regard to what CST is doing, I think it is quite rightly making (as I said before, deductive or causal or mechanistic) models at the level below the psychological level in order to explain functional relationships observed at the psychological level. For example, the time courses of states of error signals in particular organism-and-environment circuits (here's where your "field" notions come in, I suppose, although they seem too underspecified to get us very far in any particular case) are used to provide explanations for cyclical eating behaviors. Note that error signals are instantiated in physiology (as are behaviors!), but the physiological details as such aren't used in such explanations -- what IS important is the "functionality" of the circuits, which actually can be duplicated in various other (including computer) physiologies (I guess that's a good way to define "functionality": what can remain invariant across physiologies, like transfer functions). The bottom line, as I see it, is BOTH that the circuits extend outside organisms (there's your "system perspective"), AND that their functionality (capable of explaining behavioral phenomena) is at the level BELOW behavioral phenomena. Functionality is NOT material!!! As Bateson said, it is the PATTERN WHICH CONNECTS. I say it is the PATTERN WHICH EXPLAINS THE NEXT HIGHER LEVEL OF PHENOMENA).

Thanks for hearing me out again. I, at least, think I'm zeroing in toward a point of view which is convincing at least to myself.

Best wishes,

Greg Williams
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P.S. There are some really tasty licks on the new Mark O'Connor ("The New Nashville Cats") album, as I suspect you already know.

August 6, 1991

Hi Dennis,

Here's an addendum to my last letter.

I found this on page 529 of BEHAVIOR PRINCIPLES, Second Edition, by Ferster (almost the horse's mouth!), Culbertson, and Boren, 1975: "Thus we use [the phrase] abstract stimulus control because it refers to the environmental events responsible for the behavior rather than [the phrase] concept formation which tends to place control erroneously inside the organism and which has the danger of becoming an explanation rather than a description of the behavior."

This suggests that the behavior analytic authors make a similar distinction as I do regarding explanations and descriptions. Of course, contra my position, they value the latter more than the former (but never go into detail about why they do; neither "explanation" nor "description" is in the book's index, and I cannot find any other explicit treatment of this issue in the book -- I suppose it is just "obvious" to the authors that dealing with observables is best, and that unobservables "obviously" don't control behavior).

Perhaps the evolution of the (to my thinking) mistake by behavior analysts of avoiding any consideration of underlying variables began with the reaction to introspection: psychology is to be a study of BEHAVIOR, not MENTAL EVENTS. OK. But then they blew it by not wanting to use underlying ("mental," prior to becoming observable via physiological studies) variables to EXPLAIN behavior. The credo perhaps forced upon them because they wanted to be "scientific" is "description of the phenomena is enough for prediction and control of the phenomena." True, it gets you a ways. But not far enough -- the procedure is not extrapolative; for example, it doesn't predict limits on what is possible to condition -- as the cognitive psychologists have been able to convince funders. I'm convinced, too, which is why I find the modelling philosophy of control theorists more satisfying than the self-handicapping anti-modelling philosophy of some (all? nearly all?) behavior analysts.

At any rate, my notion that explanation of phenomena is description at the next level below the phenomena (which might include hypothetical entities) seems to be accepted by at least some behavior analysts. Want to join the crowd? I think physics has moved right along in large part because this notion got endorsed early on by many physicists (Newton notwithstanding). To hell with handicapped prediction and control -- let's try to EXPLAIN behavior, and BETTER prediction and control will be a product of that. (But let's try to explain with FERTILE models, like Bill's, not with ad hoc models like most of those in "cognitive science.")

Best,

Greg Williams
606-332-7606

Date: Wed Jan 01, 1992 7:03 am PST
Subject: Fred & Bill; Expanation & Description

[from Gary Cziko 920101]

Greg Williams (920101) said:

>OK, Gary, you asked for it. Here is the "Fred and Bill" story
>PLUS supporting addenda (half of a correspondence with
>Dennis Delprato; Dennis might wish to post the other half
>on the Net). I apologize only slightly for the length, since
>it equals only about three of the average daily Powers posts!

Thanks ever so much. I hope that other CSGnetters will enjoy reading the fable as much as I did. And the correspondence to Delprato was an added unexpected bonus which goes right to the heart of the description/explanation distinction I wanted to address.

While I think I understand and basically agree with your argument, the apparently reductionist aspect of it still makes me feel a bit uncomfortable. You say:

>   A generative model at the level of physiology is necessary
>   for genuine (that is, extrapolative) prediction and control
>   of psychological phenomena. In turn, a model at the level of
>   biochemistry is necessary for genuine prediction and control
>   of physiological phenomena. And so on. This does NOT mean
>   that a high-level science "reduces" to the lowest-level
>   science, but only that if you want to predict and control
>   phenomena (again, in an extrapolative way, and not just
>   summarize previous experimental results), then you must make
>   models at the next level (of phenomena) down.

But if you can explain, predict and control a phenomenon by the next level down, how is this not reductionism? Where do you put in the "downward causation" discussed by Popper and Don Campbell and at work in the control hierarchy when phenomenon at a higher level influence phenomena at a lower one?

Also, you use the word "generative" to refer model building at the n-1 level. It is interesting that this is the exact same word that Chomsky and post-Chomskyan linguists use to differentiate their approach to understanding language from the descriptivist structural linguists which preceded them. But I'm not sure where their n-1 level is. It certainly isn't physiology or even psychology. Perhaps Bruce Nevin, Avery Andrews and/or Martin Taylor could add some insight here.--Gary

P.S. How much of the $100 got consumed in your post? Will we ever hear from you again? I hope so.

Gary A. Cziko
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Date: Wed Jan 01, 1992 9:34 am PST
Subject: greetings & notes to Bill and David

[from Dick Robertson]
Happy New Year to everybody! I am still having trouble getting my posts to go through to individuals (I hope this goes on the net) since we went over to the new system. So
TO DAVID GOLDSTEIN - I got your post about sending me the note on the self and I am looking forward to getting it, I'll send you more when I can get through.
TO BILL POWERS - I have tried several times to send you the note on the grade control system but I gather you have not got it, since I haven't seen reply. I'll try again when the expert is back at school
My new address: urrobert@UXA.ECN.BGU.EDU   best wishes, Dick Robertson

Date: Wed Jan 01, 1992 10:01 am PST
Subject: Language

[From Bill Powers (920101.0900)]

Happy start of a new year, everyone.

Martin Taylor (911231) --

>As I remember, this whole information and uncertainty thread arose
>because of what I considered to be a misuse of the term "information" in
>the discussions on language and PCT. There turned out to be less
>understanding of information theoretic ideas within the (responding)
>community than I had supposed, so I have tried to improve the situation.
>With luck, the concepts will become sufficiently intuitive that
>modellers will unconsciously embed them into their models.

So I-T may get back into the act yet -- I trust you'll be watchdog and see where we should be using it.

>As always when I post from home, please forgive extraneous characters in
>the above.

Only one error I could see (a { for an e). So perhaps it's just the echo back to you that's noisy.

Avery Andrews (911231a) --

Gary Cziko said
>How could we possibly satisfy our intention that a proposition be true
>if >we couldn't somehow perceive the proposition as true?

And you replied

>Well, it seems to me that in the general case we don't *perceive*
>propositions as being true, but *judge* them to be true...

I think we deal with propositions in much the way the propositional logic does. That is, stating a proposition carries an implicit " .. is true." So when you say "The ring is gold" this is like saying "It is true that the ring is gold." When you look at a thermometer, you describe what you see by saying "The temperature in this room is 71 degrees F." This statement is a description and we treat descriptions as true propositions. The reference "signal" (awkward term) is also a true proposition (i.e., one that is desired to be true): "The temperature in this room is at or below 68 degrees F." This statement is a prescription, not a description. When the description is compared with the prescription, we see that there is an error, and presumably will convert that error into some action that will eliminate it (by altering the basis of the description). If the prescriptive proposition had been "The temperature in this room is at or above 68 degrees," comparing it (logically) with the input proposition would show that there is no error: 71 degrees is at or above 68 degrees. No error, no action.

>Does anyone have a clear idea of how to build a `fridge door open'
>(or `leopard nearby') detector along the lines of the hierarchy? My

>understanding of the higher level portions of the hierarchy was that
>they are rather tentative suggestions, and I confess to having never
>been very happy with them (what's the latest version, anyway? the
>latest I've seen has 10 levels, not 11).

No, we're still at the stage of trying to describe what it is that needs to be modeled. Even perceiving the nonverbal and nameless configuration we see when a fridge door is open is still beyond us. We seem to agree that sensations are probably weighted sums of intensity signals, and weighted sums seem to be understandable as neural computing processes. The rest, as far as working-model design is concerned, is up for grabs.

The levels as they stand now are

Intensity, sensation, configuration, transition, event, relationship, category, sequence (ordering), program (if-then contingencies), principle, and system concept. There are many systems operating in parallel at each level. A given system's perception is a function of a set of perceptions of lower level, usually the next lower level. To alter or control a perception at level n it is necessary to manipulate perceptions of level n - 1 (or lower), but the reverse is not true. For a perception of level n to exist, perceptions of level n-1 must exist, but the reverse is not true. These are the principles of HCT, which any proposed level of perception must meet.

Avery (911231b) --

>>The only way you can appreciate the problem is to get inside the
>>speaker and catch yourself using a third-person word, and ask "How did
>>I know that I should use that word?"

>No. The first wisdom of linguistics is that speakers are always wrong
>when they try to explain why they say what when (the stories are
>pathetic, and tend to fail within 30 seconds).

I believe you're talking about speakers being wrong in stating a general rule about language as they use it. That isn't what I meant (I should have said what I meant more clearly). What I meant is simpler (and I'll use a simpler example). I'm talking about the experience of looking at two dogs and selecting "dogs" as the term for describing them rather than "dog." From the external point of view, one would explain that "dogs" is the plural of "dog", and because there are two dogs, we must use the plural form to refer to both of them. What I want to know is how the speaker knows there are two dogs rather than one, so as to know whether to pick the plural form over the singular form. When we get inside the system doing the speaking, we realize that it is not at all self-evident that two dogs look different from one dog (this is especially the case if we're looking at the problem of building a perceiver that can report the number of instances of a given item).

From inside the system, it's clear that "dogs" is not just a plural: it's the name of a category. I'm seeing an instance of this category, and another instance of the same category, with the result that I perceive "twoness" in addition to "dogness." This happens before I pick the terms with which to describe the experience -- it must happen first, so I can tell what words to use. From the external standpoint, these questions of perception don't arise; the perceptions are projected into an objective world, and the assumption is that any viewer would experience exactly the same world. It might take you a while to realize that the perception I refer to as "two dogs" is my two automobiles. Or that I have not perceived the third animal as a dog, because it looks like a fuzzy cat to me.

>... getting behind the descriptions to the explanations will be
>reverse engineering all the way ..

Beautiful. Precisely.

>One important difference between talking and ordinary behavior is that
>such control systems as there are to govern the structural aspects of
>language use are limited, and don't work very well.
> On the one hand there are no significant disturbances to prevent you
>from saying something that means one thing rather than another, and on
>the other hand it's incredibly difficult to tell what the people who you
>are talking to actually make of what you are saying ...

We were talking about this not long before you got on the net. I've proposed -- to be shot down or developed -- an HCT approach to language generation that involves two parallel control processes.

One process adjusts utterances according to the meanings they evoke in the speaker as they are uttered (or in imagination prior to utterance); this leads to editing on the fly and other means of adjusting the meaning of an utterance to make it match the meaning intended to be communicated. The control system controlling for meaning has no preference for one verbal form over another as long as the result is perceived by the speaker as the speaker's intended meaning. Meanings, save in special circumstances, are nonverbal perceptions to which words and word-structures refer (memory association or some other mechanism).

The other process adjusts utterances according to whatever linguistic conventions the speaker knows and cares about. The linguistic forms are perceived, and if they are in error, the control systems adjust the developing sentence where possible or force a restart. You see why I'm concerned about how an error at a higher linguistic level gets turned into the adjustments at lower levels that will correct the error.

These two control processes select utterances that satisfy both kinds of criteria at once: the sentence must convey, at least to the person generating it, a meaning that matches the meaning to be communicated. At the same time, it must be perceived as satisfying the requirements of form, whatever they are for that person. If there is an utterance that will meet both goal conditions, it is found and emitted. Usually there is more than one utterance that will do the trick. But sometimes there isn't any.

And of course the speaker can be mistaken, socially speaking, about both criteria. The words may evoke meanings unique to the speaker, leaving the listener wondering what was meant or in possession of an unintended meaning (as you say). Or the grammatical forms may be regulated with respect to a misunderstood social convention, so they sound fine to the speaker but ignorant to everyone else.

> so it, I would say, [is] primarily a matter of flying blind via feed-
>forward (which is one reason why most people do it so badly).

In the light of the above, I hope you'll see an alternative to [ugh, blech] "feed-forward."

>There are at least two issues to be dealt with: a) what is the
>structure of the representations in the `reference signal' (desire box)
>and `input signal' (belief box) b) how are the actions calculated to get
>them converged. (a) is something which linguistics can contribute to,
>but not (b), I would say.

You're right about the issues. I hope you're willing to put in some effort on (b), which is the "reverse engineering" part of the modeling problem. Perhaps linguistics as it is can't handle (b), but let's think in terms of linguistics as it might be, given control theory. I think some study of how people construct sentences, with this problem in mind, might show us how people actually detect and correct errors. Then at least we'd know what we have to model.

I really think that for our lifetimes, "modeling" at these higher levels is going to remain at the level of defining the problems. If we can see some regularity in the way people correct errors, we can incorporate that regularity into a working model even if we can't say how this process is carried out neurally. In this way you can test a model by running it, to see if it's self-consistent and what unexpected things it will do at whatever level of detail we can handle.

I agree with you that Prolog might be a very appropriate language for doing some of this modeling. I don't know the language, but I presume that those who do will "volunteer" their services.

re modeling:

>in interesting cases, there are a
>horrifyingly large number of different paths what would have to be
>explored to figure out how to satisfy the desired goals, and it's
>not at all clear how we are able to find our way through the maze.

It's not necessary, in HCT, for any system to do a survey of possibilities. A control system of a given level doesn't care what lower-level perceptions are actually employed, as long as they add up to the right state of the perception at the higher level. What you need is just a map showing, for each value of error signal, some alteration in a lower-level perception or set thereof that will make the error smaller. As long as you keep making the error smaller, you will end up with control. It doesn't matter whether the resulting path is the shortest or best -- that would involve a different kind of perception, one that isn't essential to control. I don't think the difficulties are as awful as they first appear.

>I'd suggest that David Marr's work on visual perception & Ray
>Jackendoff's on natural language semantics (Jackendoff 1987,
>Consciousness and the Computational Mind; 1991 Semantic Structures) is
>quite relevant to the conversion problem.

I've read Jackendorf's (sp?) book and remember that he seemed to be following the same path as mine, just about step for step, until we diverged at some point that I forget. I'll leave this in the hands of linguists, however -- I'm always getting tempted to go deeper into linguistics than my knowledge justifies.

re modeling:

>So we have a simulator (`imagination'), to calculate the likely outcomes
>of various courses of action. But full analog simulation is also
>extremely expensive, whence the utility of propositions and logic
>as a cheap (though often nasty) substitute.

I agree about the simulator (imagination mode). But you have the cost of analog vs. digital operation backward. The analog approach is BY FAR, by ORDERS OF MAGNITUDE, the cheaper and simpler. You're thinking in terms of computers; I'm speaking in terms of brains. But even in the computer world, an analog computer can run rings around a Cray in computing systems of simultaneous nonlinear differential equations. It's just not as accurate, and so can't carry out integrations over long periods of time.

Neurons are fundamentally analog devices. Getting them to do digital-like operations is a complex matter that must involve constructing many-neuron circuits even for the simplest "and" or "or" operation (essentially nobody believes any more that single impulses have any signficance). So in the brain, it's the digital operations that get complicated and expensive.

>In effect, propositional representations save computuational effort by
>omitting details that are (usually) irrelevant to the important features
>of outcomes.

This is why there's a category level of perception in the HCT model. Categorizing is the process by which we create discrete symbols to represent the continuous analog world of the lower levels. Propositions are cast in words or other symbols that stand for categories of perceptions, leaving out all the differences that are by fiat treated as not making a difference. We act as if logically-calculated outcomes expressed in verbal symbols contain all relevant considerations, but that is hardly ever true. In fact, we tend to treat a description of an outcome or decision as if that's all that is required, when in fact that description must be turned into many levels of perceptual goals, and ultimately into detailed motor activities, if the described result is in fact to be brought about. How is it that we are able to create detailed effects on our worlds which, when perceived, turn out to conform to a verbal description of an act that is to be performed? Linguistics (and A-I) have essentially ignored this problem.

>It won't be easy to figure out how the interfacing and the logic works.

But won't it be fun?

Greg Williams (920101) --

You're doing fine with Delprato and I'm keeping out of it. Nice to have your thoughts on the net. Needless to say, I concur completely with your analysis of explanation in terms of generative models. Generative models aren't reductionist, because their structure is part of what makes them work. Structure isn't a reductionist concept: just the opposite.

Best to all

Bill P.

Date: Wed Jan 01, 1992 3:24 pm PST
Subject: Reverse Engineering

[from Gary Cziko 920101.1715]

Avery Andrews (911231a) said:

>... getting behind the descriptions to the explanations will be
>reverse engineering all the way ..

Bill Powers (920101.0900) replied:

>Beautiful. Precisely.

I would appreciate Avery and/or Bill giving me a description and example of "reverse engineering." I have a hunch that ALL science and ALL nontrivial engineering (i.e., finding engineering solutions to new problems) is in fact reverse engineering, but I want to no more about what this terms means before making this claim.--Gary

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Date: Wed Jan 01, 1992 3:53 pm PST
Subject: Structure vs. Reductionism

[from Gary Cziko 920101.1730]

Bill Powers (920101.0900) said in response to Greg Williams (920101)

>Generative models
>aren't reductionist, because their structure is part of what makes them
>work. Structure isn't a reductionist concept: just the opposite.

Bill, could you expand on this a bit? As I've seen the term "reductionism" used, it seems quite consistent with Greg's using psychology to explain sociology, physiology to explain psychology, and physics to explain physiology (although I guess I would stick chemistry in their between physiology and physics). So you are saying that moving to the next lower level isn't reductionism because of the structure at that same lower level?
 But what is the structure at the level of physics? Just the laws of

physics? Using physics and its laws to explain more molar phenomenon still seems reductionist to me.

What saves PCT from reductionism (for me, anyway) is the idea that higher levels of organization specify reference values for the lower levels. But what is the comparable structure in physics or biology?

Also (and this is for Greg, too), can't generative models of psychology move to a lower level that is still psychology? If you want a generative model of a dancer's ability to dance, can't you do this analyzing it into a series of smaller abilities or behaviors (sequences and programs)? And what provides a generative model of economics? Somehow, I feel uncomfortable with looking to sociology for economics.

Greg's post today had so many interesting tidbits in it that I think we'll be mining it for a while to come. I'd love to see this reductionism/structure/explanation/description topic become another lively thread on the net. I just hope that Greg's funny money from MCI Mail lasts a while!

--Gary

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Date: Wed Jan 01, 1992 4:10 pm PST
Subject: generativity

Gary Cziko:

>Also, you use the word "generative" to refer model building at the n-1
>level. It is interesting that this is the exact same word that Chomsky and
>post-Chomskyan linguists use to differentiate their approach to
>understanding language from the descriptivist structural linguists which
>preceded them. But I'm not sure where their n-1 level is. It certainly
>isn't physiology or even psychology. Perhaps Bruce Nevin, Avery Andrews
>and/or Martin Taylor could add some insight here.--Gary

There ain't none - it's a different notion of `generative', referring to the idea that the generative grammar is supposed to describe with mathematical rigor, in the manner that equations generate circles, etc. (some of them do, some of them don't). Interestingly, however, linguists can use these models to make predictions that are, I think, more impressive than the average social science generalizations, since the patterns described are quite intricate. E.g., given that s_1 thru s_n are sentences of a language, we can often successfully predicat that some new sentence s is also (and what it means (but the respective roles of the linguist and the theory are not terribly well-defined in the manufacture of these predictions, for reasons I won't go into now.

All I meant by `reverse engineering' is that there is no quick substitute for figuring out how it works on the basis of analysing what it does.

Avery Andrews

Date: Wed Jan 01, 1992 6:01 pm PST
Subject: Closed Loop and Newsletter

from Ed Ford (920101.18.55)

Happy New Year to all....

The Newsletter and the latest issue of Closed Loop should be in your mail boxes within two weeks IF you are a paid up member of the CSG. Just the newsletter if you are not paid up but still on our mailing list. Contributions (single spaced, type written) for the newsletter will be accepted, especially by E Mail. Since the advent of the CSGnet, most netters, it seems, would rather stay with the net, thus the slim pickins in the newsletter. Those non-members wishing the latest edition of Closed Loop should send their annual $40 dues payable to: Control Systems Group and send it to Mary A. Powers, 73 Ridge Road, CR 510, Durango, CO 81301.

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Date: Wed Jan 01, 1992 11:58 pm PST
Subject: Re: Control of Beh.;Kolbe;M.Taylor

From Tom Bourbon [910102 -- 0:45]
Rick Marken [911230]. The presentation I mentioned earlier, on control session, with accompanying computer demonstrations. I can't send a manuscript, because there isn't one, yet. I have the proposal, which is a glorified abstract, which I will send to you. I am writing on the paper during this holiday break. Actually, I want to learn if the presentation is accepted, before I finish the manuscript. I would like to add finishing touches ot the paper after presenting it at the meeting in April, then ship it off for rejection by JEAB.

The approach to control of others that I take is in the context of two-person tracking tasks. This study uses the program from my chapter in Wayne Hershberger's book. In that program, each of two people uses a different control handle to keep his or her cursor even with his or or her target (two targets, two cursors), but movements of one of the handles (the one on the left) also affect the cursor affected by the other handle. In the original study, this was run and modeled as an example of simple interference, by one person, with a variable controlled by the other person.

However, the environmental linkages are such that the person who affects both cursors can act to deliberately affect the cursor controlled by the other person. The options for deliberate influence range through helping or aiding the other, entering into impossible conflict, or controlling the actions of the other. For the "privileged" person (the would-be controller) to control the actions of the other (the controllee-to-be), the controller selects a pattern of handle movements he or she wants to see from the controllee, then manipulates the left handle in a way that disturbs the cursor controlled by the controllee. By experimenting a little, the controller discovers that it is only necessary to watch the handle movements of the controllee and move the left handle any way necessary to see the desired pattern. Of course, the actions of the controller are constrained, in that his or her actions constitute disturbances to a variable controlled by the controllee and if those disturbances are are excessive, the controllee will abandon the task, thereby thwarting the inteions of the controller.

Trying to describe this in words is not easy! But everything happens smoothly and quickly when two people perform in this condition. And the same PCT models I used in the chapter simulate the task beautifully. Each of those models is a single loop, controlling a relationship. In an expanded version of the models, the model of the controller has two loops -- one to produce movements of its own handle, the other to control for the perceived pattern of movement by the other handle. The pattern-control loop acts only on the integration factor of the move-the-left-handle loop. The initial gain for that loop is set randomly, then the higher loop uses a random (E.coli) procedure to change the gain, if the pattern it senses from the right handle deviates from the one intended by the left model. All the while, the right model tracks the right target with the right cursor, eliminating the disturbances produced by the left handle and, in the process, moving its simulated handle in the pattern "desired" by the left model. Just the way two people do it.

Your plan to use key presses and directly vary the schedule will obviously, and nicely, produce the results seen in the libraries full of cumulative records that behaviorists have cranked out over the years. The closest I get to that approach is when I vary the magnitude of the effect of a handle on a cursor, in a tracking procedure -- the greater the effect, the smaller the movements, and so on.

I am not familiar with behavioristic studies in which people tried to predict moment-by-moment details of behavior by invoking manipulations of schedules. However, in the November issue of _Psychological Science_, (Nov., 1991), Robert Epstein has an article (Skinner, creativity, and the problem of spontaneous behavior, pages 362-370) in which he cites some of his earlier work that recorded moment-by-moment changes in the probabilities of various "Responses" to "multiple controlling stimuli." I plan to look at those studies, when our library reopens. They might be of interest to you in your writing.

David Goldstein [911230]. So, you also lapsed into an interpretation of Kolbe that suggests she wrote about "types" of poeple -- you said that Rick and I are "follow through" types.

I did look at the book again -- for the last time. It is true, as you said, that she writes of four "variables" or "modalities" that characterize human actions -- people have variable "magnitudes" of "fact finder," "follow thru (sic)," "quick start," and "implementor." In turn, each of these "faculties" is driven by a different _instinct_: "the instinct to probe, the instinct to pattern, the instinct to innovate, and the instinct to demonstrate," respectively. If you see very much in all of that that resembes PCT, you must be reading a different book from me!

Clearly, she has a "power" or "energy" theory in mind -- a person has only a certain amount of "conative energy," nicely parceled out into each of the four categories, in different proportions for different people. This energy or power is "unleashed," "stored up," "released," "used up," "replenished," and so on. (No wonder she likes to associate her ideas with those of Freud, although she says he didn't get it all quite right. Her ideas are as psychodynamic as you can get.)

There are some gems, scattered throughout the book. "But she did a personal nosedive when the business outgrew her Quick Start strengths and needed more Fact Finder than she could muster." (p. 65) "Richard's burnout was a symptom of conative stress. He had used up his Follow Thru without being able to replenish it." (p. 75) What can I say?

She uses four-digit codes to represent the magnitudes (0-9) of each of the four "faculties." Hence, one person might be a "4629" while another is a "6718." If that is not a reversion to typological thinking, I never saw one.

If some of you pursue contact with Kolbe, I wish you well. If you find her open to PCT, please let us know.

Martin Taylor [911231]. If you have opportunities to follow through (oops, follow thru) on your resolution to look for applications of information-theoretic concepts to control theory, please share the results. Being the fundamentally lazy person that I am, I will resolve to follow your posts, with interest, and with appreciation for your efforts.

Best wishes.

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Date: Thu Jan 02, 1992 6:14 am PST
Subject: While the funny money holds out....

From Greg Williams

Gary Cziko (920101):

>While I think I understand and basically agree with your argument, the
>apparently reductionist aspect of it still makes me feel a bit
>uncomfortable.

>But if you can explain, predict and control a phenomenon by the next level
>down, how is this not reductionism? Where do you put in the "downward

>causation" discussed by Popper and Don Campbell and at work in the control
>hierarchy when phenomenon at a higher level influence phenomena at a lower
>one?

Possibly I have claimed, or implied, too much. I wanted to emphasize that trying to explain (as humans appear to typically interpret that term) phenomena SOLELY AT THE LEVEL OF THE PHENOMENA doesn't work. I admit a bias toward "how" explanations, but I admit that a case can be made for "why" explanations, too, which appear to involve the level ABOVE the phenomena. But in neither genuine "how" nor genuine "why" explanations ("genuine" meaning extrapolative; setting limits) of phenomena at level n can refer ONLY to phenomena at level n.

Skinner was enthralled by, and gave primacy to, "why" (historical and evolutionary) explanations over and above "how" explanations. But I'm not very impressed by most of his arguments for doing so. The utility of "why" explanations for prediction and control seems to me much more circumscribed than that of "how" explanations, in general -- Skinner himself admitted that the time gap in causal notions in his own "why" explanations (i.e., reinforcement BACK THEN results in increased response rates NOW) needed filling with "how" explanations at some point. Nevertheless, his argument that it is premature to attempt "how" explanations for psychological phenomena because of current technological limitations is reasonable, and this problem can only be worked around by bold and inspired (and maybe even lucky) theorists -- like Bill Powers!

>P.S. How much of the $100 got consumed in your post? Will we ever hear
>from you again? I hope so.

That was 250 X my 2 cents worth. Still plenty more where that came from (pant, pant)!

Gary Cziko (920101.1730)

>What saves PCT from reductionism (for me, anyway) is the idea that higher
>levels of organization specify reference values for the lower levels. But
>what is the comparable structure in physics or biology?

What I THINK (and I could be wrong) saves the "generative models" notion of explanation from reductionism is that you can't skip levels. The chemistry of combustion isn't used in the explanation for the stalled car; rather, the clogging of the carburetor jet is. The "structure" (comprised of functional relationships) at level n - 1 SUFFICES to explain observations at level n, and there is no pining for references to even lower levels. You COULD incorporate such lower-level structural references, but it appears to me that people don't need it to accept an explanation.

>Also (and this is for Greg, too), can't generative models of psychology
>move to a lower level that is still psychology? If you want a generative
>model of a dancer's ability to dance, can't you do this analyzing it into a
>series of smaller abilities or behaviors (sequences and programs)? And
>what provides a generative model of economics? Somehow, I feel
>uncomfortable with looking to sociology for economics.

You certainly can DESCRIBE behaviors in finer and finer terms. But if you don't include consideration of the underlying structure, it will be ONLY description. Detailed observations of, say, intention tremor can provide no more basis for its amelioration (that is, MORE THAN masking of or compensation of the symptoms) than can detailed observation of the relationships of stalling to car speed, accelerator pedal position, etc., provide a basis for knowing which jet to replace. The underlying generative mechanisms are simply underdetermined by the observed phenomena in any reasonably complex system-in-its-environment.

Ever cheaply yours,

Greg

P.S. Is there still interest in an IBM DOS binary-to-ASCII file conversion program? My wife, Pat, is working on an optimal one, which we are calling "BURN" -- it makes *.ASH files (ASCII-from-Hex) which can be UNBURNed at the receiving end. The ASHes are less than twice the size of the original *.EXE or *.ZIP or whatever, so you can E-Mail a zipped *.EXE file about as long as the original *.EXE file.

Date: Thu Jan 02, 1992 6:49 am PST
Subject: elements of a model

From: Bruce Nevin 920101 1226]

(Bill (Sat 14 Dec 1991 09:03:06))--

>You're proposing an element of a model here, so let's be sure what
>it is.

OK, if we are going to propose an element of a model, let's first carefully distinguish ontogenesis from function as a fait accompli.

Fait accompli: given a perceptual control hierarchy in place with recognizers for words according to their "argument requirement" and in their various morphophonemic variants or reductions (e.g. am, is, are, was, were, be, being, been, -'s, -'m, -'re, and zero as contextual variants of "be") and the *linguistic* context in which each variant occurs--selected according to linguistic context, NB, and not by semantic considerations--it all just works. No need for statistical studies or processes of classification, which are apt only for the antecedent processes of language acquisition. All examples of adult comprehension and production of language belong here, with the requisites of the model safely presumed into existence, thank you very much.

Ontogenesis: how can recognizers for words according to their argument requirement in fact come into existence? These appear, for many languages, to be N (primitive nouns--dog), On (operators with one N argument--sleep), Onn (eat), Onnn (give), Oo (operators with one operator, of any class, in its argument--be true), Ono (think), Oon (surprise), Ooo (cause). How can these recognizers learn how to identify various allomorphs as instances of the same noun or operator morpheme, and recognize the context of other morphemes in which each allomorph can or must occur (especially the zero allomorph which each morpheme has)? We obviously can't just presume them into existence. Even if we can't know for sure (at least at this stage) we must show that they are plausible outcomes of reasonable neurobiological processes available for the ontogenesis of the perceptual control system in general. All talk of classification, of whether or not statistical analysis is required, and of memory and learning, belongs here. It is by these means that the fait designated in the preceding paragraph becomes accompli. The appropriateness of any examples of adult comprehension and production of language here is partial and questionable at best, thank you very much.

We can avoid needless confusion by rendering unto the infant only examples of linguistic control suitable to the infant's linguistic capacities and unto the adult the fait accompli of a control hierarchy suitable for the examples of adult control of language that we so much like to toss about.

Let's stick with ontogenesis, now, since that is what you are asking me to help to pin down in modellable terms.

I said (911210):

>We don't care how frequently two things have cooccurred
>(statistical studies), but only *that* they have cooccurred.

Even the linguist only does a distributional analysis (what can occur with what) and not a statistical analysis (how frequently each combination occurs), and that was all I was claiming for the development of the perceptual control hierarchy.

Given that the prior existence and use of language in the child's community limits the cooccurrences that the child actually encounters, much that seems problematic at first blush simply goes away. The structure is immanent in the language. The child does not invent it. The child does not need a statistical analysis to construct it.

This was a point about the ontogenesis of linguistic control. Bill responded:

>You seem to be proposing that a single cooccurrance would suffice
>to establish the memory. . . . If I say "the and" one time, is
>that sufficient for you to establish this pair as a conventional
>cooccurrance, or does it have to happen more than once? When you
>think about this a little more, I think you'll agree that even the
>nonlinguist has to do some statistics (in effect) to establish
>which cooccurrances to take seriously and which to forget.
>Somehow the "memory" has to come to recognize significantly-
>frequent cooccurrances over some period of time. There's a
>process of learning involved, and it doesn't necessarily happen in
>one trial. In most cases one-trial learning would be a
>disadvantage.

Well, first off the words "the and" cooccur not only with each other but also with one or more intonation contours, as well as with other words.

1. You may say "the and" in a sentence in which you are referring to the "and" in some utterance. Example: "The `and' is unnecessary here, just use a comma."  The `and' in this

usage has stress befitting its role as a noun in the sentence, and linguistic and nonlinguistic context must of course support this interpretation for it to be intelligible. Q: "Which word do you want me to leave out?" A: "The and." There is an unstressed-stressed pattern over the two words that is found over many sequences beginning with "the" and ending with another word, with sometimes other words intervening and interrupting that pattern ("the little red book"). (There are other stress/intonation patterns too, but this is the one that occurs most freely in different contexts. NB: Not most frequently, but with the freest distribution--the fewest restrictions as to what can cooccur with it.)

2. More importantly, you may say "the and" with a break in the intonation pattern:

   Well, if you ask me, the--and I'm sure they'd enjoy this!--the rabbit should disappear into the mad hatter's hat.

   The two intonation contours, one interrupting the other and then the first resuming where it left off, are also linguistic elements coccurring with "the" and "and". This is represented by dashes in the example. Intonation contours are among the very earliest elements of language that children control in their babbling. Errors of many sorts are disclosed by breaks and interruptions of intonation contours.

3. You may say "The. And." with the intonation of reading words in a list. This is also a familiar intonation contour. It is found also in "`The,' and `and,' and . . ." and also in sentence-conjunction form "I say the word `the,' and I say the word `and,' and I say the word . . ." which might be taken as the source of the second and thence of the first as reductions.

If you can think of another sort of context in which a child might hear "the and" please tell me.

But I agree with you that (except for (1)) "the and" is not the sort of coocurrence that I want to end up controlled for in the fait accompli of language. How does it become less important and not remembered (in the memory immanent in a sequence recognizer) while the dependency between a word of an operator class and the word or words of its argument classes becomes more important and remembered by a sequence recognizer?

>You use the word "memory," but is that necessarily what you need
>for this model? Before you can remember a cooccurrance, you have
>to be able to recognize it as a cooccurrance. Something has to
>pick out of all the hundreds of things going on simultaneously a
>particular pair of perceptions to designate as a significant
>cooccurrance so that it can be reproduced later.

No, I meant just "memory." I am supposing that for a time at least one remembers all the perceptions of a situation, nonverbal and linguistic, and subsequently only an idealization or normalization or regularization of them. Is this category perception? Perhaps. Or perhaps the substitution of a remembered/imagined category exemplar for the detail of immediate experience. This idealization or normalization is typical of language. I believe it is also typical of much nonverbal perception, and the perception of exemplars or norms or ideal types is at the heart of what we call culture.

I would like to take up this idea of category perception in terms of an idealized exemplar that discrepant instances can satisfy, which has some currency I understand, but that is topic for another occasion.

As indicated above, I do agree with you that there is memory embodied in each higher-level recognizer, and this sort of memory also has an important role.

The difficulty for exposition (but the saving grace for the theory) is that these things are not learned sequentially, tidily in order, but pandaemonically, all at once. What I imagine is vaguely like a ping program in computer network technology. The ping function generates a test message that elicits a response from a target address. The device at the address is obligated to acknowledge a ping with a response. Another analogy is to up/down protocols with their "are you there" messages. So a recognizer receives an input that matches its reference signal and outputs a signal that could be perceptual input to other recognizers. One possibility, though messy: by associative memory and imagination, signals come back to its input function from collateral ECSs in the hierarchy that either strengthen signals already coming in to its input function or supply signals not actually derived from the environment. Neater would be for associative memory and imagination to be a function of higher-level ECSs. A higher-level recognizer expects perceptual input that is not actually being supplied from the environment. What sort of interaction between it and lower-level recognizers would lead one of them to supply the missing signal by imagination ("it must be there, everybody else says so") rather than generating an error message so that the organism goes about seeing that it gets picked up from the environment? Or, failing that, reorganizes to a different ECS (bouncing up and down on its toes in the wings, only its signal is not so strong as that of the ECS whose signal is preferred because currently stronger).

A perceptual signal comes into a nascent foo-recognizer. It sends out a signal as though (anthropomorphically speaking) it were transmitting the news "I perceived foo" (or just "foo"). Other perceptual signals experientially associated with the "foo" signal are being received by other recognizers (nascent or well established). Another nascent recognizer that is available (redundant perhaps, or else not yet committed to a particular role in the hierarchy) receives such a signal from the foo recognizer and from a bar recognizer. With repetition over time the foo recognizer becomes well established, as the bar recognizer was, and a higher-level foobar recognizer comes into being. Or perhaps an input function develops that combines the signals so that the nascent foo recognizer merges with the bar recognizer, resulting in a foobar recognizer. (How does an unspecialized grouping of nerve cells come to be a specialized recognizer for a particular collection of input signals? How does an input function for an elementary control system come into being? Just there lies a lot of the mystery of memory, I should judge.)

Actual cooccurrences of morphemes (words, affixes, intonation contours) with one another and with nonverbal perceptions are the basis for developing recognizers for classes of words. (The morpheme cooccurrences are more constrained, more regular, more mutually redundant than the nonverbal perceptions associated with them insofar as the former are conventional to a much greater degree than the latter.) You don't need to count cooccurrances of "the" with following "and" vs. cooccurrances of "the" with following "dog". "The" is a peculiarly restricted word in the language to which the child is exposed. A nascent word-sequence recognizer for words that follow "the" is plausible. It would call for or set up a recognizer for a class of words. Another recognizer for words that follow "a/an" would turn out to call for the same class of words. Another class of words ("adjectives") can intervene between "the" and the first class ("nouns"), and these two classes also cooccur (in noun-adjective order) with some form of "be" intervening. And so on. All that is needed is some means by which recognizers with identical or highly similar input requirements come to draw on the same subsidiary recognizers. The language itself provides numerous points of particularly obvious regularity at which the child can start, because of which cooccurring words are more susceptible to classification (and we know from Bruner that the social context provides a much more explicitly supportive LASS), and then one emergent word class supports the cultivation of others in a delightful reciprocal process of which the child does not tire for many years.

It is the higher-level recognizers that foster development of class-recognizers. I surmise that they start out life as word-pair recognizers. What are all the words that cooccur with this word? What is needed next is means for recognizers with intersecting or coincident input classes to pool their input requirements in a single class recognizer. It is the class recognizers that give the appearance of statistical analysis having been done.

I have not yet read any of the neural Darwinism literature. From what I have seen, it looks as though it might provide the sort of mechanisms that I am groping for here.

Skipping back to the fait accompli, it might be useful to emphasize what sorts of recognizers I am looking for.

I want a sequence recognizer that takes input from the recognizer for the word "sleep" and expects there to be any word of the N class in a position appropriate for a first argument. (This might not necessarily be a prior word--remember poetic inversion, as in "long slept he e'er he woke"--and of course it might be a zero allomorph of the argument word if e.g. the recognizer for a higher operator provides a signal based on parallelism among its arguments.)

I want a sequence recognizer that takes input from the recognizer for the word "dog" and expects any word of any of the operator classes On, Onn, Onnn, Ono, Oon. Maybe this is just a requirement for an ACK signal from one or more operator-recognizers saying "I assume you are in my argument." Such a signal would be in the *input* requirement for the word-recognizer. This amounts to saying that recognizing a word entails assurance of its structural (dependency) position relative to other words in the utterance, assurance that can only be given by satisfying a sequence-recognizer for those dependencies. Receiving more than one such signal in pandaemonium is fine, but at least one must be received else the word recognizer generates an error signal--which may be ignored, of course, if higher-level purposes are attained nonetheless. This sort of arrangement might account for how we accomodate error.

A couple of paragraphs more on how reductions work. I want the word recognizer to be able to get a signal from the sequence-recognizer for a conjunction, say, telling it that a zero allomorph of the word is OK (since the same word, indentified for same reference, cooccurs in a parallel argument position under the other argument of the conjunction--e.g. "I like pistachio and Mary chocolate" or "I like pistachio but not banana fudge"). Signals about context for reduction must come from various places to the word recognizer, which may use them or not, depending on the word (not all words are reduced or varied in shape in a given word- dependency context) and (at least for production of an utterance) depending on the gain on the word recognizer--the higher the gain, the less reduction permitted.

The morpheme recognizer for -ed on e.g. "fixed" must recognize it as a reduction of explicit morphemes asserting temporal relation something like "before my saying this." This must be available for all verbs and all past tense allomorphs, for break and the o of broke as well as for fix and -ed.

I certainly do not want a recognizer for every pairwise dependency of individual words, that would preclude novelty and creativity in word combination, or make it much more difficult than it manifestly is. Some input requirements I think should not be replicated for e.g. every word to which they apply, but rather to classes of words. The sequence recognizers for all operators whatsoever must be able to recognize absence of a first argument together with a certain intonation (written "!") as a zeroing of something like "I demand that you should ___" in the source of the imperative, as in "Go home!", "Sleep!" and even "Be heavy! (See what I care!)"

Fortunately, the word classes seem to be few and simple for purposes of the operator grammar. (The taxonomies of classifier words (collie, dog, animal, etc.) really seem to be a separate system, deserving of attention perhaps with respect to how we use language to help organize our nonverbal perceptions.)

It appears that there is just one sequence recognizer for each class of operators--one for Oo operators, one for Onnn operators, and so on. Here would be specified input requirements such as number of operators that generalize over the whole class. Many of the Ono and Oon operators are used only with a subclass of the N class of words, whose meaning might be stated "human or human-like" (these include think, believe, surprise, etc.). These subclasses of operators and the subclass of N would have (redundantly) separate recognizers with the requisite input requirements. But generally, I think, semantic matters are handled by the association of word dependency trees with dependencies among nonverbal perceptions. The latter do not I think form tidy dependency trees but rather a sticky, fluid, and volatile mesh of dependencies that surrounds and partly intersects that portion of dependencies that corresponds with the word dependencies.

In this respect I do agree with Martin and others who give primacy to semantic considerations in the interpretation of language. I differ with them in the claim that the construction of a set of candidate word-dependency trees precedes this interpretation, so swift and unconscious as not to be noticed by them, and control for associated meanings in memory and imagination acts to select one of these that seems appropriate. Some of the dependency trees proposed in this simple view of pandaemonium would be preposterous were we conscious of them. (Of course he doesn't mean the bone chased the dog, when we hear "The butcher, wanting the bone, chased the dog." Though this is in fact a poor example, given the interrupting intonation contour represented by the commas in the written form, but maybe it illustrates the idea.) I believe that we draw on this process to mend gaps where two imperfect dependency trees cannot be reconciled into one, and that this after the fact return to the materials of syntactic dependency is more accessible to consciousness, so that they believe that is the whole of it.

I would like next to present some examples of operator grammar analysis of an actual text along these lines, when I get some time again.

I hope this clarifies what I am proposing for a model, Bill. The sentence forms like N1 t V N2 were drawn from the early history of transformational analysis that led to operator grammar. I offered them only to support the plausibility of ontogenesis (in the sense above). I don't look for them in the model I am proposing, though the sequence recognizers for classes of operators and their arguments are analogous.

Bruce Nevin
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Date: Thu Jan 02, 1992 6:51 am PST
Subject: Humpty Dumpty

[From: Bruce Nevin 911231]

Martin (911213 17:10) lays on me the heavy charge of redefining "information" in the manner of Humpty Dumpty. Rather, I am declining to follow Humpty Dumpties of the past, and self-avowed Humpty Dumpties at that. To show this, I will quote from some of the originators of information theory (drawing many of the quotes and paraphrasing some lines of argument from Tom Ryckman's 1986 dissertation).

Hartley's original (1928) formulation does not define "information" but speaks of information becoming "more precise" with the successive selection of symbols from a specified repertoire. Shannon in his seminal 1949 papers on the "mathematical theory of communication" (a more apt name than "information theory," and still the preferred term in the UK) distinguishes clearly between meaning and his new measure of "amount of information." In various later writings Shannon repeatedly cautioned that the unexpectedness of a message need have no discernible connection with any sense of its semantic content or meaning, e.g. in von Foerster _Cybernetics_ (1952:219):

This kind of information is an ensemble concept. It is not a statement about a proposition, if you like, or a fact, but a statement about a probability measure of a large ensemble of statements or propositions or facts. It is a measure of a kind of dispersion of that probability distribution.

Weaver in his contribution to the 1949 collection of Shannon's papers said:

> The concept of information developed in this theory at first seems disappointing and bizarre--disappointing because it has nothing to do with meaning, and bizarre because it deals not with a single message but rather with the statistical character of a whole ensemble of messages, bizarre also because in these statistical terms the two words _information_ and _uncertainty_ find themselves partners.

The communications engineer MacKay wrote in his 1954 "Operational aspects of some fundamental concepts of human communication":

> Communications engineers have not developed a concept of information at all. They have developed a theory dealing explicitly with ohnly one particular feature or aspect of messages "carrying" information--their unexpectedness or surprise value. . . . Their measure of unextectedness, the average logarithm of the improbability of the message, . . . is hnot therefore _information_ but simply a particular measure of what they termed _amount-of-information_: (i.e.) the _minuteness of the selection which the message makes from the set or "ensemble" of all possible messages.

Cherry in his 1966 book _On human communication_ (p. 51) expresses regret that "the mathematical concepts stemming from Hartley have been called 'information' at all" since this new technical usage so little accords with the presystematic notion of information. Shannon also said as much (quoted by von Foerster loc cit):

> I think perhaps the word "information" is causing more trouble in this connection than it is worth, except that it is difficult to find another word that is anywhere near right. It should be kept solidly in mind that it is only a measure of the difficulty in transmitting the sequences that are produced by some information source.

Shannon and Weaver acknowledged that they had played the role of Humpty Dumpty in perversly redefining an intuitively understood term in a radically counterintuitive way. Shannon in his 1956 paper in _IRE Transactions_ entitled "The bandwagon" registered surprise and perhaps dismay at "the heady draught of general popularity" accorded to his measure of the amount of information transmitted in a channel, saying "information theory has in the last few years become something of a scientific bandwagon [and] . . . as a consequence, it has perhaps been ballooned to an importance beyond its actual accomplishments." In so doing they played into the desire of many to have placed in their hands mathematical tools whereby the presystematic concept of information could be at once given a more explicit basis and made more tractible for various projected manipulations. Probably instrumental in this was the promise, never fulfilled, though presaged even in Hartley's work, of banishing the subjective element from considerations of meaning.

Early enthusiasts, engaged and carried along apparently as much by their ambitions for science as by the suggestiveness and promise of the new theory, tried to apply it beyond its legitimate domain, but were unable to get past the fundamental impropriety of seeking an index of "meaning" or "content" or of qualitatively graded discriminable response with the measure of communication quanta that it defined.

This measure, which specifies only statistically average quantities from statistically stationary sources, "did not correlate with any interesting or relevant behavior of real perceivers, rememberers, or thinkers" (Haber, in his critique of Dretske in BBS, 1983). The information content of individual messages or situations cannot be specified by such statistical averages over all possible messages (or situations). As Dretske says (in his Precis in BBS 1983):

> Insofar as communication theory deals with quantities that are statistical _averages_ . . . it is _not_ dealing with information as it is normally understood. For information as it is ordinarily understood, and as it must figure in semantic and cognitive studies, is something associated with, and _only_ with, individual events (signals, structures, conditions).

The theory stipulates that statistically average quantities that this measure specifies must be from statistically stationary sources; that is, estimations of relative frequencies of occurrence of a given symbol must not depend upon the time at which the estimate was made. But this is scarcely a legitimate assumption (even as an idealization) for any living control system, which learns and whose responses change over time.

MacKay (in a 1969 postscript to the reprint of a 1950 paper, in _Information, Mechanism, and Meaning_) summarizes:

> It soon became clear that the biggest problem in applying Shannon's selective information measure to human information processing was to establish meaningful probabilities to be attached to the different possible signals or brain-states concerned. After a flourish of `applications of information theory' in psychology and biology which underrated the difficulty of this requirement, it has now come to be recognized that information theory has more to offer the biologist in terms of its qualitative concepts than of its quantitative measures, although these can sometimes be useful in setting upper or lower limits to information-processing performance."

Furthermore, we can see that Shannon "information" really has nothing whatsoever to do with meaning, and purposely so. Hartley's measure H = n log s (for all sequences of n symbols chosen from an alphabet of s symbols) requires that each symbol in succession in a sequence be chosen independently, ruling out the stochastic and recurrent dependency processes that characterize language (cf. Harris _Mathematical Structures of Language_). Hartley, indeed, was at some pains to eliminate "psychological factors" relating to the meaning or interpretation of symbols. The symbols and symbol sequences might be meaningful or meaningless--that is immaterial for purposes of determining channel capacity. Indeed, the imagery of a sender "mentally selecting" symbols and symbol sequences from an ensemble is inappropriate, though almost always employed in presentations of the theory. The same considerations apply to Shannon's extension of Hartley's work. Shannon "information" is good for channel capacity and that is all. It has not a thing to do with meaning.

> Shannon's measure H would be `just another measure' if it did not lead to the Channel Capacity Theorem. The fact that H leads to that remarkable insight gives H a definite status. In problems concerning coding of information for efficient transmission through restricted channels H is the natural measure. (J. Licklider, discussion, p. 24 of Cherry (ed.) _Information Theory: proceedings of a symposium, London 1955.)

I realize that it is difficult to countenance the suggestion that a cherished theoretical position is wrong. I must nonetheless do just that: the identification of communication theory, socalled information theory, with the usual sense of "information" is specious, misleading, pernicious, and wrong. We must accordingly distinguish carefully between Shannon information and information content or meaning. (A little further on, I will show that this is a tripartite distinction.)

I have asked you before to account for how the same measure of "amount of information" might be found for several disparate messages and situations. Suppose that a rigorous mathematical treatment of the range and probabilities of alternatives were carried out for a variety of messages and situations, such as a child opening a birthday gift, an executive answering the telephone, and a computer running a program leaving one state and entering another in response to keystrokes. Suppose that the same number n is computed, quite concidentally, for all of these situations and messages. This means that the channel capacity required to communicate each is the same. It does not, I think, mean that their information content is the same.

Regarding information in white noise, you say:

> a white noise can easily be a signal, and good criptography tries to create signals that look as much like white noise to an outside observer as possible. That there is a pattern of redundancy . . . in the signal is a fact known perhaps to all observers, but the shape of that pattern is known only to the recipient with the key.

But the information is in the near-white-noise _plus_the_key, not in white noise itself. Similarly for data compression. Bill affirms (911215.1700) that white noise can be a signal, giving the example of an engineer trying to determine the ambient noise level, relative to which the broadcast of a symphony is "noise". But it is the mere amplitude of the white noise that is the information here ("noise level") and not any information articulated in the white noise. I had already allowed that the presence or absence of white noise could be a binary signal. I will allow that amplitude modulation of white noise can in principle carry information, such as the audio signal of a symphony performance or a voice. But that information is not in the white noise, but rather in the amplitude of the white noise, and for it the white noise is only a carrier.

Shannon information of communication theory may be useful for some aspects of modelling, or for some metatheoretical considerations of upper and lower limits (and perhaps even requisites) for modelling--Wayne's question to you brings this into focus. It may seem useful rhetorically to redefine aspects of the perceptual control hierarchy in terms of communication theory, because of its continuing credibility and impressiveness. But is this anything more than the continuing inertial momentum of the bandwagon creaking on? As Haber (BBS 1983) pointed out, a purely quantitative measure can have no empirical significance unless it is relativized "to what the recipient of the signal already knows about the signal and about the circumstances of its reception." The specification of this knowledge and the relativization to it are both hopelessly beyond the capacity of communication theory.

Other theories are required for that: a theory of hierarchical perceptual control and a theory of information content. It is the latter which Harris provides us. I know of no other theory that comes remotely close. The logicism of Carnap and Bar Hillel, though taken up later by Hintikka, has been an arid dead end, the much heralded "calculus of information" not forthcoming. Dretske's bold promises rest on the cracked foundation of old premises. (For a comparison of the measure of "information" in communication theory with those in statistics, see Schuetzenberger's contribution to the 1955 symposium proceedings edited by Cherry, op. cit.)

Harris's theory of linguistic information does rest upon a correlation of redundancy (a hierarchy of constraints on combinability) with meaning. It is related to information theory. However, it proceeds in terms of relations of combinability of elements, rather than in terms of probabilities over ensembles of elements.

Harris's use of gradings of acceptability or likelihood as a criterion for transformational equivalence of homeomorphic sentences obscures this: it is a red herring. First, it applies only to one level of the hierarchy of redundancies. Second, it may be dispensed with in place of other criteria relating to subject- matter restrictions (sublanguage). No statistical or probabilistic treatment of the elements of language is required. (I will reply to Bill on this point separately, I hope tomorrow.)

It is essential to realize that the linguistic information in a sentence is not and cannot be identical with the perceptions it may evoke in memory and in imagination. We may be pleased to think of these associated nonverbal perceptions as the meaning of the sentence. Such meaning differs between any one language user and any other perceiving that sentence. The linguistic information in that sentence, however, is constant, a communal property of the speech community. The meanings in terms of idiosyncratically associated nonverbal perceptions are an interpretation of that linguistic information and, being grounded in that communal property, will have much in common from one language user to another--but also unpredictably much disparate. Communication has to do with evocation of perceptions in memory and imagination. The linguistic information in language is one social tool for doing this, which is efficient for some aspects of meaning but not for others.

We thus have Shannon information, which has nothing to do with meaning or information content; Harrisian linguistic information, which correlates with socially conventionalized aspects of meaning; and meaning in a general sense which inheres in the associations of perceptions in memory and imagination. The commonalities from one person to another in the latter are not nearly so great as our use of shared language comfortably leads us to believe. It is in the essence of our social being that we are charitable and overlook much.

Bill replies to you (911215.1700)

>I think I side with you here: an outside observer, contrary to
>Bruce, can't know what interpretation is being applied to incoming
>messages, and so can't determine the information content just by
>looking at the message. If the message says "yes", the outside
>observer can't know how much semantic information that word
>carries without knowing the question that the listener is trying
>to get answered.

Let us be careful here, and distinguish clearly between Shannon "information," linguistic information, and associative meaning. Let's look at each of these in turn with respect to this "yes."

_Shannon_information_, which is the "definition of information that has formed the core of [Martin's] research work for some 36 years" referred to in the passage to which you, Bill, are directly replying here, is irrelevant. That is the point of all the foregoing in this post.

The _linguistic_meaning_ of the word "yes" is always bound up in its role in the second half of a question-answer pair. Linguistically, when you hear "yes," you must remember or imagine a yes-no question, and with yes you must be able to include the affirmative member of the yes-no disjunction given in the question. Recall that the source of a yes-no question is a disjunction, e.g.

     I ask whether John left or [he did] not [leave]. -->
     I ask: did John leave [or not]? -->
     Did John leave?

Having heard or uttered this question, a heard or uttered "yes" refers to the positive alternative:

     Yes. <-- Yes, John left.

Just as a "no" refers to the negative alternative:

     No. <-- No, John did not leave.

Hearing only "yes" (or "no") and not remembering an explicitly asked question, we imagine the question, and from it we then fill out the remainder of the answer to go with the single word. If we can't, we ask "`Yes' what?".

In this reconstruction of the linguistic information intended by the person who said just "yes" (or "no") we draw upon present, remembered, and imagined perceptions all three. These _associative_meanings_ intertwine with the linguistic information in utterances in an inextricable, sticky mesh. But that semantic mesh is a highly variable flux, differing as I have said from person to person and from one occasion to another even for the same person (acceding to the comfortable fiction of "same person," which must occasion a smile from any good Buddhist). What is relatively constant is the linguistic information immanent in the utterance, which functions for us somewhat as a skeiner's bob on which we try to loop and sort the strands of perceptual life. The inherited agreements implicit in language give us a leg up in coming to agreement about our perceptions, and about their valuations and orderings in our mutual transactions (themselves perceptions, of course). The structure of language and the linguistic information in any particular utterance also does vary across persons (or rather, across speech communities) and does change through time, but at a rate slow enough and to a degree slight enough that we can mostly contrive to overlook it, in the customary charitable oversight that I have suggested is essential (probably biologically innate) to our social nature as humans, primates, mammals, vertebrates.

By this charitable accomodation we also reconstruct another's linguistic intent by ignoring and patching over hesitations, false starts, mispronunciations, substitutions of wrong words, and so on. That we reconstruct and normalize the linguistic information in their utterance as well as their perceptual intent (associative meaning) is shown by the immediacy and confidence with which we repeat what they said, with the repair, if e.g. someone asks "what did she say?"

It is also amusing sometimes to lapse from charity and poke fun, for example at the entrepreneur at Charles and Dianne's wedding some years hence, whose sign said:

SOUVENIR PEN'S

He was proud of his apostrophe, I suppose, or proud of demonstrating control of this arcane device of orthography. Anyway, he made a good, big one--so big that it looked like a letter "I". Just so with Rick's critique of "in" instead of "into" in the sign written by the janitor tired of picking pee-soaked trash out of the urinal. Here, Rick, you are on shaky ground. "In" and "into" have long been of closely allied meaning and are interchangeable in many contexts, viz.:

I told you to put your toys in the box.
Toss your toys in the box now!

(Patter to pitcher in baseball game):
Throw it in there!

Where did he go? He went in there. What? I said he went in that room. Look in there for yourself. No, I said to look in _that_ room, through the window here. I'm afraid the conversation about his ex-wife drove him in there.

The punctilious could insist on "into" for each "in" here, though it is a bit awkward in standard English before "there." In some dialects, the use of "into" in "don't throw paper into the urinal" is as punctilious sounding as "Throw it right into there, pitcher!" would sound in a baseball game. But even if the janitor made a mistake (is not a member of a speech community in which this is normal) the error is only commission of an avoidable ambiguity--the alternative interpretation of standing in the urinal to throw things, or of throwing things one finds in the urinal (plus additional interpretations a machine parse would turn up). The fact that substituting "onto" or "with" or "after" becomes progressively less intelligible in context shows the primacy (in the reading of the sign) of linguistic information, of which associated perceptual "meanings" are an interpretation.

This has nothing to do with your "(ug) grammar," Rick, a set of rules such as for use of apostrophe in writing or not saying "ain't" or "him and me seen it." Of such tiny shibboleths are mighty social barriers made, indeed, but they are judgements upon dialects spoken by members of less prestigious social classes and have nothing to do with linguistic information or meaning. The concern here is with structure that is in language, and that can be determined by scientific study of any dialect of any language. "Ghetto talk" is entirely as capable of coherent, logical, internally consistent and informative discourse as any other language or dialect, from Cato to Gore Vidal. The fact that not all people use their language so all the time is a separate matter, and also applies to all languages and dialects whatsoever.

A little test is in order perhaps, of the hypothesis that semantics and pragmatics are primary and syntax is only used to disambiguate difficult cases. I have offered this before, but it apparently passed without notice. I'll turn up the gain a little. One aspect of the structure of English is that the basic word order is subject-verb- object (with some alternative orderings possible for emphasis and stylistic nuance). In this SVO ordering, prepositions precede their objects ("on the table," not "table on") just as verbs do, and adjectives precede the words they modify.

In other languages, other basic word orders are found--VSO, SOV, VOS, OSV, and OVS, all the logical possibilities. There are corresponding typical differences for position of prepositions (or equivalent) and of modifiers, among other things.

For our little test, let's just change the SVO order of English to VSO (with prepositions and adjectives following). A simple change, trivial really. All the vocabulary will remain English. It should be easy then to determine the meaning of sentences with this changed word order, if

> almost all language understanding is based on situation-related meaning, and that syntactic constructions are used only as a backup in case of ambiguity in the possible semantics and pragmatics.

(Martin 9112209 14:25.) Think I find will you easy accomplish to very so not. Example for, look might sentences few the seen have you previously way this:

> Go he where? Thereinto went he. What? Went he room into that said I. Look thereinto selfyour for. No, look room _that_ into to, window the here through said I. Afraid am I drove conversation the ex-wife his about him thereinto.

Please read the above three paragraphs rapidly, in a clear, well articulated way, with appropriate intonation, to a number of people, having advised them that you are going to quote a conversation to them and that they are then to repeat the content of the conversation back to you. I am sure they will have no trouble with it. The words are quite unambiguous (I even used "into"). There should be no need to rely on syntax to understand this passage, right?. Or perhaps the following words of a mother to her child might be easier:

Told I you put toys your box the into to.
Now toss toys your box the into!

This is my view of what is actually going on.

* The possible dependencies among words are determined swiftly and unconsciously prior to any association with remembered and imagined perceptions.

* Most of the contenders are ruled out on purely language-related grounds--there are only so many ways the possible subtrees can fit into a single dependency tree spanning the whole sentence defined by the intonation contour (or punctuation).

* Those that remain are structural ambiguities, and the selection among them is semantic.

The point is that in the case of a well-formed utterance the role of associative meaning is selective, not determinative. Where there are errors, lapses, etc. we can't come up with a dependency tree that completely spans the sentence, and the appeal to associative meanings helps us to repair the linguistic information, that is, the words and word dependencies. (Typically, intonation shows that we are dealing with two or more sentence fragments, and we fill out each as best we can, then make a coherent discourse fragment of them for ourselves, often as a basis for clarifying questions.) But this must be the topic of a separate post, this one is already too long.

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Date: Thu Jan 02, 1992 9:09 am PST
Subject: generative

[From: Bruce Nevin (920102 1008)]

(Gary Cziko 920101) --

>Also, you use the word "generative" to refer model building at the n-1
>level. It is interesting that this is the exact same word that Chomsky and
>post-Chomskyan linguists use to differentiate their approach to
>understanding language from the descriptivist structural linguists which
>preceded them.

Mead also uses the term "generative" in a similar sense, Chuck Tucker or Clark McPhail could elaborate.

The term in linguistics is drawn from mathematics, where "to generate" means to define a set or structure by applying rules or operations to axiomatically defined objects. In Generative grammar, the initial objects are not at a lower level defined by a prior science, but are abstractions defined for the purpose of accounting for observed sentences. The essential difference appears to me to be that Greg is discussing generative models, but Generative grammar does not proceed by modelling.

As a differentiator in linguistics, "Generative" is a mere trademark (hence my capitalizing it). Any descriptive grammar is also generative in the sense that Chomsky used the term. In particular, the much maligned but seldom actually read methodological handbook by Harris explicitly calls for ordered rules organized so as to generate the set of utterances of a language. (Chomsky was Harris's student when this was written and helped to proofread it.) What is at stake in the comparison of Generative grammars and other sorts of grammars is not whether or not they are "generative," but rather how badly the grammar leaks--how many exceptions it fails to account for, how many productions it predicts are in the language but language users deny. Since there has never been a whole grammar of this school with reasonably complete coverage approximating that of, e.g. Jespersen's _A grammar of English on Historical Principles_, the point remains moot. In the literature one sees only isolated examples selected or concocted to illustrate things that a particular proposal for a part of the grammar handles especially well as contrasted with less adequate treatment by rival proposals. A compendious summary was attempted at UCLA but foundered with the next calamitous fracture of Generative theory. There has been no comprehensive treatment of any language from a GB perspective that I know of, but I could be wrong; in any case, Chomsky is even now in process of supplanting GB theory with the next Standard Theory of Generative Grammar. If any comprehensive Generative grammar of English is published, it could be compared with Harris's _A Grammar of English on Mathematical Principles_, which is of course generative (lower-case).

Bruce Nevin
bn@bbn.com

Date: Thu Jan 02, 1992 10:16 am PST
Subject: linguistic varia

Re Powers:

>One process adjusts utterances according to the meanings they evoke in
>the speaker as they are uttered (or in imagination prior to utterance);
>this leads to editing on the fly and other means of adjusting the meaning
>of an utterance to make it match the meaning intended to be communicated.

But this adjustment basically consists of putting in something where there was nothing, which is maybe significantly different from the more usual cases of feedback control.

>The other process adjusts utterances according to whatever linguistic
>conventions the speaker knows and cares about. The linguistic forms are
>perceived, and if they are in error, the control systems adjust the

>developing sentence where possible or force a restart. You see why I'm

I don't know about this one. The problem as I see it is that there is not that most error-editing (Bill Labov, the numero uno observational linguist, tells us that 75% of speech is grammatical as is), & once a mistake is made there is not that much that can be done to fix it. My recollection from when my kids were younger is that they could say impressively complicated things `I want to push Owen while being carried', but that if anything went wrong, they had to restart the whole communication from scratch, including securing the channel. E.g.:

  kid: Daddy?
  dad: what?
  kid: do you remember when we went the park and
         [crash]
  kid: Daddy?
  dad: what?
  kid: Do you remember when we went to the park and saw a koala and
        [crash]
  kid: Daddy?
  dad: what?
  kid: Do you remember when we went to the park and saw a koala and
         it was climbing up the tree ...

This doesn't look like an interesting control system to me. I see the problem as one of putting out complicated novel performances mostly without errors, but also (to make it easier) without having to oppose any disturbances.

>>There are at least two issues to be dealt with: a) what is the
>>structure of the representations in the `reference signal' (desire box)
>>and `input signal' (belief box) b) how are the actions calculated to get
>>them converged. (a) is something which linguistics can contribute to,
>>but not (b), I would say.
>
>You're right about the issues. I hope you're willing to put in some
>effort on (b), which is the "reverse engineering" part of the modeling
>problem. Perhaps linguistics as it is can't handle (b), but let's think
>in terms of linguistics as it might be, given control theory. I think
>some study of how people construct sentences, with this problem in mind,
>might show us how people actually detect and correct errors. Then at
>least we'd know what we have to model.

I see both (a) and (b) as involving reverse engineering (it is not at all obvious what the correct representations are - all we get to see overtly is the string of words, & most of what I've done in syntax over the last 12 years is show how certain forms of representations make certain grammatical patterns less surprising than they would otherwise be. The extant results of linguistics bear on (a) but not (b) - my thoughts on (b) come from browsing in AI and logic programming literature, not linguistics. My belief about syntax is that mostly there *aren't* errors to correct, and when there are, they mostly aren't corrected, but one starts again from scratch. At the level of content, however, a perception of `I haven't said what I meant yet' presumably plays a major causal role, at least in some speech styles (Anglo-Saxon male, task oriented, etc.). Such is my guess, at any rate.

>It's not necessary, in HCT, for any system to do a survey of
>possibilities. A control system of a given level doesn't care what lower-
>level perceptions are actually employed, as long as they add up to the
>right state of the perception at the higher level. What you need is just
>a map showing, for each value of error signal, some alteration in a

We'll have to see how this works out, but my guess is that for lots of problems, the needed maps don't exist, & exploration of possibilities is necessary, with its attendent problems. E.g. when I set out for the university, I might go by car or bicycle, and we haven't found any satisfory way to decide which except via an agonizing consideration of what has do be done by who. By the time there are perceivable errors, it's too late.

>Jackendorf's (sp?)

Jackendoff (presumably North Slavic rather than German). He certainly doesn't have any idea about control, but, I'd say, an interesting line on higher-level perception/cognition.

Avery Andrews

Date: Thu Jan 02, 1992 10:19 am PST
Subject: generativity

A point perhaps worth making about `generativity' in grammar is that it is more often discussed than achieved. Chomsky substantially overestimated the extent to which early TG was generative, & so did Harris for his own proposals. It wasn't till the late seventies that linguistic theories began to appear in which it wasn't absurd to try to implement the analyses one was writing about. Most GB (`Government-Binding' theory, Chomsky's now former flagship) is not actually generative, although there are now (finally) a reasonable number of people trying to change this (Bob Berwick at the MIT AI lab being the leading figure).

  Avery Andrews

Date: Thu Jan 02, 1992 11:08 am PST
Subject: Re: Control of behavior

From Ken Hacker [010292]

Re: Marken's questions about behavior control:

I am presently reviewing some writings of Noam Chomsky, both on language and on politics (and their intersections). Chomsky is staunchly opposed to behavior control and argues that the social sciences have become tools of social manipulation and control (not a positive sense of control!). He says that the scientific study of human behavior has given better techniques of coercion, not liberation. Moreover, he argues that these sciences have given an ideological cover to those who wish to manipulate the most -- that cover being science. Chomsky argues the appeal of behaviorism is the ability to generate many studies and generalizations from just a few axioms.

I hope this might help you somewhat. I have been lurking on this hotline for months, trying to catch up with the conversations, and never seeming to find a point of equilibrium to jump on to. So, with this perturbation, maybe I can participate a little.

Ken Hacker, Dept. of Communication Studies, New Mexico State University

Date: Thu Jan 02, 1992 11:21 am PST
Avery Andrews (Fri, 3 Jan 1992 05:05:42 EST)

It is very good to have another linguist and a sympathetic spokesman for one or more of the Generativist perspectives actively involved. I suggested to Bob Yates some time back that there is a fine opportunity here to work on a real (read modelling) foundation for the Generative program re language acquisition, innateness, and so on. I won't, as I have never found a way to believe in that program. I hope you will take up the challenge, as you seem to be.

> `generativity' in grammar is . . .
>more often discussed than achieved. Chomsky substantially
>overestimated the extent to which early TG was generative, & so did
>Harris for his own proposals. It wasn't till the late seventies
>that linguistic theories began to appear in which it wasn't absurd
>to try to implement the analyses one was writing about. Most GB
>(`Government-Binding' theory, Chomsky's now former flagship) is
>not actually generative, although there are now (finally) a reasonable
>number of people trying to change this (Bob Berwick at the MIT
>AI lab being the leading figure).

I would add Tony Kroch at Penn, who has been working with Aravind Joshi to make of the latter's TAGs (Tree-adjoining grammars) a unifying framework for the divergent varieties of GB theory.

You seem to be identifying "generative" with "successfully implemented on a computer." Implementability has been a telling embarrassment for Generative theory over the years, and a criterion on which the Harrisians have consistently done well since the the early work by Danuta Hiz and Joshi in the 1950s and the inception of the Linguistic String Project by Sager at NYU. Stephen Johnson has written a Prolog implementation of Harris's operator grammar. (I could send you his NYU dissertation _An analyzer for the information content of sentences_.)

This is an interesting and probably uncharitable identification of criterion with definition. In my view, there will never be a perfectly generative grammar of a language, because there is no such unitary thing called "the grammar" nor such a unitary thing as "the language" of which there could be a grammar. Some recent posts to the Linguist list go into my reasons for saying this. Unfortunately, the notion of a homogeneous, monolithic "the language" and "the correct grammar of the language" are deeply embedded in the presuppositions as well as the rhetoric of Generative linguistics.

As each language learner creates an idealized model of the languages and dialects she acquires, and controls her perceptions of language for conformity to this norm, it is understandable why the perception of "the language" and "the grammar of that language" should seem natural and obvious. I believe this perspective aligns well with Control Theory.

On the generativity of grammars, I prefer the formulation that I offered before, that any grammar is generative, it is simply a question of how adequately generative it is. This more charitable understanding even works with respect to the criterion of implementability: there have been many computer implementations of Generative theory. They just haven't performed worth spit.

Looking forward to some productive conversations,

Bruce Nevin
bn@bbn.com

Date: Thu Jan 02, 1992 11:24 am PST
Subject: Re: While the funny money holds out....

[from Gary Cziko 920102a]

Greg Williams (920102)

Thanks for you response to my query about reductionism. I can follow your arguments and find them appealing and illuminating. There was some talk about this a while back on the net, particularly the how and why (and "what else") aspects of explanation.

While I can see how the distinction between how and why works in PCT, I still have trouble applying this distinction to physical phemonena. Take falling objects, for example. What type of explanation did Newton give us? It seemed that he explained nicely how objects fall, but was unable to explain why gravity acted as it did ("Hyotheses non fingo" and all that).

But I do find very useful your notion that true explanation requires moving out of the current level, either to n-1 for how or n+1 for why with no skipping allowed.

>P.S. Is there still interest in an IBM DOS binary-to-ASCII file conversion
>program? My wife, Pat, is working on an optimal one, which we are calling
>"BURN" -- it makes *.ASH files (ASCII-from-Hex) which can be UNBURNed at the
>receiving end. The ASHes are less than twice the size of the original *.EXE or
>*.ZIP or whatever, so you can E-Mail a zipped *.EXE file about as long as the
>original *.EXE file.

There is already such a set of programs called uuencode and uudecode. I have uuencoded .exe files into ASCII files which are only 40% larger than the original .exe file. Bill Powers' also has these programs and has used them to send me some programs but we have been having problems. Perhaps you already know of these programs and yours will be better in some way.

I am planning to make more program files available to CSGnetters through Bill Silvert's file server in Halifax, but am still working out some bugs. Look for Bill Powers' Demo1, Demo1 and your and Bill's "Little Man" arm demo coming soon to a file server near you!

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Date: Thu Jan 02, 1992 12:14 pm PST
Subject: Kolbe

To: Tom Bourbon and other CSGnet people
From: David Goldstein
Subject: Kolbe
Date: 01/02/92

You seem to be under the impression that I think that Kolbe's ideas are similar to PCT.

I do not.

However, I think that there may be some value in what she has done which would be of interest to us.

I basically ignore her thermodynamic model of what she has done. As you have pointed out, it is very similar to the one of Freud.

I also ignore her type-like discussions which are certainly in her book.

What is left? I think she has identified some principle level perceptions which may be useful building blocks in self-image perceptions.

I also think that it is a good idea to friendly towards people who express some interest in learning how PCT might apply to what they are doing.

Rick has made a useful distinction between control facts and control theory. I think that Kolbe has identified some control facts.

Best,
David Goldstein
goldstein@saturn.glassboro.edu

Date: Thu Jan 02, 1992 2:49 pm PST
Subject: Still hasn't run out....

From Greg Williams

Gary Cziko (920102a):

>While I can see how the distinction between how and why works in PCT, I
>still have trouble applying this distinction to physical phemonena. Take
>falling objects, for example. What type of explanation did Newton give us?
>It seemed that he explained nicely how objects fall, but was unable to
>explain why gravity acted as it did ("Hyotheses non fingo" and all that).

Yes, indeed, Newton was the Skinner of the early 1600s. He DESCRIBED the phenomena at the level of the phenomena. His "g" was an unexplained hypothesis concocted to suit the description, similar to Skinner's "reinforcement" -- both are JUST THERE (the latter, only sometimes there, and Skinner was unable to predict when it would be and wouldn't be [give me your rat for a minute...], but control theorists are working toward such predictions, which requires dealing with the "inner" organismic hierarchy.

>There is already such a set of programs called uuencode and uudecode. I
>have uuencoded .exe files into ASCII files which are only 40% larger than
>the original .exe file.

We haven't seen uu.... Maybe Pat is reinventing the wheel (but has only spent less than a day on it so far). Can anyone tell us: (1) How big are uu...? The Turbo C versions (medium model) of Pat's are about 10KB each, or about 7KB when LZEXEd. MUCH smaller, eventually, in assembly versions. (2) Do uu... include checksum info to warn the receiver of a corrupt executable in case of line noise? Ours would. (3) Are uu... optimal in the sense of encoding the most frequent 94 byte values in the original into single byte ASCII values? Ours would, with optional table-driven encoding for quickness and for very small files.

It's been fun, but Pat's willing to quit and do other things if she's been scooped. After all, we named it BURN, not FLAME!

Best wishes for the rest of the already old year,

Greg

Date: Thu Jan 02, 1992 4:19 pm PST
Subject: Reverse engineering; reductionism

[From Bill Powers (920102.1600)]

Gary Cziko (920101) --

Re reverse engineering:

Reverse engineering is a term from (I believe) the semiconductor industry. It refers to duplicating the function of someone else's integrated circuit. What with copyrights and patent laws, modern reverse engineering gets pretty complex. One team analyzes the function of the competitor's chip, and prepares a specification stating the relationships between inputs and outputs (and other aspects of visible behavior) that the "unknown" chip creates. This specification is then passed on to a design team which is never given access to the chip itself, only to the specification. The design team is never allowed to communicate directly with the analysis team. From the spec alone, the design team generates a completely new chip design, from scratch, that will accomplish exactly the specified functions. I'm sure there has to be some cheating -- the design team has to know that the specs describe a computer, for example, and not a sewing machine.

At any rate, the result is a new chip that can be plugged into the same socket that the original chip occupies, and that works exactly the same way down to the last detail of functioning. This is the ultimate in the method of modeling.

In fact, the final chip may not accomplish the functions in exactly the same way the original did. Sometimes the new chip proves to perform some functions more efficiently than the original - in fewer steps, or faster. Presumably, if those aspects of functioning had been part of the spec, the design team could have deliberately slowed some circuit operations and matched the slowness of the original too! But the design team, prior to releasing its product, never can know whether it has accomplished the functions in the same detailed way that the original does. In the final comparison, it is often found that some functions were re-invented exactly as in the original, while others do the same things in a different way. That is what is hoped for, what avoids a suit for patent infringement.

This is basically what I am arguing with Wayne Hershberger about. We are trying to reverse-engineer evolution (or whomever you want to blame). In doing so we come up with a model of underlying design features constituting a system that interacts with its environment just as real organisms do. Of course in doing this we try to reproduce only those functions we understand, and we ignore many others such as skin color, weight, exact lengths of appendages, and so on through a long list of "unimportant" parameters. As initial models succeed, we bring in more detailed parameters to match, even to the level of neural functions in a few cases.

But we can never know that we have accomplished something in the same way that an organism accomplishes it, in every detail. For that matter, we have no reason to think that every organism of a given species accomplishes its functions in the same way as other organisms of the same species. Judging from the very large differences in brain anatomy that exist from one person to another, in fact, it's unlikely that all people are internally organized in the same way even if they behave in roughly the same way. The brain is plastic and its organization is influenced by the experiences of a single lifetime. Our reverse engineering is fundamentally limited by this fact: no one model can ever reproduce to the last detail the inner functioning of all examples of any kind of higher organism, because the originals are not all designed in exactly the same way. We will always be limited to modeling the "general idea" behind an organism, because that is the limit of consistency in the originals. The method of modeling is primarily a method of understanding individuals, and only secondarily a way of saying general things about all individuals. Models must always contain parameters that can be adjusted to fit the "general idea" to a specific organism.

This, naturally, has some serious implications concerning the nature of scientific research into human nature. It's usually assumed that one is dealing with a standard instance of homo sapiens -- the very idea of assigning such a term to the whole human race is to assert that fundamentally we are all the same. In the psychology lab, great attention has been paid to using a standard animal model -- the Sprague-Dawley rat, during my formative years. If you have a standard rat or a standard person, you should get standard responses to standard stimuli. If any human being is as good an example of homo sapiens as any other, you can study groups of people as interchangeable units, drawing generalizations from the data which you assume to be measures of common underlying properties fuzzed out by uncontrolled stimuli.

But what if, below some level of observation, there ARE no common underlying properties? Then the whole rationale of statistical studies of populations collapses. The specification team can't come up with a spec that fits all instances of the chip that is to be reverse-engineered. All they can describe, for each parameter, is the average spec. As Russell Akoff said in a lecture that Dag Forssell has transcribed, there's no way to design the optimum human being by combining the optimum spec for each function making up the person. This would be like trying to build a perfect car by using the engine of a Rolls-Royce, the suspension of a Ferrari, the body of a Chevette, the carburetor of a Chevrolet, and so on. The functions all have to work together in a single person; the final workable form of each function depends on the final forms of all the other functions. Each part of a person is adapted to all the other parts of the same person, not to the same parts as they are manifested in other individuals. And the process of mutual interadaptation never ceases.

Re Reductionism:

You say

>What saves PCT from reductionism (for me, anyway) is the idea that
>higher levels of organization specify reference values for the lower
>levels. But what is the comparable structure in physics or biology?

There's also the matter of one level of perception depending on lower levels of perceptions. A relationship such as "above" in the spatial sense depends on perceptions of independent objects, each with its own spatial location. A reductionist could say that spatial relationships, therefore, are "nothing but" spatial locations, because any spatial relationship can be analyzed into some specific set of locations. This claim overlooks the fact that there are infinite combinations of locations that would qualify as instances of "above," and even more combinations that would not. In making the reduction from the relationship to the elements of the relationship, one has discarded something -- the very something that distinguishes valid instances of "above" from invalid ones. This something is the structure of the locations, the structure that is recognized in a human being as a relationship invariant over all instances of it.

In biology, reductionism appears as analyzing some complex function into its constituent sub-functions. Pointing a finger at something reduces to adjustments of tensions in specific muscles. Therefore pointing is "nothing but" tensing muscles in certain patterns of contraction. It is perfectly true that any instance of pointing entails muscles in specific states of contraction -- but it is not true that all instances of pointing, even those that look the same, entail the same specific patterns of muscle tension. Something has been discarded in the reduction, in this case the organization of the control systems that adjust the muscle tensions differently in various circumstances so as to create the same sensed consequence, which we see as the pointing behavior.

Even in physics, reductionism fails for the same reasons. The phenomenon of gravitational attraction, for example, reduces to a set of point-masses each attracting all other point-masses according to the inverse square of their separation and the product of their masses. But "separation" is a notion that goes beyond mass and force: it is an abstraction, a feature of intangible spatial arrangement, that mysteriously influences the forces between masses. Doing away with this structural feature of gravitation would leave us with tangible forces and masses, but would discard the intangible inverse-square law.

Avery Andrews (920101) --

I use the term "generative model" as Humberto Maturana defined it (perhaps following someone else). A generative model is one that will reproduce the phenomenon of interest by operating strictly from the interplay of its own properties. A generative model of control behavior is a control system with an input function, a comparator, and an output function, in an environment that links output to input in a specific way. There is no component in a control system model that "controls." Control is the result of operation of a system with these functions in it, connected as specified by the control-system model, and operating as dictated by the input-output properties of each component.

So given inputs, constraints, and parameters, a generative model must always produce some kind of behavior. We can't necessarily anticipate what such a model will do, but whatever it does is rigidly set by the properties we have given it, and by the surroundings with which it interacts. We hope that the behavior of the model will resemble the phenomenon we're trying to explain. If it doesn't (and few models do, the first time they are set in motion), we have to modify the model. That's how models grow and improve.

Off to see Mary in the hospital (again!!). Blood clot this time. Damn.

Best to all,

Bill P.

Date: Thu Jan 02, 1992 7:00 pm PST
Subject: generativity, etc.

Re Bruce Nevin (Thu, 2 Jan 1992 13:59:39 EST)

Yes, I think this could be fun.

>You seem to be identifying "generative" with "successfully implemented
>on a computer." ...

>This is an interesting and probably uncharitable identification of
>criterion with definition.

A more considered statement (criterion, I guess) would be that a model is generative a la Chomsky (c-generative, as distinct from m-generative?) to the extent that somebody can implement it without having to extend or modify it (and the implementation doesn't have to be practical or efficient). `Move Alpha' (a GB arcanity) is presumably implementable without further invention (but not efficiently by known methods, according to something I read recently), but not `Avoid Pronoun', in the form that this notion is presented in Chomsky's *Lectures on Government and Binding*.

>In my view, there will never be a perfectly
>generative grammar of a language, because there is no such unitary thing
>called "the grammar" nor such a unitary thing as "the language" of which
>there could be a grammar.

I have no problem with this, but I do think that `languages' and `grammars' are useful idealizations for some purposes. E.g. `In Kayardild, one puts the locative case-marker on the end of every word in a complement clause' (K is a pretty wierd language - see the article by Nick Evans and Alan Dench in the Australian Journal of Linguistics (1988)).
And I don't find `monolithicity' and `correctness' to be amongst the essential assumptions. Nor perfection amongst the appropriate goals, at least for now.

There are features of generative theories (especially the TG original) that would tend to suggest these ideas, but I don't find
them to be foundational assumptions. For example, as to monolithicity, modern GT's tend to be higher `modular', with various different kinds of rules and principles interacting to produce the final result. So these GT's are a lot closer to the idea that language use involve the interaction of various different cognitive capacities, which might interact in different combinations to perform other tasks. This idea entails such things as a blurring of the (Chomskyan) competence/performance distinction, which I mumbled about a bit in the discussion of `infinite languages' on the Linguist newsletter.

> there have been
>many computer implementations of Generative theory. They just haven't
>performed worth spit.

True, but it doesn't alter the point that a great deal of the work that goes on is conducted at too vague a level to be implemented by a mere programmer rather than a linguist-programmer. Like when I asked around MIT to try to find out how `Move Alpha in the Lexicon' (in a paper by Keyser and Roeper about middle verbs in LI some years back) was actually supposed to work, and the best I could do was to get Beth Levin (I think) to tell me that she didn't think anyone around the place knew, including the authors. And what gets implemented, when anything is, is usually some kind of cheap and nasty approximation to the theory it's supposed to be an implementation of. For example, the original transformational parsers in the sixties did not work off transformations as they would be written by the Ling 101 students for their homeworks, but off hand-crafted `reverse transformations' concocted by teams of graduate students. And they had ad-hoc PS rules for surface-structures, etc.

And I personally have no idea how lousily the vaguely Chomsky-inspired
(including LFG, GPSP, HPSG) implementations perform relative to ones with
other antecedents. I've written an LFG system that I find performs
reasonably well for teaching baby syntax (from a fairly descriptive
point of view - basic phrase structure, cross-referencing, etc.), but
you certainly couldn't use it for any `industrial' purpose. And there's
still plenty of kinds of basic stuff that it doesn't deal with properly
(such as getting the Aux into first or second position in Warlpiri).

I think it's a serious problem in evaluating claims about computational
linguistics results that you typically can't take the programs home
to play with on your Mac/PC, so it's very hard to be sure what they
are actually able to do. Then the current vogue for accentuating the positive
and downplaying the problems (Noam's fault, I think) does not help much.

And I'd be quite interested in looking at the Johnson thesis.
By snail I'm:

  Linguistics, The Faculties
  ANU, PO Box 4
  Canberra ACT 2601

Or, is email/ftp a posibility?

Date: Thu Jan 02, 1992 7:22 pm PST
Subject: Re: Still hasn't run out....

[from Gary Cziko 920102b]

Greg Williams (920102a):

>Yes, indeed, Newton was the Skinner of the early 1600s. He DESCRIBED the
>phenomena at the level of the phenomena. His "g" was an unexplained hypothesis
>concocted to suit the description, similar to Skinner's "reinforcement" --
>both are JUST THERE (the latter, only sometimes there, and Skinner was unable
>to predict when it would be and wouldn't be [give me your rat for a
>minute...], but control theorists are working toward such predictions, which
>requires dealing with the "inner" organismic hierarchy.

Interesting stuff, this Newton as Skinner viewpoint. If this is so, does
modern physics differ from this "just there" approach to science? Where is
the ""inner" organismic hierarchy" in modern physics?

==

>(1) How big are uu...?

My version of uuencode.exe is ; uudecode.exe is

>(2) Do uu...include checksum info to warn the receiver of a corrupt >executable
 in case of
>line noise?

I'm not sure, but it must check something since there is information about
how many preceding lines and/or characters there should be at various

points in the ASCII encoded file. I bet that Bill Silvert or Bill Powers could tell you more.

>(3) Are uu... optimal in the sense of encoding the
>most frequent 94 byte values in the original into single byte ASCII values?

No idea. Sounds like a neat idea, though. I bet Bruce Nevin would like it, too (sounds downright Harrisian!). But why stop at 94?--Gary

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Date: Fri Jan 03, 1992 5:50 am PST
Subject: Mary;Control of Beh.;Kolbe

From Tom Bourbon [920103 -- 1:51]

Bill Powers -- Please tell Mary that Betty and I send our love and our wishes that she is out of the hospital soon. Is the clot a consequence of the accident in Illinois?

Rick Marken. One of the most articulate contemporary statements I have seen on the importance of prediction and control, and the primacy of control, in a science of behavior is in:

Steven C. Hayes & Aaron J. Brownstein (1986). Mentalism, Behavior-Behavior Relations, and a Behavior-Analytic View of the Purposes of Science, _The Behavior Analyst_, _9_, 175-190.
I cite it extensively in the manuscript on "control of others." You might find it useful in your writing.

On page 175, the authors say, "We will attempt to show that an emphasis on prediction and control is not arbitrary in behavior analysis because it is a _necessary_ part of successful forms of the philosophy that underlies behavior-analytic theorizing."
In a lengthy section titled, "The purposes of science: Prediction and control," are two subsections: "The emphasis on control," and "Why prediction must be included and control emphasized."

I think you get the picture. This is a necessary reference in any treatment of "control of behavior" as a truth test, in behavior-analytic theories.

David Goldstein [920102] -- I don't really think you believe Kolbe presents a PCT model of behavior. I swear I don't. And I certainly do not think we should close the door on her. But I can't help but have doubts that she will be interested in a formal, quantitative theory and model, such as exists in PCT. Perhaps I feel that way in light of passages form her book like this one: "My own assessment is that he (Richard Nixon) was a Fact Finder resistant in Quick Start who was incapable of dealing with the bottom line" (p. 34) I am willing to predict how she would react to a paper or chapter on modeling with PCT -- and I am willing to be proved wrong.

Best wishes.

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Date: Fri Jan 03, 1992 5:51 am PST
Subject: Re: Still hasn't run out....

[from Jan Talmon 920103]

Gary Cziko (920102b)

Some info about uuencode.
Uuencode only expands a binary file by storing the bitpattern of 3 bytes into 4 bytes by taking groups of 6 bits. That's all.

When you want to reduce the amount of data to be transmitted, I would suggest an approach in which you first compress a file with an archiving program like PKZIP. Then uuencode the zipped file and transmit that file.
On the receiving site, you just uudecode the file and then unzip the result and voila....

I've used this scheme to send several types of data files, including GEM and WP.xx files.

So in summary... UUENCODE is not efficient but effective.

Jan

Date: Fri Jan 03, 1992 5:51 am PST
Subject: example text

[From: Bruce Nevin 920102 19:29]

I am looking at the sample analysis given in Stephen Johnson's dissertation. A text that his program analyzed is as follows:

> In Newport, Rhode Island, there is an old stone tower that was built many years ago. No one knows by whom it was built, but it must be very old. Some people think it may have been built by the Vikings, who may have come into the region before America was discovered by Columbus. A few years ago an investigation was begun by a group of scientists to find out who built the tower. They dug under it. They had to dig quite a while before anything was found. Finally some old

buttons and indian arrowheads were found. But the scientists never discovered who built the tower.

Take the relatively simple sentence

They dug under it.

Proceed word by word:

Recognize "they" as a reduction of "one same as mentioned" plus plural, where plural is a reduction of a conjunction. Thus, something like:

Someone dug under it and someone dug under it . . . -->
Someone and someone . . . dug under it -->
Someones dug under it

Someones--said someones are same as mentioned nearby--dug under it -->
They dug under it.

(The ellipsis here indicates that there can be any number of terms in the conjunction, so long as there are at least two. The number depends upon associated perceptions in sensory input, memory, and imagination.) Given the prior mention of "scientist," association with perceptions in imagination of the scientists and their activities around the tower leads to correlation of "scientists" with "they." However, this is in the realm of associative meanings rather than syntax, which can only go so far as the generic "someone" plus the specification of sameness.

I have included the other words "dug under it" in the reconstructions above, but of course at this point the "they" recognizer has only waked up the recognizer for indefinite noun "someone" (two or more times), the recognizer for "and" (with its provision for the reduction to plural), and the recognizers for the sameness statement. You could propose that this be an ECS (elementary control system) controlling for sameness of reference. However, the words of the sameness statement exist and the word recognizers for them can do the same work, so I bow to Occam's lifted eyebrow and follow Harris. The indefinite nouns are zero-order words, requiring no argument, but required to be in the argument of some operator word, which must be repeated under the "and" for the reduction to plural. So there is an expectancy set now for an operator that requires "someone" in its argument. By way of association of nonverbal perceptions, we want to say that the generic "someones" is resolved to a more specific "scientists," the word used earlier in the text. The indefinite nouns like "one, someone, a thing, something" are at the top or generic end of the hierarchy or taxonomy of classifier words. It might be proper to look on the less specific as reductions of the more specific.

Classifier words are often used as referentials. This sort of resolution of a nonspecific referential to a more specific referent is frequent. Thus:

So there we were in the new apartment, Clara, Skip, Cleo, Sam, and I. The dogs were fine, especially Skip and Sam--you know "Aff! Aff! Aff! See how affable I am!" Sam, sort of like Reagan. But the cats were entirely disapproving.

  The dogs were fine <--
  The dog and the dog were fine <--
  The dog was fine and the dog was fine <--
  That which is a dog was fine . . .

Here, generic "that" resolves by way of associative meanings (she assumes we remember her pets and their names as she does, and anyway she immediately refreshes our memory) to Skip and Sam. Names are at the maximally specific end of classifier taxonomies.

Returning now to the main text, two recognizers respond to "dug," as "dig" plus past-tense morpheme and as "dig" plus participle morpheme (as in "has dug"). "Dig" is an operator of the Onn class, as in "they dig holes." What appears here to be an intransitive On is reduced from zeroing of an indefinite second argument, e.g. "they dug" reduced from "they dug something." "Dig" satisfies the expectancy set up by the recognizer for "someone" or thence "scientist," noted just before we went to the dogs, and the perceptual signal from the recognizer for "scientist" in turn satisfies the input requirement of the "dig" recognizer for a word of the N class to occur suitably nearby. (I'll leave that "suitably nearby" requirement vague for now. Most importantly, it means without stronger competition from some other recognizer. There is none here.)

The past-tense morpheme is a reduction of something like "which is before my saying this" as an adverbial modifier on "dig." "Before" is an Ooo operator, here with "dig" and "say" as its two arguments. (The past tense is similarly from "after.") So "dug" wakes up the past-tense recognizer, which wakes up the recognizers for "before my saying this" and associated perceptual meanings. (Carrying it a bit further, "this" is reduced from a source similar to that given for "they" above.)

"Dug" also wakes up the past-participle recognizer. This is reduced from something like "state" or "condition" as an argument of one of a few operators, such as "have". Thus for "They have dug":

     They have dug <--
     +They have a digging-state
     +They have a state of their digging

(The + here indicates that the sentence so marked is marginal, extremely awkward because reductions to a more conventional form are virtually obligatory. I won't go into the argumentation in support of this here. See GEMP for that.) The past-participle recognizer has an input requirement for a higher operator like "have."

These two recognizers are competing with their interpretations of "dug." There is no higher operator like "have" in the sentence, nor are the conditions present for one to have been zeroed. At the end of the sentence, therefore, the past-tense interpretation wins.

But we have not yet reached the end of the sentence. The next word is "under." Two recognizers respond. One recognizer for "under" sees it as an argument indicator whereby a third argument is linked to an operator. I can't recall any operators at the moment that impose "under" as their argument indicator (I could find some in an old paper by Ryckman and Gottfried on prepositions), but perhaps an example of a different preposition in the role of argument-indicator will suffice: "to" in "I gave the book to her" is an argument indicator, it is not an Oon operator entering on "I gave the book." Since "dig" is not an operator that imposes an argument indicator on a third argument, and the conditions for zeroing such an operator are not met, part of the input requirement of this recognizer is not met, and it goes back to sleep.

The second recognizer for "under" recognizes it as an operator of the Oon class. Its input requirement includes a perceptual signal from an operator-recognizer and a perceptual signal from a noun-recognizer. Is there a recognizer for the class "operators" or does each and every operator-recognizer include in its perceptual output a signal that says "I've found a word of the operator class'? This was a question I raised in my last post addressed to Bill. The requirement for an O argument is met by the signal from the "dig" recognizer (or from an operator-class recognizer that it wakes up). A requirement for an N word remains open. "Someone" (underlying "they") is a possibility, but is already satisfying "dig" with no other candidate for that role, and anyway the N argument for "under" almost always follows it in linear order.

However, "under" here is an adverbial modifier reduced from "their digging is under something." Thus:

They dug; their digging was under it. -->
+They dug, which was under it. -->
They dug under it.

The last word of the sentence is "it," reduced from a source very similar to that of "they" without the plural: "something same as mentioned nearby." This indefinite "something" satisfies the remainder of the input requirement for "under."

I won't try to draw the dependency trees constructed, the assembling of some of them into a tree spanning the sentence, and the discarding of others. They are I hope not too hard to visualize. I'll send you a nice picture by paper mail if you wish.

For details and evidence supporting this analysis, refer to _A grammar of English on mathematical principles._ I'll try to carry this forward with some other sentences from the text if I get time and energy. However, I am bringing Sarah and the kids back from her mother's house in NY tomorrow, and she is still not free of pneumonia, so I guarantee nothing. Perhaps this is enough for a while. It goes without saying that the swiftness and precision of the bedlam excuse me pandaemonium process sketched here belies the complexity of describing it or the time taken to read the description, and without our being in the slightest aware of it.

Now to bed. 4:30 comes astonishingly quickly, and its almost 10 again.

Bruce Nevin
bn@bbn.com

Date: Fri Jan 03, 1992 7:28 am PST
Subject: Theoretical physics; the magical number 94

From Greg Williams

(Gary Cziko 920102b)

>Interesting stuff, this Newton as Skinner viewpoint. If this is so, does
>modern physics differ from this "just there" approach to science? Where is
>the "inner" organismic hierarchy" in modern physics?

You bet your booties it differs! What do you think all that quark and charm and QED stuff is about? GENERATIVE! GENERATIVE! And note that there are several levels being modeled by physicists -- discipline boundaries don't necessarily follow levels! Many physicists make a living DESCRIBING certain phenomena, just as many psychologists are experimentalists. But the modern theoretical physicists eschew the "hypotheses non fingo" stuff. And make EXTRAPOLATIVE, EXPLANATORY models. Unfortunately, the bulk (well, there really aren't all that many) of theoretical psychologists still persist in making DESCRIPTIVE, NONEXPLANATORY models solely at the level of the phenomena. "If you do basically the same procedures again, the organism will do basically the same thing." The weasel word is "basically," because these folks cannot circumscribe its bounds. So, the turn toward statistics.

By the way, I claim that the only reasonable answer to Hume's inductive skepticism (i.e., why should the sun rise tomorrow?) is making generative models which "hang together." Hypotheses non fingo leaves open the possibility that matter might disappear at any moment, since it can't predict that it WILL disappear at a PARTICULAR moment. QED says there's no "disappearing at such-and-such-a-time" relation within its (modeled) structure, so give us a break from your concocted philosophical "possibility" tales, Hume!

>But why stop at 94?

There are only 94 single-byte ASCII values available. The first 32 of the 128 total "low ASCII" are "control" codes, not passed by E-Mail, and our encoding scheme uses tilde (decimal 126) and delete (decimal 127) as the first bytes of two-byte values (representing values LEAST frequent in the original file, for the optimal encoding case -- optional, because it isn't optimal for very small files, since a code table must be appended to the encoded file, and because it is slower than table-driven encoding).

One other BURN feature which might or might not be in uu...: A (somewhat bloated, but only required once) way to encode and send UNBURN via E-Mail, allowing that file to be unencoded by anyone with a copy of the DEBUG program which comes with MS-DOS. Then they can download BURN.ASH and UNBURN it, and they're all set to BURN up the E-Mail wires to others! Anybody game out there?

Greg

Date: Fri Jan 03, 1992 9:18 am PST
Subject: Mary

[from Joel]

Bill,

Please accept my wishes that Mary's condition improves quickly and she returns home soon.

Date: Fri Jan 03, 1992 9:22 am PST
Subject: Software payment

[From Kent McClelland (920103)]

Bill Powers

Bill, I've been trying to send you this message by direct E-mail, but can't seem to get an address that works, so I'm trying through the net.

I'm teaching a seminar next semester in which I will be focusing on control theory, and I'm interested in using the demo I & II programs which you sent me last summer. I'm trying to arrange for Grinnell College to send you the $95 you ask for to cover a semester's use, but they (typically) want an invoice to process in the usual red-tape way at the Treasurer's Office, and what you sent didn't include anything that looked like an invoice. Do you have some such form handy that I could give to them?

I've been monitoring the net this fall (usually at least a week or two behind) but haven't had time to contribute much. It's been very busy, as I had to take over an extra class from a colleague who contracted TB (!). Maybe next semester when I have a class that's more relevant, I'll be able to get back into it a little more.

You might be interested to know that the latest issue of American Sociological Review (one of the two leading journals in sociology) contains an article by Peter J. Burke which cites BCP and bases the argument on control theory. The article deals with "Identity Processes and Social Stress" and seems fairly strongly indebted to Heise and to Carver and Scheier for its general approach, but he seems to have more or less the right idea about control theory, and it's a sign that control theory may be making some inroads in sociology. Is Burke somebody known to the net?

Another interesting bit of news is that the American Journal of Sociology (the other leading journal) has just expressed fairly strong interest (revise and resubmit) in my manuscript on PCT and power issues. Reviewers seemed to take the article seriously on its own terms, rather than dismissing it out of hand. This is better than I had expected, and may be yet another sign that the control theory approach is gaining some respect among sociologists.

Happy new year to you! I'm very sorry to hear that Mary is in the hospital. I hope she is better soon.

Best wishes,

Kent

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Grinnell, IA 50112-0810

Date: Fri Jan 03, 1992 9:26 am PST
Subject: Re: Control of behavior

[From Rick Marken (920103)]

Thanks to Bill Powers, Tom Bourbon and Ken Hacker for suggestions about references on control of behavior. Ken, could you recommend a particular one of Chomsky's works that has his views on behavior control. I know of his review of Skinner's "verbal behavior" and I seem to recall something in the NY Times review of books -- in the early '70s. Is there something of his that is more recent. This is exactly the kind of thing I'm looking for -- thanks for reminding me about Chomsky.

By the way, I have started my human operant control program. It's a hell of a lot easier to do than I thought. I have it set up as a tracking task with the position of the cursor influenced (in part) by discreet "rewards" produced by pressing the space bar. The cursor is also influenced by a constant disturbance (the "deprivation" effect) and (if you want) a variable disturbance (which influences the size of the reward you get from pressing). I can now simulate simple ratio schedules and plot the results in realtime as a cumulative record. The results look just like the pigeon data (I'm the subject so maybe it works because I'm a bird brain). This weekend it shall be interval schedules. Hope I get some scalloped curves.

Regards

Rick
marken@aerospace.aero.org

Date: Fri Jan 03, 1992 10:02 am PST
Subject: Re: Humpty Dumpty

[Martin Taylor 920103 12:00]
(Bruce Nevin 911231)

Mea Culpa. I should know better than to call people names like Humpty-Dumpty, knowing that it can only produce an irritated response. Also, given Bruce's quotes, I acknowledge that he is continuing a tradition rather than innovating. Having said that, I would ask Bruce to look back at my "tutorial" postings on information, substituting a word of his choice, say, "galumaphrism" where I have used "information" and then try to see what I have been saying without the connotations that he brings to his reading.

I am reminded by Bruce's posting of a quote in today's Toronto Globe and Mail, to the effect that a German officer at the end of World War II said something like "No wonder you won. You just piled up your equipment into a tower and let it fall on us." I feel like that about Bruce's mass of words. I resign.

Now. Let us consider the background to the quotes Bruce uses. Bruce thinks of galumphrism theory as a fading fad hitched by mathematical precision to the romantic concept of information as meaning. I agree that it has been so, but the fact that a chisel has been used to pry nails makes it no less useful to use chisels for wood carving. Faddism is the fate of many useful tools (especially in psychology, where few useful tools exist). I was warned about the fad problem in galumphrism theory by my professor over 30 years ago, and I hope I have kept that warning close to my heart, seldom using the theory where it is inappropriate. I do not think I am using it wrongly in this case.

A little while ago I sought the basis for the misunderstandings between Bruce and me, and I think his posting shows two. One, widely held, is that to use galumphrism theory (GT) requires stationary statistics. Bruce is right in asserting that the measures refer to ensembles, wrong in asserting that they refer to time-average statistics. The systems we are dealing with are not ergodic. That is at the heart of my comment (which puzzled Bill) that we had to deal with subjective probability, not frequentist probability Past event frequency may serve as evidence that leads to a probability estimate, but it is not the same as that probability estimate. It is also not true that in GT the galumphry in an instance is incalculable. Shannon's measure deals with the average galumphry over an ensemble of instances, and is determined by the actual galumphry -log(p) of the instance.

The second misconception is that there can be a unique measure of the channel capacity observable by an outside observer. I think I have tried to dela with this assumption/assertion in previous postings. I know it is a widely held view, but it is simply wrong. The measure depends on the difference between a prior (before the observation) probability and a posterior (after the observation) probability (better--probability distribution) for that observation, and those probabilities belong to the observer and to no-one else. Another may infer the observer's probability distributions, or may act as the observer, but there is no guarantee that the prior and posterior probability distributions agree between any two observers. Channel capacity is not a property of the channel itself, but of the channel and the recipient of the output of the channel.

It is, of course, possible to assert that a channel has certain probabilistic prperties, and by that assertion equalize the capacity as seen by different observers; but make that channel physical and real, and all bets are changed. Do the observers of the channel's output believe that the abstract description really applies to this channel? Maybe. but it is up to each one whether to do so.

Bruce mentions, quite correctly, that it is the observer plus key that gains information out of a message encrypted to look like white noise. That is an example of what I am talking about. The prior probabilities associated with the possible chunks of white noise are quite different if you have the key from the probabilities available to an observer without the key. To the observer with the key, every different chunk of the signal conveys a different message, whereas (as I noted in my postings on "good form") to the observer without the key, one chunk of white noise is much the same as another. To such an observer, indeed it is only such things as the amplitude modulation of the noise that can convey information (but what if that modulation is itself encrypted to look like white noise to a third party?).

The question of meaning versus galumphry is tricky, not least because galumphry is well specified whereas meaning is not. I do not think this posting is the place to discuss that, because it is a long and different issue to discuss. My own opinion is that meaning inheres in the hierarchic mutual control systems that connect the participants in a conversation, and that it is intimately related to galumphry. I do not "mean" by this that a litre is the same whether it is of wine or of water.

Sorry for the irritant, Bruce. There is more in your posting that still requires response, but the issues are different, and will have to wait.

Martin Taylor (with egg yolk on face)

Date: Fri Jan 03, 1992 12:04 pm PST
Subject: Re: control of behavior

From Tom Bourbon [920103 -- 13:20]
  Rick Marken [920103 -- 10:46] -- how are your scalloped curves, today? Our library opened again, today. In a few minutes I will go check on the work by Epstein, that I mentioned a day or so ago. In his description of that work, he says he used key presses to move a dot around on a screen, with the dots serving in the role of key presses or pecks, in traditional operant work. Sound as though it might be relevant to your project.
   Another good reference for gaining some insight into the concept of "control" in contemporary radical behaviorism is, Jay Moore (1981). On mentalism, methodological behaviorism, and radical behaviorism. _Behaviorism_, _9_, 55-77. Moore rather tediously develops an account of how the behavior (especially verbal behavior) of a scientist (read that "behavior analyst") comes under the control of two sets of contingencies -- one imposed by the general culture, the other, by the subject matter of the scientist's investigations. It is the latter case that I am illustrating with my "control of others" programs: the actions of the would-be controller "come to be under the control of" the actions of the one the controller would control. (Much as Skinner described in his famous "case history" paper, where he described his delight upon manufacturing a device that would free him from the tedious task of actually watching his rats so that he could drop a pellet into the cage when they pressed tha bar. Up until that time, he was acutely aware of the behavior of the rats, and of the fact that they controlled him, as much as he controlled them -- the source of the famuos cartoon in which one rat stands with a paw on the bar and exclaims to another, "Boy, have I got him trained ...," in reference to the psychologist who is poised to deliver a pellet. The invention of the automated pellet dispenser seems to have ended Skinner's chance to discover the principles of control as we conceive them in PCT.
  At any rate, Rick, you might find Moore's article helpful in your writing.
   Best wishes.

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Date: Fri Jan 03, 1992 12:21 pm PST
Subject: measuring the egg

[From: Bruce Nevin (920103 1448)]

(Martin Taylor 920103 12:00) --

Thanks for the good humored reply. I should follow some other advice from Ben Franklin, about argument.

> One [misunderstanding], widely held, is that to use
>galumphrism theory (GT) requires stationary statistics. [These
>quotations are] right in asserting that the measures refer to ensembles,
>wrong in asserting that they refer to time-average statistics. The
>systems we are dealing with are not ergodic. That is at the heart of my
>comment (which puzzled Bill) that we had to deal with subjective
>probability, not frequentist probability. Past event frequency may serve
>as evidence that leads to a probability estimate, but it is not the same
>as that probability estimate. It is also not true that in GT the
>galumphry in an instance is incalculable. Shannon's measure deals with
>the average galumphry over an ensemble of instances, and is determined
>by the actual galumphry -log(p) of the instance.

I take it (to reverse the Dretske quote) that

> Insofar as communication theory deals with quantities that are NOT statistical _averages_ . . . it MAY BE dealing with information as it is normally understood. For information as it is ordinarily understood, and as it must figure in semantic and cognitive studies, is something associated with, and _only_ with, individual events (signals, structures, conditions).

In other words, a nonstatistical basis for a measure like that of Shannon information would be a *necessary* but not sufficient condition to make such a measure--call it mmt-information--relavent to information as it is ordinarily understood. Are you claiming that Shannon, Weaver, Haber, Dretske, and many others (I didn't quote Norbert Weiner or Bertrand Russell) were simply mistaken in attributing a statistical basis for Shannon information? Or are you only objecting to the statement that

>The theory stipulates that statistically average quantities that
>this measure specifies must be from statistically stationary
>sources; that is, estimations of relative frequencies of occurrence
>of a given symbol must not depend upon the time at which the
>estimate was made.

Is there a way to apply the measure to non-stationary sources? I ask out of ignorance. In what sense is any system treated in these terms not being treated as ergodic, even an unchanging system that requires no time averaging to be made statistically stationary? Are not elements in the ensemble being averaged, and then an individual element compared to the average?

As I understand it, we PCT folk might think of subjective probability the perception of likelihoods associated with other perceptions. The evidence on which these perceptions of likelihood (or reasonableness, etc.) is based may include frequentist probabilities, but need not. As you say:

>Past event frequency may serve
>as evidence that leads to a probability estimate, but it is not the same
>as that probability estimate.

Is there anything that prevents subjective probabilities (beliefs about likelihood or reasonableness) being derived in a way that is entirely whimsical and irrational? In what way is this distinct from a set of perceptions that we might term beliefs, with different gain on the different control systems for these belief-perceptions? Or are you invoking here Bayesian calculations? What am I missing here?

>The second misconception is that there can be a unique measure of the
>channel capacity observable by an outside observer.

I never asserted this, to my knowledge. I said that Harris's linguistic information is determinable by an outside observer. This is essential to the function of language as a social tool. Associative meanings are not so determinable, though we infer many of them with a pretty high degree of reliability (depending on the sublanguage, i.e. on the more or less disciplined character of the universe of discourse).

I don't have a lot of interest in channel capacity and have referred to it only to distinguish it from information content (linguistic information) and from associative meaning (associated nonverbal perceptions).

>Bruce mentions, quite correctly, that it is the observer plus key that
>gains information out of a message encrypted to look like white noise.

That is not what I said. Assume a message in English so encrypted. It contains linguistic information (with which sender and receiver may associate additional meanings). If the message is encrypted in what appears to be white noise (but actually there must be detectable redundancy in it--near-white-noise, you said?), then the linguistic information is still there. However, it is not present in the white noise. It is obviously not present in the key (which serves for any encrypted message). It is present in the combination of white noise plus key. The redundancy in the key and the redundancy in the near-white-noise together make up the redundancy in the linguistic message. Encryption involves taking from the message that redundancy that is found in the key, so that putting the redundancy of the key back in restores the original redundancy of the message.

>The question of meaning versus galumphry is tricky, not least because
>galumphry is well specified whereas meaning is not.

The measure of channel utilization is well specified. Linguistic information is well specified. Associated meanings cannot be well specified. Linguistic information is an important aspect of meaning that is well specified. The measure of channel utilization has no demonstrated relation to meaning. If channel capacity is too slight, you lose some information in the sense of meaning, but you have no way

of knowing just what increments of meaning you will lose. The missing
channel capacity does not even bear the same relation to the corrupted
message as the key does to the encrypted message, because the missing
channel capacity has no structure (no redundancy within itself). Nor
does the provided channel capacity. It is a bare quantum.

>My own opinion is that meaning inheres in the hierarchic
>mutual control systems that connect the participants in a conversation,
>and that it is intimately related to galumphry.

Are you advancing a theory of hierarchic mutual control systems
connecting living hierarchic perceptual control systems to one another?
How are the reference signals set? Where are the comparators and input
and output functions? How many levels in this transpersonal hierarchy
of control, and what are they?

In some haste,

Bruce Nevin
bn@bbn.com

Date: Fri Jan 03, 1992 3:54 pm PST
Subject: Re: measuring the egg

[Martin Taylor 910203 17:15]
(Bruce Nevin 920103 1448)

>
>In other words, a nonstatistical basis for a measure like that of
>Shannon information would be a *necessary* but not sufficient condition
>to make such a measure--call it mmt-information--relavent to information
>as it is ordinarily understood. Are you claiming that Shannon, Weaver,
>Haber, Dretske, and many others (I didn't quote Norbert Weiner or
>Bertrand Russell) were simply mistaken in attributing a statistical
>basis for Shannon information? Or are you only objecting to the
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>>sources; that is, estimations of relative frequencies of occurrence
>>of a given symbol must not depend upon the time at which the
>>estimate was made.
>
>Is there a way to apply the measure to non-stationary sources? I ask
>out of ignorance. In what sense is any system treated in these terms
>not being treated as ergodic, even an unchanging system that requires no
>time averaging to be made statistically stationary? Are not elements
>in the ensemble being averaged, and then an individual element compared
>to the average?
>
I think the term "statistical" is another that will lead us into confusion.
My understanding of all this is being refined by the argument, and is getting
back to something like it was 25 years ago, so bear with me if my usages
seem to change a little between postings. So much of this has become intutive
and has formed the underlay for my understanding of so much of the world
that to make it once again explicit is quite a task.

My underlying notion seems to be that of Einstein, when he argued that there is no justification for taking any viewpoint other than that of the observer. The Newtonian world, with its absolute space and time, is an abstraction that can be useful much of the time, but is misleading when you push it too far. Like Einstein, I see only the viewpoint of the observer, whether the observer be a person or (Bill--note) the input of an elemental control system. Therefore, all probability must be subjective, almost by definition, and it is indeed hard to separate the notion from that of degree of belief. There is nothing "that prevents subjective probabilities (beliefs about likelihood or reasonableness) being derived in a way that is entirely whimsical and irrational" except that an entity that did so derive its probabilities would be unlikely to survive very long. The species that survive a long evolutionary process probably don't do it. Their subjective probabilities are good enough to help them get along, although they often do not coincide with frequentist estimates (we have consistent biases that can be measured in experiments, for example).

>
>As I understand it, we PCT folk might think of subjective probability the
>perception of likelihoods associated with other perceptions. The
>evidence on which these perceptions of likelihood (or reasonableness,
>etc.) is based may include frequentist probabilities, but need not.

I think it might be better to think in terms of the "imagination" loop, to distinguish the perception of likelihoods of possible "other perceptions" from some externally controllable perception, though, of course, one aspect of control is to bring to a maximum the likelihood of a desired perception.

>I don't have a lot of interest in channel capacity and have referred to
>it only to distinguish it from information content (linguistic
>information) and from associative meaning (associated nonverbal
>perceptions).
>
But I do, because it is channel capacity that affects the rates at which control can be exerted, and the stability of the control hierarchy. Transport delay has a separate influence on those things, so don't read me as being exclusive here.

>
>>Bruce mentions, quite correctly, that it is the observer plus key that
>>gains information out of a message encrypted to look like white noise.
>
>That is not what I said. Assume a message in English so encrypted. It
>contains linguistic information (with which sender and receiver may
>associate additional meanings). If the message is encrypted in what
>appears to be white noise (but actually there must be detectable
>redundancy in it--near-white-noise, you said?), then the linguistic
>information is still there. However, it is not present in the white
>noise. It is obviously not present in the key (which serves for any
>encrypted message). It is present in the combination of white noise
>plus key. The redundancy in the key and the redundancy in the
>near-white-noise together make up the redundancy in the linguistic
>message. Encryption involves taking from the message that redundancy
>that is found in the key, so that putting the redundancy of the key back
>in restores the original redundancy of the message.
>
The redundancy in this case is in that not all possible messages in some

universe (shall we say word sequence) are messages in English, and of those
that are, not all are equiprobable given the circumstances under which the
message is being received. There need be no detectable redundancy in the
white noise signal, in that, given the encryption algorithm, all conceivable
patterns of the same average energy are equiprobable (as far as the recipient
is concerned). That means that the encoding, knowing the key, is such
as to equalize the probability distributions from the universe of possible
messages. Codings that approximate this ideal are the target of much
research. But the redundancy is not in the white noise, nor in the key,
nor in the white noise plus key. It is in that after the message is decoded
into a word sequence, not all of the word sequences conceivable will occur.
Looked at as subjective probabilities, the coding is such that all noise
packets are equiprobable, but not all word sequences are. The noise packet
will be much shorter than the word sequence, because the noise packet does
not have redundancy whereas the word sequence does.

\>
\>>The question of meaning versus galumphry is tricky, not least because
\>>galumphry is well specified whereas meaning is not.
\>
\>The measure of channel utilization is well specified. Linguistic
\>information is well specified. Associated meanings cannot be well
\>specified. Linguistic information is an important aspect of meaning
\>that is well specified.

As you can see from the above, the measure of channel utilization is
well specified only from the viewpoint of the recipient. Linguistic
information, as you have described it, is the redundant part of the
galumphry of the utterance, and does not relate directly to the content
of the utterance (for "utterance" read any linguistic unit of your choice,
from phoneme to argument and above or below). I think Linguistic
galumphry is well specified at many levels of abstraction. Harrisian
Linguistic information seems to be well specified, to judge from your
analytic examples. But not all of these are in the same realm of discourse,
so they may not be connectable, despite being well specified.

\>
\>Are you advancing a theory of hierarchic mutual control systems
\>connecting living hierarchic perceptual control systems to one another?
\>How are the reference signals set? Where are the comparators and input
\>and output functions? How many levels in this transpersonal hierarchy
\>of control, and what are they?
\>
Yes, that's what I am advancing, but so far it doesn't look quite like
Powers' hierarchy, because I see no way to restrict it to scalar elemental
control systems. The references are set, as in Powers' PCT, from higher
levels in the communication hierarchy, but they are not scalar variables.
They are intentions that the transmitter of a message perceive the
recipient as having reacted appropriately to the message (e.g. to have
"understood" it, or to have performed a desired action ...). This is
true at every level, as in Powers' PCT.

A second difference with my current understanding of PCT (although recently
I begin to feel it is my misunderstanding) is that there is no fixed
set of connections, and that elements within a level can control
each other. My current understanding of PCT is that elemental control
systems at one level provide references for ECSs at a lower level,
not at the same level. (But I'm wrong on this, aren't I?).

So, I can't assert anything about levels within the transpersonal
hierarchy (at least not yet). Any particular virtual message may go
through many conversion stages, but these stages may in many cases
correspond to the same level (say "sequence") of the PCT hierarchy.
It may well be the case that top-level messages may be sent between
the same PCT levels in the two parties, but I don't want to assert
this to be so.

Martin

Date: Fri Jan 03, 1992 5:07 pm PST
Subject: Mary

Bill, Please give my best wishes to Mary.

Hugh

Date: Fri Jan 03, 1992 5:56 pm PST
Subject: GET WELL MARY

[From Wayne Hershberger]

I would like to wish you all a belated Happy New Year, Mary
Powers, in particular!

Bill, Joyce and I are distressed to hear that Mary is in the
hospital again. We hope, for the sake of us all, that she is
able to come home soon. Give her our love.

Chuck Tucker, did you get the post I sent you directly
on Christmas Eve?

David Goldstein (920102)
>Subject: Kolbe
>I also think that it is a good idea to [be] friendly
>towards people who express some interest in learning
>how PCT might apply to what they are doing.

Amen. I am in general agreement with every aspect of
your well measured assessment of Kathy Kolbe's
position, but the sentence quoted above is the most
important, in my view.

Chris Malcolm:
Your description of the evolution of your ambidexterity
(912423) was choice, and seemingly very instructive!
Have you ever compared notes with other lefties trained
to write with the right (wrong) hand?

Bruce Nevin (911231)
>One other touch of personal news, I wrote what I

>thought were satisfactory answers to the PhD
>preliminary exam in historical and comparative
>linguistics the 24 hours of the 18th and 19th, though
>I won't have official results until toward the end of
>January.

If you thought your answers were satisfactory, I can not imagine the committee thinking otherwise. Let me be the first to congratulate you. Also, I must say I was charmed by the following mellifluous metaphor, in your stellar post of (911231):

>Dretske's bold promises rest on the cracked foundation
>of old premises.

Bill Powers (Christmas, 92)
>Our perceptions that we call "feet" are certainly not
>too big to fit into a brain: they are precisely small
>enough to pass through a neural fiber.

Neural signals in the brain may be said to be relatively small, but the replicable perceptions (phenomenal objects) those signals help mediate are not necessarily small. Smallness is an aspect of phenomena and it is a mistake to suppose that the size of a phenomenal object is in any way related to the size of the neural signals which help mediate it. You have yourself been championing this sort of argument in many of your recent posts.

>All aspects of our perceptions are proprietary,
>including our convictions that some are not.

No. A proprietary aspect is immanent in all experience, or so it seems. But this does not imply that there are no other aspects.

Bill Powers (920102)
>This is basically what I am arguing with Wayne Hershberger
>about. We are trying to reverse-engineer evolution (or whomever
>you want to blame). In doing so we come up with a model of
>underlying design features constituting a system that interacts
>with its environment just as real organisms do.

Yes. As I see it, we are trying to reverse engineer the phenomenal domain, and the "spec" that I think is of the first importance in this venture (also, as I think Kant was saying) is that phenomena are bipolar: in a word, psychophysical. Control theory appears to be uniquely compatible with this psychophysical specification, providing one continually recognizes both ends of the dipole--a control system AND its environment. Perhaps we should change our language habits and speak of control SUBsystem, since the control system is only one part (or pole) of the system being captured by our reverse engineering.

Warm regards, Wayne

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Date: Fri Jan 03, 1992 6:56 pm PST
Subject: recent misc.

[From: Bruce Nevin (920104 2104)]

Bill, I hope all is well with Mary. I don't need to tell you that a blood clot in the wrong place can be a very serious matter indeed. Though we have never met, please add my mite of hope and care to what must be flooding your way from your many friends.

(Tom Bourbon [910102 -- 0:45]) --

Fascinating model of control of others.

Extrapolating grandly: seems like what is wanted with Kolbe is to introduce disturbance of a sort that makes it impossible to ignore error arising in her model of psychodynamics while concurrently making the strength of PCT equally obvious as a safety net so that she doesn't feel compelled to rationalize the error preserving the theory. She has to own the responsibility for the error signals so the disturbance itself must be subtle enough not to seem an attributable cause. Perhaps the exercise is worth it even with a person whose conversion you don't value particularly highly. If this could be accomplished for relatively benign Kolbe it might be a skill transferrable to others who are less amenable. But I'm just restating the obvious.

Bill had said:

>The other process adjusts utterances according to whatever linguistic
>conventions the speaker knows and cares about. The linguistic forms are
>perceived, and if they are in error, the control systems adjust the
>developing sentence where possible or force a restart. You see why I'm

Avery Andrews (Fri, 3 Jan 1992 04:56:30 EST) --

>The problem as I see it is that there
>is not that [much] error-editing (Bill Labov, the numero uno observational
>linguist, tells us that 75% of speech is grammatical as is), & once a
>mistake is made there is not that much that can be done to fix it.

Errors that require correction bring the process of languaging to conscious awareness, and that is the main reason for our surprise that as much as 75% is without error (ref prior posts, examples from Martin Taylor, etc.). At the other end of the awareness spectrum, the control

systems adjusting most incipient error act so swiftly that at most a
hesitation is the only audible evidence.

>My recollection from when my kids were younger is that they could say
>impressively complicated things `I want to push Owen while being carried',
>but that if anything went wrong, they had to restart the whole communication
>from scratch, including securing the channel.
<nice example>
>This doesn't look like an interesting control system to me. I see the
>problem as one of putting out complicated novel performances mostly without
>errors, but also (to make it easier) without having to oppose any
>disturbances.

That's a nice observation. (You must be an observational linguist.)
But it just means that control of conscious error repair where
unconscious error repair processes have broken down is a late
development. This fits with familiar observations about consciousness
of control interfering with control. Later, we learn to ride the
conversational bicycle better.

>My belief about syntax is that mostly there *aren't* errors to correct,
>and when there are, they mostly aren't corrected, but one starts again
>from scratch.

I deny the second clause for adult speech. You cannot have examined
many transcripts of real-time conversation. Maybe you're not an
observational linguist after all. :-)

(The fact that a distinction between an "observational linguist" and I
guess a theoretical linguist could make sense in the field will
seem astonishing to historians of science, I predict.)

(Ken Hacker [010292]) --

Noam Chomsky is very fortunate that no one has had the imagination to
develop his ideas of biologically innate capacities along racist lines,
in the manner of Agassiz at the turn of the century.

Greg Williams (Thu, 2 Jan 1992 22:24:00 GMT) --

>   (3) Are uu... optimal in the sense of encoding the
>most frequent 94 byte values in the original into single byte ASCII values?

A few years ago someone was trying to market a scheme that mapped items
in an English word list to single-byte ASCII values, in hardware. I
don't think it went anywhere, or at least I have heard no more of it.
Such a function for text files might make your burn more desirable than
uu??code. It would not be so useful for non-text files.

Bill Powers (920102.1600) --

>In biology, reductionism appears as analyzing some complex function into
>its constituent sub-functions. Pointing a finger at something reduces to

Reductionism usually refers to the relationship between science B and
science A that is claimed to be antecedent to it, in the sense of defining
its foundations. In a pejorative sense, it is a claim that the findings
of science B reduce to "nothing but" arrangements of the findings of

science A. I understand there is an active unity of science movement quite apart from the now defunct ambitions of the logical positivists, which depends upon reductionism in a positive sense. The insight that the whole is greater than the sum of its parts stands against reductionism (e.g. the Holism first articulated by Smuts, and organismic biology especially Woodger's fascinating attempt to formulate foundations for biology by adapting the symbolic logic of _Principia_).

>its constituent sub-functions. Pointing a finger at something reduces to
>adjustments of tensions in specific muscles. Therefore pointing is
>"nothing but" tensing muscles in certain patterns of contraction.

If "pointing" is an observation in psychology then this appears as reductionism in the usual sense.

To the extent that Control Theory erases boundaries between sciences concerned with living things, it must appear reductionist to those sciences. Practitioners of any one science are apt to see it as encroachment of an adjacent science, rather than proposal of a new and more encompassing science. Is the latter a different sort of holism?

Your discussion of physics suggests that the more "basic" science requires contextualization, must be made sense of in a perspective that transcends the strict boundaries and terms of the science itself. From an understanding of the perceptual control hierarchy we can see how this must be so.

Avery Andrews (Fri, 3 Jan 1992 13:48:01 EST) --

One of the problems of various flavors of Generative grammar is that they all presume use of phrase structure grammar, rewrite rules like:

        A --> BCD /X___Y

(Abstract symbol A becomes string BCD in the context after X and before Y.) The problem is that, in the labelled bracketings or tree structures that these rules generate, the elements that are relatable to one another (presumably by elementary control systems in the perceptual control hierarchy) are abstract symbols that carry no information about the classes of words and morphemes at the bottom (leaves) of the tree, much less about the particular words and morphemes to be "inserted" at some point into these structures. This means that nonverbal perceptions (real-time, remembered, and imagined) can have no role in syntax until after lexical insertion. One consequence is that preterminal symbols *must* carry indices (graphically, subscripts) to indicate sameness of reference. The whole metalanguage apparatus must be imagined to have evolved somehow unrelated to relations among nonverbal perceptions, and the mapping from strings of abstract preterminal symbols onto words and morphemes must have evolved without recourse to relations among the words and morphemes themselves. No wonder a biologically innate apparatus for language is essential to the theory!

Dependency grammar is so much more plausible and manageable--yet despite cogent arguments to that effect by Jane Roberts and others it has seldom been seriously considered. So far as I can see it is the same as trying to get a WordStar user to consider a different word processing package, or a QWERTYUIOP typist to consider learning to use a DVORAK keyboard. Even string grammar (Joshi's center-and-adjunct grammar) and Joshi's

TAGS (mixing adjunction rules with minimal rewrite rules) are better
than PSG. Some of the PSG derivatives (head-driven, etc.) get around
some of the problems, but not the worst of them such as those sketched
above. Operator grammar is obviously a dependency grammar plus the
reductions.

When I get a chance to make a copy of Stephen's dissertation, I'll mail
it to

Avery Andrews
Linguistics, The Faculties
ANU, PO Box 4
Canberra ACT 2601
Australia

It is not on line, though you could ask Stephen
johnson@cucis.cis.columbia.edu
if he has it on line. He wrote a paper "Mathematical building blocks"
in _AI Expert_ for May 1987, which you should be able to turn up. He's
currently in charge of medical informatics at Columbia Presbyterian.
He's gearing up for a resumption of research, together with Lynette
Hirschman, who is co-author of a number of things with Naomi Sager.

Bruce Nevin
bn@bbn.com

Date: Fri Jan 03, 1992 7:16 pm PST
Subject: Metaphysical Slumber

Greg Williams 920103

>By the way, I claim that the only reasonable answer to Hume's inductive
>skepticism (i.e., why should the sun rise tomorrow?) is making generative
>models which "hang together." Hypotheses non fingo leaves open the possibility
>that matter might disappear at any moment, since it can't predict that it
>WILL disappear at a PARTICULAR moment. QED says there's no "disappearing at
>such-and-such-a-time" relation within its (modeled) structure, so give us a
>break from your concocted philosophical "possibility" tales, Hume!

But isn't this argument circular? The theory must always go beyond the
data and so your theory, no matter how well "supported" by the data up to
now, can still be wrong. So I think Hume's major logical point is still
valid, although I don't accept his psychological interpretation.

--Gary

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Date: Fri Jan 03, 1992 8:46 pm PST
Subject: Cultural Anthropology

Bill Powers (and anybody else interested in cultural anthropology):

Do you know of any cultural anthropologists who have used PCT in their work? It would seem that PCT could give valuable insights into culture and cultural differences and suggest methodologies for understanding and contrasting different cultures. I seem to remember someone called Bohannon that wrote something with you at some time. Perhaps he has done something with PCT.

An anthropologist colleague of mine, Jacquetta (Jacquie) Hill is shooting videotapes of preschools in Thailand and Japan and wants to present edited versions of these to people both within and across cultures. I suggested that it might be useful to see the videotapes, especially of the foreign cultures, as consisting of disturbances and to elicit reponses from the viewees of what is "wrong" with a particular situation, what the teacher "should" have done, etc., to get a grip on controlled cultural variables. Any ideas about using PCT for this type of research would be most appreciated.--Gary
----

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Date: Sat Jan 04, 1992 9:51 am PST
Subject: Metaphysical slumber vs. physical awakening

From Greg Williams

(Gary Cziko 010392)

>[Greg says:]
>>By the way, I claim that the only reasonable answer to Hume's inductive
>>skepticism (i.e., why should the sun rise tomorrow?) is making generative
>>models which "hang together." Hypotheses non fingo leaves open the possibility
>>that matter might disappear at any moment, since it can't predict that it
>>WILL disappear at a PARTICULAR moment. QED says there's no "disappearing at
>>such-and-such-a-time" relation within its (modeled) structure, so give us a
>>break from your concocted philosophical "possibility" tales, Hume!

>But isn't this argument circular? The theory must always go beyond the
>data and so your theory, no matter how well "supported" by the data up to
>now, can still be wrong. So I think Hume's major logical point is still
>valid, although I don't accept his psychological interpretation.

Certainly, ANYTHING is POSSIBLE, ANYTIME (i.e., "with God"). My point is that Hume's skepticism is KNOW-NOTHING skepticism, which must be contrasted with "uniformity" (through time) claims made on the basis of models which have worked pretty well so far. It's comparable to a stranger coming up to you and telling you that you could suddenly renounce life and become an ascetic tommorrow at noon. Wouldn't you put a LOT more confidence in your own theory ("knowing" yourself) that your life tomorrow will go on pretty much as in the past (hanging out on CSGNet, and all that!) than in the stranger's theory, even though the latter is logically possible?

I called "the argument from modeling" a "reasonable" answer to Hume, not a refutation of his skepticism. And reasonability is what science is supposed to be about, within logical bounds set by philosophy. To predict requires the possibility of being wrong about it -- otherwise it reduces to dogma or tautology.

Nobody else on the Net seems to have much to say about the Delprato correspondence, which contains what I thought are some highly controversial claims. Am I preaching to the converted (except maybe you), or is this tangential stuff, or am I ready to publish in PHILOSOPHY OF SCIENCE? In reviewing notions of scientific explanation (Salmon's recent synopsis of 40 years thereof is of the greatest importance), I found nobody saying that explanation of phenomena depends on description at the next lower level -- so that, if anything, is what is original. Perhaps the p of s's missed it because they haven't tended to be observers, which is where I came in ("What do my sons count as explanations?").

The (slight) irony of control theorists glomming on to Maturana's notion of generativity is that it appears (at least Bill Powers and I think) that Maturana's claimed-to-be-generative notion of "autopoiesis" ISN'T generative.

Greg

Date: Sat Jan 04, 1992 10:43 am PST
Subject: Philosophy of Science

[from Gary Cziko 920104.1220]

From Greg Williams (920104)

Keep it up. You're educating me. Your emphasis on reasonableness seems quite reasonable to me. The real skeptics will never be convinced, so why bother?

I have been interested in the philosophy of science for some time, although most of what I know about is arguments for and against Popper's philosophy and Campbell's evolutionary epistemology, towards which I'm basically favorable (my evolutionary bias showing through).

>Nobody else on the Net seems to have much to say about the Delprato
>correspondence, which contains what I thought are some highly controversial
>claims. Am I preaching to the converted (except maybe you), or is this
>tangential stuff, or am I ready to publish in PHILOSOPHY OF SCIENCE? In
>reviewing notions of scientific explanation (Salmon's recent synopsis of 40
>years thereof is of the greatest importance), I found nobody saying that
>explanation of phenomena depends on description at the next lower level -- so
>that, if anything, is what is original. Perhaps the p of s's missed it because
>they haven't tended to be observers, which is where I came in.

I have not seen this argument before. I've seen falsificationist views (e.g., Popper), new prediction views (e.g., Lakatos), deductive views (e.g., Salmon) and Bayesian views (e.g., Howson and Urbach) but not your n-l perspective.

I will forward some of your stuff to Patrick Maher, a real philosopher of science who is on my campus and who studied with Salmon. Perhaps we can get him to join the discussion.

By the way, could you give me a reference to the Salmon synthesis on explanation which you mentioned?--Gary

----
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Command: print 1-7

Date: Sat Jan 04, 1992 11:45 am PST
Subject: language etc.

[From Bill Powers (920104.0900)]

Thanks from me and Mary for all the concerned messages.

The mail is getting out of hand. If I fail to respond to a specific inquiry, remind me.

Greg Williams (920102) --

Very nice commentary on reductionism.

I think there's actually a "why" concealed even in a "how" explanation. When you reduce a phenomenon to the next lower level of processes, you see the "how" in the arrangement of finer details. But it's easy to overlook the fact that not all arrangements will have the same effect. There's always the question of WHY that particular arrangement exists instead of an alternative. Why not an inverse-cube law? The hidden why reveals the structural laws that lead from level n-1 to level n.

Avery Andrews (920102) --

>>One process adjusts utterances according to the meanings they evoke in
>>the speaker as they are uttered (or in imagination prior to utterance);
>>this leads to editing on the fly and other means of adjusting the
>>meaning of an utterance to make it match the meaning intended to be
>>communicated.

>But this adjustment basically consists of putting in something where
>there was nothing, which is maybe significantly different from the

>more usual cases of feedback control.

Not really different. The initial error is identical to the reference signal (there is no perceptual signal). If you can explain how a partially-completed meaning is brought a little closer to a reference-meaning, you have explained everything.

Re: adjusting sentences according to linguistic rules:

>I don't know about this one. The problem as I see it is that there
>is not that most error-editing (Bill Labov, the numero uno observational
>linguist, tells us that 75% of speech is grammatical as is), & once a
>mistake is made there is not that much that can be done to fix it.

There's a difference between "mistake" and "error signal." All that an error signal indicates is how much of the goal is not yet met. A sentence under construction is accompanied by a decreasing error signal (actually multiple descreasing error signals), the amount and direction of the error always indicating what remains to be done. The error signal doesn't indicate that there's anything wrong with the part of the sentence so far completed. You only have to start over when the remaining error turns out to be uncorrectable. So the fact that 75% of sentences are grammatical upon utterance means only that the progressive error-reduction during formation of a sentence usually continues to zero error without any hitches. Zero error means perceiving a grammatically correct sentence that conveys the right meaning.

However, I love your child's "crashes."

I think of the process of sentence construction as one of _assembly._ One has in mind a meaning that is to be converted into a sentence -- perhaps just an event that is to be described. Just prior to starting the assembly process, one has in mind memories of the interesting parts of the event, sitting there in imagination or perception like a checklist. There are objects involved, usually, so one set of processes picks up the object names (or action names or relationship names -- the nouns and noun phrases). Another aspect of the meaning consists of relationships, which requires setting up part-forms that convey relationships, such as prepositional phrases. At the same time, multiple meaning-perceivers are at work, seeing what the component parts of the assembly as it stands at any instant evoke and comparing the evoked meanings with the intended ones at all the levels. This probably results in discarding some candidate components and picking up new ones in their places. At a higher level, the component pieces of the sentence are being judged as to syntax -- we need "the" here and "a" there -- and modifications are going on at that level, too. All these selection and error-correction processes gradually (in a few tenths of a second) converge to a completed sentence. As soon as the first parts of the sentence are completed, utterance can begin, with the remaining parts of the assembly process keeping ahead of the utterance (if not, the utterance pauses). None of this necessarily involves any "mistakes." It's just a set of processes going on at several levels of organization, some in parallel and some in sequence, with continual error-correction guiding the assembly of the sentence.

Even in the assembly of a prepositional phrase, both meaning and syntax are being controlled for at the same time. One set of processes concerned with meaning picks up "baby," "bouncing" and "chair" to go with those imagined perceptions. "On" is selected to indicate the kind of relationship between bouncing and chair, and a sequence-control system sees to it that the phrase comes out "bouncing on chair", while another control system pops a "the" in between "on" and "chair" and also before "baby." The words and relationships are simultaneously being perceived and the images they evoke are being compared with the reference images of the event held in memory. While this assembly is proceeding, one of the meaning-control systems might see that "baby" evokes the wrong image and change the word to "child." And of course the sentence-parser detects the lack of a verb and sticks "is" in where it belongs, if it belongs (if something is to be said about the child bouncing in the chair -- it fell off -- the "is" is omitted: The child bouncing in the chair fell off).

This is strictly amateur night, but this scenario feels much like the way I assemble sentences as I type them. How does it strike you?

Bruce Nevin (920101) --

>Ontogenesis: how can recognizers for words according to their
>argument requirement in fact come into existence? These appear,
>for many languages, to be N (primitive nouns--dog), On (operators
>with one N argument--sleep), Onn (eat), Onnn (give), Oo (operators
>with one operator, of any class, in its argument--be true), Ono
>(think), Oon (surprise), Ooo (cause).

In part, see above. Think of all the argument-requirement perceivers as operative at the same time. Whatever words are selected at lower levels for lower-level reasons (such as naming things, actions, and relationships), all the requirement-perceivers are checking the results to see if they fit. Do you think it would be possible to define "O" and "N" in terms of the level of perception to which they refer? The basic problem here, it seems to me, is how the system can decide that a particular word is an "O" or an "n". That might be easier to work out if the system classified the _meaning_ rather than the word.

I said

>>You seem to be proposing that a single cooccurrance would suffice
>>to establish the memory. . . . If I say "the and" one time, is
>>that sufficient for you to establish this pair as a conventional
>>cooccurrance, or does it have to happen more than once?

Your answer didn't really give an answer -- or it gave an answer you perhaps didn't intend:

>Well, first off the words "the and" cooccur not only with each
>other but also with one or more intonation contours, as well as
>with other words.

My somewhat oversubtle point was that in the sentence above, you could see the cooccurrance "the and" because I had just typed it. My question was whether that cooccurrance, that you just saw on the screen, established "the and" as a cooccurrance once and for all. Your answer and its elaborations actually appealed to more than one experience with the phrase -- in fact, to general rules that can only be derived from multiple experiences over many occasions. Thus you were illustrating that we come to recognize cooccurrances as significant only if they occur enough times in recognizeable contexts. So when you say

>The structure is immanent in the language. The child does
>not invent it. The child does not need a statistical analysis to
>construct it.

... this seems to contradict the fact that we do NOT accept as real every cooccurrance, and that we DO learn them over time by making judgements about which are meaningful and which are not.

I think that you're hanging on to an objectification of language that you don't really believe in. While it's not odd that a linguist should do this, it is odd that _you_ should do this.

>I am supposing that for a time at least one remembers all the
>perceptions of a situation, nonverbal and linguistic, and subsequently
>only an idealization or normalization or regularization of them.

But remembering all the perceptions is not the same as remembering cooccurrances. If you remember 6 perceptions, there are 720 cooccurances of single perceptions. Are you saying that each cooccurrance is remembered, as well as the 6 perceptions? I would say that a cooccurrance does not exist for a given person until it is explicitly perceived as a cooccurrance (I would prefer to say "sequence," partly because the order makes a difference, but mainly because I have such an awful time typing that word).

>One possibility, though messy: by associative memory and imagination,
>signals come back to its input function from collateral ECSs in the
>hierarchy that either strengthen signals already coming in to its input
>function or supply signals not actually derived from the environment.
>Neater would be for associative memory and imagination to be a function
>of higher-level ECSs.

If Joe Lubin is still paying attention, I wish he would comment on this. It sounds a lot like the structure he's developing as a model of hierarchical perception.

I think your conjectures are heading in the right direction. Maybe it would be profitable at this point to look for some simplifications. There are many levels of processes implicit in your ideas. Could we try to model some part of this, just something simple like naming? Or whatever chunk you can see that could be pulled out of the whole mess and modeled as a standalone process. Maybe this flood of ideas is making perfect sense to you, but it's confusing me.

>I want a sequence recognizer that takes input from the recognizer for
>the word "sleep" and expects there to be any word of the N class in a
>position appropriate for a first argument.

This would do as a starting point. But let's back down just a scoche. The word "sleep" implies a number of meanings, one of them being sleeping. How does this word get recognized as belonging to the N class? Does the N-class recognizer have to have a list of all possible words that belong to it, or is there some shortcut? Is there some way it could know that sleep belongs to it just by knowing what kind of perception it means? Since all word-recognizers emit the same kind of signal upon recognition, there can't be anything in the signal that reveals the necessary fact. Could it be that _any signal from a given level of recognizer_ can be treated as an N? Any set of levels?

Maybe the problem will become a little simpler if we say that the sequence recognizer isn't concerned with the source or nature of the N-class word. We can imagine classfiers that detect N-ness and o-ness by unspecified means, and a set of sequence-recognizers that simply look for "On", "Onn", and so on (without having to know what the underlying words are). The syntactic requirements are met if one of the sequence-recognizers emits a signal. If there's a syntatical error, it can be corrected by altering reference signals to lower-order systems (somehow, doing something or other about the error). In building a sentence, the sequence-recognizers simply check for existence of a valid syntax without caring what the valid example itself is. They are single-purpose devices; no one of them is a speech-generating device. Only the whole ensemble of systems, each concerned with one dimension of an utterance, creates an utterance (or recognizes it).

>I want a sequence recognizer that takes input from the recognizer for
>the word "dog" and expects any word of any of the operator classes On,
>Onn, Onnn, Ono, Oon. Maybe this is just a requirement for an ACK signal
>from one or more operator-recognizers saying "I assume you are in my
>argument." Such a signal would be in the *input* requirement for the
>word-recognizer.

I think this collapses too much into a single level. "The word dog" implies a lot more than the sequence of marks that goes D, O, G. In order to talk about operator classes, we have to look at what they mean -- what is the "operation?". Can we focus on just one of them, and take it apart a little more? What about On?

>It appears that there is just one sequence recognizer for each class of
>operators--one for Oo operators, one for Onnn operators, and so on.

This is what I'm counting on. If we can say how the meanings of these operators differ, we will be getting closer to relating this process to HCT. I am convinced that meaning gets into this. So I'm glad when you say

>In this respect I do agree with Martin and others who give primacy to
>semantic considerations in the interpretation of language.

Bruce Nevin (920102) --

A very convincing and thorough discussion of information theory, for which thanks.

Re: "Yes"

>The _linguistic_meaning_ of the word "yes" is always bound up in its
>role in the second half of a question-answer pair. Linguistically, when
>you hear "yes," you must remember or imagine a yes-no question, and with
>yes you must be able to include the affirmative member of the yes-no
>disjunction given in the question.

Consider the message "yes" in response to the following utterances:

Como se dice en Ingles, <<si>>?

What is a three-letter affirmative?

Did you say "yes" or "no"?

Say "yes."

If your reply is "yes" I will assume you disagree.

In none of these cases is the message "yes" in response to the given statement "bound up in the second half of a question-answer pair" in the senses that you propose. Sometimes a word is just a word. Knowing only the message, one has no way to tell objectively what it means.

>What is relatively constant is the linguistic information immanent in
>the utterance, which functions for us somewhat as a skeiner's bob on
>which we try to loop and sort the strands of perceptual life.

I hope to persuade you some day that there is nothing "immanent" in any perception (unless you slip in first and convince me of the opposite). Wayne wants physics to be part of the immanent order. You want language to carry imnmanent information. I suppose that Martin wants redundacy and probability to be immanent. I believe that organisms are really, truly, control systems. I suppose that all of us would prefer to think that anything on which we depend heavily for survival (physical or intellectual) has some objective truth in it. But if we all have our way, we will soon be back to naive realism, which doesn't make any sense to any of us. Could it be that everything is a matter of perceptual construction EXCEPT language? EXCEPT physics? Etc?

Bite the bullet.

No time to comment on the rest -- I'm sure it will come up again.

Greg Williams (920101 etc) --

It would be nice to BURN a long file into a single transmission, which my version of uuencode won't do. What's the suggested retail shareware price?

Bruce Nevin (920103) --

>   Someone dug under it and someone dug under it . . . -->
>   Someone and someone . . . dug under it -->
>   Someones dug under it

I have never detected anything remotely like this going on in my head when I hear sentences. What I got was images:

Bunch of people standing around near a (stone) tower. Something scientist-like about them.

Bunch of people with shovels (quickly revised to bunch of people giving orders to peons with shovels).

Shovels stuck in ground, dripping dirt, piling dirt. Happening at base of tower (revised from basement of tower ("under")). Problem getting the exact digging site -- you can't lift the tower to dig under it. Finally settled on a tunnel (tower leaning precariously).

By the time I got to "they dug under it" the little-group-of-scientists icon was already there, and the tower icon. "They" popped up the scientist icon, "it" popped up the tower icon, and "dug" put the shovels and dirt into the picture. The fact that it was "dug" and not "dig" made the whole thing like a memory, something already over with.

Of course you could claim that your version of the processing was really going on unconsciously and that I was just being entertained by these images long after the process had done its work. Seems to me that this would be difficult to prove (or falsify), even to yourself. The fact that there is some ingenious expansion-contraction procedure that will end up with the actual sentence form doesn't show that this procedure actually occurs. And I don't see any evidence in its favor. Of course I don't know much about it -- my subjective report probably looks pretty naive.

>The past-tense morpheme is a reduction of something like "which is
>before my saying this"

This is what I meant in saying it was "something like a memory." But that sense wasn't in words. It seems to me that you're relying on a lot of non-linguistic perceptions in this analysis, which are then approximated by words. I doubt very much that we ever say to ourselves "the dog and the dog ... were fine." But it's possible that when you try to describe what actually goes on, it might come out like that. I think that the "reductions" probably involve nonlinguistic perceptual steps.

I do think, however, that you're approaching the point of defining the neural operations that must be taking place.

Martin Taylor and Bruce Nevin (920102 etc) --

Re: information.

Information in its nonstatistical sense gets us into epistemology, doesn't it? That is, to say that message A contains information about B implies that you have a way of knowing what B really is, so you can say in what respect the message informs us about B. It's also implied that you can tell when the information about B is wrong. In any case, the presumption is that there is something objective to which the message can refer, but even more that we can know what that something is in some way other than relying on what the message has to say. One could just as easily transfer this argument to the question of perception. A perceptual signal is a message. To what does it refer, and how can the system containing the perception know that?

Martin says

> Like Einstein, I see only the viewpoint of the observer, whether the
>observer be a person or (Bill--note) the input of an elemental control
>system.

What's the difference between an input to a person and the input to an ECS? I thought the inputs to elemental control systems WERE the inputs to the person.

Wayne Hershberger (920103) --

>Neural signals in the brain may be said to be relatively small, but the
>replicable perceptions (phenomenal objects) those signals help mediate
>are not necessarily small. Smallness is an aspect of phenomena and it
>is a mistake to suppose that the size of a phenomenal object is in any
>way related to the size of the neural signals which help mediate it.

But you assume, in order to say this, that phenomenal objects and attributes of objects are something other than neural signals. I assume they are the same thing. How do we get past that?

>As I see it, we are trying to reverse engineer the
>phenomenal domain, and the "spec" that I think is of the first
>importance in this venture (also, as I think Kant was saying) is
>that phenomena are bipolar: in a word, psychophysical.

Why do you assume the "-physical" part of psychophysical? There is nothing in the physical domain that is not derived from perception and thoughts about perceptions. It seems to me that you slip your conclusion into your premises. I do not see the "psychological" aspect of experience as being on an equal footing with the "physical" part. The physical part is a set of ideas, and so is a subset of the psychological part.

I find the topology of your point of view baffling. It seems to involve some magical way of knowing things without perceiving them, and some way of checking on the meanings of perceptions other than comparing them with other perceptions. I can't grasp the role that you give to perceptual signals, or for that matter, to the brain. I can't understand what position you're assigning to the Observer -- if the observer isn't in the brain, where is it? And where, then, are the objects of observation?

Kent Mcclelland, I'll work up some sort of invoice for the Demos and post it. Maybe it can be incorporated into the files that Bill Silvert is making available. To everyone else: the only charge is for professional use (i.e., teaching). Copy and distribute freely for your own use and use of friends.

Keeping up with the mail, even sketchily, is getting to be a problem. I guess all I can do is hit the high spots. Maybe this would become more manageable if we narrowed the focus to a few basic problems we might explore, toward building models of handleable chunks.

Best to all
and thanks from Mary,

Bill P.

Date: Sat Jan 04, 1992 11:53 am PST
Subject: Lies, damned lies....

Thinking about magic (that is, conjuring) got me headed in this direction, which I suspect is to start an argument. Oh well....

A standard line in CT is "no control of another person, except by overwhelming physical force." Ultimately, we are supposed to be free to follow our reference signals. OUR reference signals. But shouldn't that read "free to follow our ERROR signals"? Errors (the DIFFERENCES between reference signals and perceptual signals) drive actions. And people sometimes can control others' perceptual signals, and hence, errors, and hence, actions. The simplest example is deceit following the establishment of trust. Say that you consider me a friend, and generally believe what I say. I decide to control your actions as follows. I want you to get up and go out of the room, even though you just said a minute ago, "I just want to sit here and relax." So I go out of the room and then yell "HELP! I cut my finger!" This is not true, of course. But if you treat my cutting my finger as a true perception, you will "come to my aid." (You might never, again, though!) At the very least, I think I have, with you, JOINTLY controlled your getting up and going out of the room. You wouldn't have done it without my deceit. MY hierarchy is, in effect, using you as an instrument to achieve my ulterior goals.

This might be generalized to non-deceitful cases, too. As a teacher, I don't have direct access to your reference signals, but I can (at least jointly) control your PERCEPTIONS, and hence, your ERRORS, and hence, your actions.

May the barrage begin.

Greg

Date: Sat Jan 04, 1992 1:45 pm PST
Subject: More lies?

From Greg Williams:

Yes, the "Lies..." post was from me (no lie!). Sorry I forgot my name at the top.

Gary, the reference you wanted is: "Four Decades of Scientific Explanation:, by Wesley C. Salmon, in MINNESOTA STUDIES IN THE PHILOSOPHY OF SCIENCE, volume 13, 1989. (Also reprinted recently as a separate monograph with the same title.)

Greg (P.S. Gary -- I need the December "B" Log, also. Got the rest! THANKS!!

Date: Sat Jan 04, 1992 5:52 pm PST
Subject: Re: language etc.

[Martin Taylor 920204 20:30]
(Bill Powers 920104.0900)
>
>Martin says
>
>> Like Einstein, I see only the viewpoint of the observer, whether the

>>observer be a person or (Bill--note) the input of an elemental control
>>system.
>
>What's the difference between an input to a person and the input to an
>ECS? I thought the inputs to elemental control systems WERE the inputs to
>the person.

You asked earlier whether the "subjective probability" implied that the global organism that is the person was doing the probability estimate, or whether it was each individual ECS. I didn't answer that at the time, so I brought it to your attention in my answer to Bruce. And it doesn't imply, as you earlier suggested, that the low-levels ECSs have properties that you attribute to (the action of?) higher level ECSs, only that they behave *as if* they do.

The input to a person *may* be the combined effect of the inputs to all the myriads of ECSs, or it may be those that are consciously used to form a perception in (of?) the world, or it may be something else. But the input to a ECS is (for now) a single scalar variable. That's different.

You weren't interpreting "elemental control system" as "lowest-level control system," in your question, were you?

Martin

Date: Sat Jan 04, 1992 5:59 pm PST
Subject: Re: Lies, damned lies....

Ken Hacker [920104]

Greg, I second the intention of your comments about who controls what with reference signals. From the works of Vygotsky and Mead through modern communication studies, we know from mounds of studies that what an individual does with his or free agency is largely constrained by parameter, definitions, rules, percepts, and goal states which have been socially derived.
Indeed, one of the objections that people in my discipline (communication studies) have with cybernetics is the assumption that individuals are closed systems.

We are free to choose or design our own actions to a certain extent, that extent having profound social and cultural origins. As French political theorist Jacques Ellul argues, we are most dominated by others when we think we are free but really are free only within the fences created by others.

I personally see control systems theory as having enormous potential, both as a perspective guiding theories of adapation and regulation, but also as a source of knowledge regarding ways that humans can create more of their own loci of control and free centers of control from external sources. I also see a need for more attention to social cybernetics which may extend our realizations that human behavior is rarely, if ever, a socially isolated set of phenomena.

Ken Hacker, Dept. of Communication Studies, New Mexico State University

Date: Sat Jan 04, 1992 6:11 pm PST
Subject: Re: Humpty Dumpty

[Martin Taylor 920104 20:50]
(Bruce Nevin 920102)

>
>It is essential to realize that the linguistic information in a sentence
>is not and cannot be identical with the perceptions it may evoke in
>memory and in imagination. We may be pleased to think of these
>associated nonverbal perceptions as the meaning of the sentence. Such
>meaning differs between any one language user and any other perceiving
>that sentence. The linguistic information in that sentence, however, is
>constant, a communal property of the speech community. The meanings in
>terms of idiosyncratically associated nonverbal perceptions are an
>interpretation of that linguistic information and, being grounded in
>that communal property, will have much in common from one language user
>to another--but also unpredictably much disparate. Communication has to
>do with evocation of perceptions in memory and imagination. The
>linguistic information in language is one social tool for doing this,
>which is efficient for some aspects of meaning but not for others.
>
There is either confusion here, or we have another difference of opinion. In
my view, you have said two things about "linguistic information", one of which
I agree with, and the other of which seems to be contradictory. I agree with
the first sentence, and the last two, which seem mutually consistent and
consistent with what I think has been implied by all Bruce's postings on
linguistic information.

But " The meanings in
>terms of idiosyncratically associated nonverbal perceptions are an
>interpretation of that linguistic information"
seems to be inconsistent. The linguistic information, as I understand
Bruce to mean it, is in the Operator-argument structure and the reductions,
in other words it is the carrier structure, that contains nothing of the
content. The meanings inhere in the words chosen to fill that structure,
the relations among them which are certified by that structure, and their
relations to the pragmatic situation of the talker and listener. I do not
see how meaning can in any way be "an interpretation of that li{guistic
information" if the first sentence and the last two sentences are also to
be believed (as I do).
>
>A little test is in order perhaps, of the hypothesis that semantics and
>pragmatics are primary and syntax is only used to disambiguate difficult
>cases.

A little misunderstanding here. The hypothesis (at least mine, in the BLC
model in the Reading book) is not that syntax is used only to disambiguate
difficult cases, but that it is used to check interpretations made in all
cases by the faster but imprecise and potentially ambiguous associative
process. If the syntax disagrees with the easy interpretation AND provides
an alternative that fits, then it will override the quick interpretation.

The basic principle, which is evident in experiments in many different
situations, is that humans (and maybe others) have two quite independent

processes for comparing one situation with another: a similarity process and a distinction process. One says "is this like something I know" and the other says "is this different from something I know." These processes, when experimentally separated, do work at different speeds, the similarity process being faster (it's also informationally easier). The distinctiveness process can and does override the results of the similarity process, but usually in non-contrived situations the only overriding it has to do is to eliminate erroneous similarities that arise in ambiguous situations--most patterns are similar to many other in various ways. In reading, for example, there seem to be three or four stages of similarity process that occur before any distinctiveness process cuts in (after about 200 msec). (Note that newer research may have refined or altered these results. I haven't looked at this field for about 6 or 7 years with any seriousness).

The existence of two different processes of comparison ought to have some consequences for PCT, I think. As I understand it, PCT is much more aligned with the distinctiveness process: "Is this percept different from what I want it to be" rather than the similarity process "is this percept something like what I want it to be?" I'm not going to try to follow this up, unless someone has some ideas, but it is worth keeping in mind.

Martin

Date: Sat Jan 04, 1992 8:16 pm PST
Subject: language generation

Re Powers(04 Jan 92 13:40:38 CST)

>This is strictly amateur night, but this scenario feels much like the way
>I assemble sentences as I type them. How does it strike you?

Pretty good. In computational linguistics jargon, it's `simultaneous constraint satisfaction', about which there is a fair amount of literature. The direction in which my speculations run is that the grammar (be it phrase-structure based, dependency, or something completely different) provides a sort of partial schedule for organizing the satisfaction of the goals. But I don't have time to work out a detailed story right now. Needless to say, there is also a literature on sentence generation, which I ought to know more about before going much further with this.

One issue, I guess, is whether explicit control theory can do better with these problems than, say, current logic programming techniques (PROLOG and more various more sophisticated experimental languages), which have a sort of goal-directed flavor to them. But I really don't have the right kind of math background to take that issue on. I suspect that a more accessible target is to try to work out what the goals motivating speech production actually are - `describe an incident' seems like a common one, `say something about a protagonist' maybe another. There is a lot of literature on discourse analysis which might be relevant to this.

Avery Andrews

Date: Sat Jan 04, 1992 10:45 pm PST
Subject: Misc comments

[From Bill Powers (920104.2200)]

Greg Williams (920104) --

>Nobody else on the Net seems to have much to say about the Delprato
>correspondence, which contains what I thought are some highly
>controversial claims.

They sounded fine to me in my provincial innocence. Are you saying that someone thinks it works some other way?

>A standard line in CT is "no control of another person, except by
>overwhelming physical force." Ultimately, we are supposed to be free to
>follow our reference signals. OUR reference signals. But shouldn't that
>read "free to follow our ERROR signals"? Errors (the DIFFERENCES between
>reference signals and perceptual signals) drive actions.

I'll stick with "follow our own reference signals." We don't have exclusive control over our error signals because of independent disturbances. The reference signal defines the intended *outcome* of action, not the action. Our freedom of choice extends only to outcomes; the actions needed to achieve any outcome depend on the state of the environment. So we do not control our actions. Selecting an intended outcome constrains us to produce those actions that will in fact bring the perceived outcome nearer to the intended outcome. Someone else, by fiddling with the environment can, if that person has guessed our intent correctly, predetermine our behavior (our outputs). If, of course, our producing the output that the other wants doesn't entail causing errors of other kinds in ourselves.

>And people sometimes can control others' perceptual signals, and hence,
>errors, and hence, actions. The simplest example is deceit following the
>establishment of trust.

See pp. 259 ff in BCP, in which I discuss control of other people through disturbances, through deceitful promises, through misinformation, through rewards, and of course through physical coercion.

These are short-term methods of controlling others; what is wrong with them is that they fail as long-term policies.

In order to use cleverly-manipulated disturbances for controlling the behavior of others, one has to be careful not arouse direct opposition, create conflict, or induce intrinsic error and call forth reorganization. This is almost impossible to do -- it is certainly impossible for anyone now to do, given our limited ability to understand another person's complete hierarchy of goals. Perhaps some day we will be able to map another person's hierarchy so well that we will be able to control that person's behavior down to the last detail, completely avoiding causing any errors that will either produce resistance or result in reorganization. In that far-off day, we will see the ultimate impracticality of controlling others in a peaceful way: to do so we would have to become the other person's slave, anticipating every desire and obligingly protecting the other from all disturbances that would elicit unwanted (by us) actions. I presume that this is not exactly what putative controllers of other people have in mind to do.

As to coercive control or control through deceit, both fail because of the ultimate impossibility of preventing the other person from reorganizing. Even the giver of rewards will ultimately be unmasked as the monopolistic owner and withholder of rewards. When the discomfort of being clumsily manipulated to suit someone else's goals finally arouses the reorganizing system, the victims will change themselves and eventually outwit the controllers. As the maxim went in the old Wild West days: no matter how good you are, there's always a faster gun somewhere.

Short-term control of other people's actions is possible. Long-term control is not.

Martin Taylor (920204) --

You say

>The input to a person *may* be the combined effect of the inputs to all
>the myriads of ECSs, or it may be those that are consciously used to
>form a perception in (of?) the world, or it may be something else. But
>the input to a ECS is (for now) a single scalar variable. That's
>different.

I wonder if there's an accidental misstatement here or the key to a real misunderstanding. The INPUT to an ECS is not a scalar variable; the OUTPUT of the perceptual function is. The input is generally a multidimensional set of variables X1 ... Xn which are the arguments of the perceptual function. The output of the function, the higher-order perception, is the value of this function, which of course means only one value at a time as for any proper function. The output, the perceptual signal, can have only a single value, so it is a scalar. The set of inputs can constitute a vector, a tensor, a set -- whatever you like, in as many dimensions as you like.

Ken Hacker (920104) --

>From the works of Vygotsky and Mead through modern communication
>studies, we know from mounds of studies that what an individual does
>with his or free agency is largely constrained by parameter,
>definitions, rules, percepts, and goal states which have been socially
>derived.

Thinking in terms of a hierarchically-organized person instead of just one monolithic thing gives a lot of these observations new meanings. Socially-derived goal-states, for example, can be seen as chosen by each individual in order to achieve a private purpose. Societies can propose and constrain, but the individual still must decide whether to resist or adopt any rule, custom, principle, and so forth. The basis for the decision is not external: it ultimately comes from the requirements for well-being in each person. I am free to yell FIRE in a crowded theater, to borrow an old saw. The only reason I would not do so is that I am aware of the consequences, and find them more onerous than restraining myself. It's still my decision, not society's. In the same way, society provides roads for me to drive on, and laws saying that I should drive only on them, but I am perfectly free to drive anywhere I decide to drive, all things considered. It's just a matter of turning the wheel and stepping on the gas; there's no way society can prevent me from doing that. If I don't do it, that's because I have decided not to, for my own reasons.

Of course, my own reasons may stem from the fact that I approve, generally, of social order and want to do what I can to support it. I may well give up the freedom to do whatever I like at lower levels of perception, as society recommends and often demands, in order to enhance my ability to control for higher-level goals. This isn't altruism: it's doing my bit toward shaping the world nearer to the heart's desire.

Most people have learned how to create the impression of going along with social demands of the more stupid sorts, satisfying what they imagine other people to want of them, while actually doing and thinking pretty much as they please. This gets pretty funny sometimes.

>I personally see control systems theory as having enormous potential,
>both as a perspective guiding theories of adapation and regulation, but
>also as a source of knowledge regarding ways that humans can create more
>of their own loci of control and free centers of control from external
>sources.

I think of this freeing-up in a slightly different way. To understand oneself as a control hierarchy (in detail, not just in general) is to realize that one has always, in fact, been free. Every apparent instance of external control contains in it a personal decision to go along with the perceived demand. Some reflection and interpretation is necessary to realize this, but ultimately it becomes clear that "social control" can never go further than presenting one with a situation. What one actually does in that situation arises from inner intentions, not external forces. All the mechanisms for making choices, seeking goals, reorganizing, and acting are inside each person, not outside them. Nobody moves my arms and legs, directs my eyes, takes my breaths, or thinks my thoughts but me. The only iron constraint on what I do, am, and become is my own human nature, which comes to me not from society but from my extremely long history as a living system.

Of course to the extent that one doesn't realize these things, it can easily seem that society is firmly in charge. When you think you're being controlled, try going up a level.

Mary and I will probably take a jaunt through New Mexico in a month or two, Ken. Where are you? We could do lunch.

Best to all

Bill P.

Date: Sat Jan 04, 1992 10:45 pm PST
Subject: Re: Phil. of science;Lies

From Tom Bourbon [920105 -- 0:08]

Greg Williams [920104a], it is not that control theorists (at least not this one) accept the idea that Maturana's ideas about autopoiesis provide a generative model of behavior. To the contrary, he intends his work to be generative, but it is descriptive, and pretty selectively so, at that. However, when he writes of the differences between descriptive and generative models, he does so in a manner that I find effective. His characterization of the difference is apt; his application is apt to fail.

Greg Williams [920104b], so what is the argument? It is patently true that one person can control the actions of another, if .... That was the topic of some of my recent posts on control of behavior -- was I _that_ opaque?! (This reply applies also to Ken Hacker [920104].) To control the _actions_ of another, all one must do is disturb a variable that is controlled by the other. The other (the controllee) has no choice but to act in a way dictated by your disturbing influence, IF the controllee values control of the original variable more than he or she values not complying with that requirement. A direct implication of the fundamental PCT model is that, while a person might freely choose an end result he or she intends to experience, he or she is not free to select the specific actions required to achieve that end. That concept was part of the first publications on PCT and is one of the points made in nearly all of the publications on modeling with PCT.

It was also a central concept in the psychology of William James and was at least implicit in Aristotle's psychology. People have known of the principle, if not the formal theory, for ages. They use it every time they practice to deceive, as in your example, or to coerce, as when they threaten the loved ones of the person they would control. And they often use it when they try to teach, to tutor, to coach -- nearly any time they try to change or "shape" the behavior of another. Nearly all behavioristic literature the results of projects in which the controller disturbs a variable controlled by the controllee -- but behaviorists do not recognize or acknowledge that fact.

Ken Hacker, the criticism of "cybernetic" models as closed systems, immune to outside influences, is irrelevant, for perceptual control theory: we try to describe and predict the continuous interactions of organisms and their surroundings. There is no way in which a perceptual control system is a closed system, in that sense. However, it is equally true that no "social system," as such, controls tha individual. Social systems are groups of individuals, each of whom uses her or his own muscles to act on the environment -- an environment that might include another individual that some members of the group desire to control. Each of the would-be controllers will act so as to experience perceptions of the controllee doing what he or she intends to perceive the controllee doing.

The members bent on controlling the controllee might have come to their state of desire through conversations, through group readings or chants, or any other means of communication we can imagine, but each individual will have come to that decision and each will act to achieve his or her intended experience.

Greg, how is THAT? Fire away!

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Date: Sat Jan 04, 1992 11:04 pm PST
Subject: Language

[From Bill Powers (920104.2345)]

Forgot a comment on

Avery Andrews (920104) --

"Simultaneous constraint satisfaction" sounds quite appropriate.

>Needless to say, there is also a literature on sentence generation,
>which I ought to know more about before going much further with this.

I hope you decide to go further with it -- this sounds like a natural bridge to control theory.

>One issue, I guess, is whether explicit control theory can do better
>with these problems than, say, current logic programming techniques
>(PROLOG and more various more sophisticated experimental languages),
>which have a sort of goal-directed flavor to them.

I know very little about PROLOG, but what I have heard suggests that it is basically organized in a way compatible with control theory. As I understand it, you can, in effect, set a conclusion as a goal and let the program find the premises that will lead to that conclusion. That's the right structure for a high-level control system (even though it is also a formal way of begging questions). If anyone wants to become proficient in PROLOG, this might be a way to learn more about control systems that work at the program level. Rick Marken, haven't you done some work with PROLOG? I wish Bill Williams were on this net -- he has learned that language and used it. Anyone else out there?

>I suspect that a more accessible target is to try to work out what the
>goals motivating speech production actually are - `describe an incident'
>seems like a common one, `say something about a protagonist' maybe
>another. There is a lot of literature on discourse analysis which might
>be relevant to this.

What you're saying strikes a chord of rightness in me. This sounds like a workable project that would go somewhere. Does this look as if it might lead to a model involving both linguistic control and meaning control, as I described them two posts ago? I presume I'm reinventing all sorts of wheels here, but the CT orientation is bound to add at least some new spokes.

Best

Bill P.

Date: Sat Jan 04, 1992 11:47 pm PST
Subject: Re: Misc comments

[Martin Taylor 920105 02:30]

(Bill Powers 910204 2200)
>
>
>I wonder if there's an accidental misstatement here or the key to a real
>misunderstanding. The INPUT to an ECS is not a scalar variable; the
>OUTPUT of the perceptual function is.

A minor misunderstanding, perhaps. I have always thought of the ECS as consisting of two scalar inputs (Perception and reference), a difference operator, and an output transform. I have thought of what you call the perceptual function as being another part of the network, though I recognize that it is legitimately brought within any individual ECS. If you think of it as belonging within the ECS, I have no problem (except possibly a memory problem) in talking of it that way in future. I suppose the same applies to a reference function, which similarly combines all the reference signals influencing the ECS?

In both perception and reference, there is a many-to-many connection among ECSs, and I guess it would be a good thing if you, Bill, would post a "standard" diagram of what is considered to be within any one ECS. The distributional parameters and combinatorial functions are as much a part of what affects the overall performance of the total system as are the gains of the individual ECSs, and it would be nice to talk about the same thing when we discuss them.

Martin

Date: Sun Jan 05, 1992 5:14 am PST
Subject: Levels, Description and Explanation

[from Gary Cziko 920104.2045]

Greg Williams (920101)

Greg, I'm still mulling over your correspondence with Dennis Delprato which you shared with us on the net on the first of the year. I find I need some more help to understand all the implications of your view on description and scientific explanation. To set the context:

>   In sum, then, my notion is that, contrary to the view
>   generally held by scientists, genuine (extrapolative, rather
>   than summarizing) prediction and control of phenomena at
>   level n can be achieved only by theories couched in terms of
>   level n-1.

Now the puzzle:

>   This implies that empiricist theories in
>   psychology can be used to (genuinely) predict and control
>   (and explain, as I use the term) sociological phenomena, NOT
>   psychological phenomena. Empiricist theories in physiology
>   are required to (genuinely) predict and control psychological
>   phenomena.

My problem is understanding how description in psychology can be used to explain sociology. For argument's sake, let's say that people are as Skinner conceived them to be and that we have data showing that they will produce certain behaviors to get certain rewards (money, food, sex, etc.).

Now you say this can't be used to explain individual behavior but CAN be used via sociology to explain some aspects of group behavior.

But if the empiricist psychology of Skinner is just a summary of observations and gives you no basis for generalization (and you must always have generalizations to predict since conditions are never exactly the same), then how can this be any better for sociological prediction? I can't see how moving up a level to n+1 solves the problems that are there at level n. You will now want to be able to predict that given a bunch of people under certain conditions they will interact in a certain way as a group. But how can this be done if your psychology in inadequate to begin with? Why don't the inadequacies at any level n entail inadequacies at all levels greater than n? You can see the pit I am falling into here. Can you stop my fall, or at least provide a soft landing spot?--Gary

P.S. Maybe some examples would help me to understand these ideas better.

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Date: Sun Jan 05, 1992 7:38 am PST
Subject: BURN, generativity, explanation

From Greg Williams

[Bill Powers (920104.0900)]

>It would be nice to BURN a long file into a single transmission, which my
>version of uuencode won't do. What's the suggested retail shareware
>price?

BURN will handle long files. What's the going beta tester's wage rate?

[Bill Powers (920104.2200)]

>>[Greg Williams (920104)]
>>Nobody else on the Net seems to have much to say about the Delprato
>>correspondence, which contains what I thought are some highly
>>controversial claims.

>They sounded fine to me in my provincial innocence. Are you saying that
>someone thinks it works some other way?

Yes, Dennis Delprato and Wayne Hershberger, to name two someones (hint, hint).

[Tom Bourbon 920105]

>Greg Williams [920104a], it is not that control theorists (at least
>not this one) accept the idea that Maturana's ideas about autopoiesis
>provide a generative model of behavior. To the contrary, he intends
>his work to be generative, but it is descriptive, and pretty
>selectively so, at that. However, when he writes of the differences

>between descriptive and generative models, he does so in a manner
>that I find effective. His characterization of the difference is apt;
>his application is apt to fail.

Amen. I should have said that Bill Powers, Tom Bourbon, and myself (and Bill Williams thrown in for good measure) don't think Maturana's "model" is really generative, even though we think his notion of generativity is important. Still, that IS ironic.

[Gary Cziko 920104.2045]

>My problem is understanding how description in psychology can be used to
>explain sociology. For argument's sake, let's say that people are as
>Skinner conceived them to be and that we have data showing that they will
>produce certain behaviors to get certain rewards (money, food, sex, etc.).
>Now you say this can't be used to explain individual behavior but CAN be
>used via sociology to explain some aspects of group behavior.

I say that descriptions at the behavioral level don't explain behavior, and descriptions at the sociological level don't explain sociological observations. A description in the Skinnerian vein would be that people show certain behaviors which are correlated with certain outcomes which can be lumped into a class termed "rewards" (of course, this begs the question of why some outcomes end up in the class and others don't, which can only be answered by invoking structural constraints embodied in organismic physiology). However, such description at the individual behavioral level is, I claim, what counts as an explanation of sociological phenomena. It appears to me that people generally accept accounts such as the following as explanation: How come the voting turnout rates of the poor are much lower than those of the rich? Continuing in the Skinnerian vein, for argument's sake, it is because some individuals receive few rewards from voting (and reduce their voting rates), while other individuals receive many rewards from voting (and keep voting). That is pure description at the individual behavioral level. But it isn't an explanation of the sociological phenomena, just yet.

What must be added to the description at the individual behavioral level, as given above, is (aha!) description at the sociological level, to wit: most individuals belonging to the class "poor" are in the first (few rewards from voting) group described above, and most individuals belonging to the class "rich" are in the second (many rewards from voting) group. Now we can DEDUCE that the poor will come to vote less frequently than the rich. We have a generative model at the individual behavioral level which, coupled with a description of certain conditions observed at the sociological level (BUT NOT THE PHENOMENA TO BE EXPLAINED AT THAT LEVEL), result in an explanation of the sociological phenomena in question. This is an EXTENSION of what I am coming to call the "Car Talk" theory of explanation (since it derived in part from considering what callers to this "problem-solving" show accepted from the "expert mechanic" hosts as explanations of their automotive maladies), and I thank you, Gary, for pushing me along to make it.

>But if the empiricist psychology of Skinner is just a summary of
>observations and gives you no basis for generalization (and you must always
>have generalizations to predict since conditions are never exactly the
>same), then how can this be any better for sociological prediction? I
>can't see how moving up a level to n+1 solves the problems that are there
>at level n. You will now want to be able to predict that given a bunch of
>people under certain conditions they will interact in a certain way as a
>group. But how can this be done if your psychology in inadequate to begin

>with? Why don't the inadequacies at any level n entail inadequacies at all
>levels greater than n? You can see the pit I am falling into here. Can
>you stop my fall, or at least provide a soft landing spot?

Moving up DOESN'T solve the problems. In the example above, pure faith (precisely as criticized by Hume!) is the ONLY basis for believing that TOMORROW the poor will continue to vote less frequently than the rich, since there is no basis except a beief that "what was, will be" for extending the "functional relationship" (Skinner's term) between past correlations between voting and reward and future frequencies of voting. Without limits on the generalizability of such relationships, which I claim can only be placed by generative models at the level below individual behavior, you're in free fall. One might call it the free fall of statistics -- comfortable, until you meet a boundary. Then, SPLAT!

I'll wait a bit to see if there are more comments on my "Lies" post before replying.

Greg

Date: Sun Jan 05, 1992 2:56 pm PST
Subject: diagrams

[From Bill Powers (920105.1400)]

Martin Taylor (920104) --

>I have always thought of the ECS as consisting of two scalar inputs
>(Perception and reference), a difference operator, and an output
>transform. I have thought of what you call the perceptual function as
>being another part of the network, though I recognize that it is
>legitimately brought within any individual ECS.

We do need to get together on the basic ECS diagram. If you have access to back issues of BYTE, look in the issues for June, July, August, and September of 1979 for my series called "The nature of robots." In the August part there is a diagram on p. 103 that shows "a typical control system in the middle of a hierarchy of control systems." This is set up for building a computer model with an indefinite number of levels and systems at each level, including connection matrices. On p. 107 this unit of organization is shown in a two-level control system having three systems at each level, independently controlling x-force, y-force, and muscle tone in a three-muscle system. On p. 108 the same diagram is shown topologically transformed into an arrangement more like the actual anatomy of the spinal cord and brainstem.

That settles it. GENERAL PROPOSAL:

We really could use a way to transmit diagrams. The problem of course, is the vast array of different graphics systems out there, from PCs to MACs to workstations to mainframes. We need a very simple program that can be compiled on different machines and linked to drawing functions peculiar to the individual machines and printers. Once this was done we could send sketches back and forth and save billions of words.

As Version Zero, I envision a simple graphics command language in ASCII that describes lines, ellipses, and rectangles, with a one-font lettering capacity as well. At the creation end, an elementary keyboard-operated drawing program would place the lines and other figures using cursor keys, under visual feedback control. The elements of a drawing would be described as ASCII strings containing command codes and arguments. On the receiving end, the ASCII strings describing the elements and their sizes and locations would direct the drawing process. PCs could use the Print Screen (graphics function) to print the diagrams to a dot-matrix printer. I don't know if MACs have a similar facility, but someone could write one.

If this were written in C, the drawing and graphics decoding programs could be left up to the user -- all we need is one programmer per type of machine used on the net. By keeping the basic program and graphics language very simple, we could transmit useful diagrams quite tersely. After we get Version Zero running, maybe we could think about a Version One with somewhat greater capabilities -- but the main thing is to transmit diagrams with labels even if the means is very elementary.

I think we could have a workable system in a few weeks after we settle on a spec. So this is a call for volunteer programmers and for contributions to the spec. To kick off the project, I offer the following list of commands as a starting point for corrections, additions, modifications, and so on.

```
LINE x1,y1,x2,y2;          Line from x1,y1 to x2,y2
CONTINUE x,y;              Line continued from current position to x,y
ELLIPSE x,y,a,b,theta;     Ellipse centered at x,y, major axis a,
                           minor axis b, orientation angle theta.
RECTANGLE x1,y1,x2,y2;     Rectangle
TEXT x,y,"Text string";    8 x 8 dot text, baseline starting at x,y
$BEGIN;                    Begin program
$END;                      End program
$PRINT;                    Print diagram
$CLEAR ;                   Clear screen
```

(Note that each command string ends in a semicolon. Leading spaces, carriage returns, line feeds, and nonalphanumeric characters are ignored.)

The left-hand column contains the ASCII strings that would be transmitted. A rectangle with corners at (10,10), (100,10), (100,30), (10,30) would be drawn by the command, in ASCII characters,

```
     RECTANGLE 10,10,100,30;
```

By judiciously selecting command names, we can shorten them to single letters: L, C, E, R, T, $B, $E, $P, and $C.

I think I could draw usable diagrams with just the above commands.

The creation program should also be simple. I suggest, beside the basic ability to draw according to the above commands,

1. rubberbanding lines, ellipses, and rectangles
2. copying rectangles and ellipses to different places on screen
3. showing the current drawing position with a cross or x.
4. typing in text labels using alphanumerical keys.

The decoding program needs only to look for a starting $B;, then interpret each string ending in a semicolon and doing the operation until the $E; string is encountered. This would allow the decoding program to find and execute diagram commands embedded in text.

One great advantage of this simplicity of encoding is that a patient person could create a diagram just by typing in the commands -- the creation program just makes the process easier. So the only really basic requirement is that you have a decoding program that can turn the commands into graphic elements. This should be an hour's work for a proficient programmer.

We need some information about the various display formats that are out there, so we can pick a reasonable least common demoninator. Please fill out and post your answers:
----
1. computer system:

2. max graphics screen dimensions (x,y number of dots):

3. dot matrix printer type:

4. programming language:

5. programmer available? :

I'm assuming that out of 120 listeners we can come up with enough programmers to write the decoding routines for all the machine types that are involved. I volunteer Pat Williams (Greg's wife) to write a nice creation program for PCs (if she doesn't want to do it, I'll give it a hack). I volunteer Rick Marken to write one for MACS. Volunteers needed for other machine types.

Best

Bill P.

Date: Sun Jan 05, 1992 4:08 pm PST
Subject: logic programming

Since there don't appear to be any real prolog gurus on the net, I'll pretend to be one (where there are no fish, even a crab is a fish, as the Russians like to say):

Thinking about logic programming & control theory, my current suspicion is that it's another `close but no cigar'. Basically, in LP, the definition of a goal is used to attempt to construct an object that satisfies it. For example, one might define the factorial function as follows:

```
   factorial(0,1).     % first clause, base for recursion.

   factorial(N0,N) :-  % second clause, does the recursion.
     N0>0,
```

```
     N1 is N0 - 1,     % in Turbo/PDC Prolog you'ld use `=' for `is'
     factorial(N1,N2),
     N is N2 * N0.     % for efficiency, the recursive call ought to
                       %   appear at the end of the clause (tail recursion
                       %   optimization, but I can't figure out how to do it.
```

The function is defined as a relation, such that factorial(X,Y) is true if X is the factorial of Y. If we make the `query' `factorial(0,1)', the prolog system will respond `true', if we query `factorial(1,0)' it will respond `false', while if we query, say `factorial(2,X)', it will respond `true, for X = 2'. The way it works is that the inputs to the factorial `relation call' are matched against the clauses in the definition. So the first query suceeds because it matches the first clause. But the second fails. It first fails to match against the first clause, so we match against the second, which specifies a list of subgoals which have to be satisfied for the whole clause to succeed. The first subgoal succeeds, as does the second, incidentally manufacturing a value for the variable N1 (the value being 0). We now in effect query the goal `factorial(0,N2)', which matches against the first goal  if we set N2 = 1. How for the final goal we discover that 0 (the value of N) is not equal to 1*1, so the whole goal fails.  The `factorial(2,N) goes through in a similar way, except we get value for N when we're done.

There's no magic here (though it sometimes seems like there is): if we query `factorial(N,1)' we will get failure or an error, because the `is' statement can work only as an arithmetic relation checker, if all its inputs are fully specified (`instantiated'), or as a function calculator, if the variable to the left is uninstantiated; but in any case all the variables to the right have to be instantiated.

Backing off a bit, the basic idea is to work throught the various clauses in the definitions of the goals, attempting to either (a) find out if the items you have been given satisfy the definition (b) see if un- or partially specified inputs can be further specified (`instantiated') so that they do.

The difference between this and interesting cases of control, I speculate, is that in control, the definition of the goal does *not* in and of itself provide the means for achieving it: the reference levels constituting the desire for the sensation of beer do not contain within them anything that implies getting out of chairs, opening fridge-doors, etc. Whence reorganization, etc. Perhaps this a sort of degenerate case of control, but the flavor is different.

Many important linguistic goals, such as `produce a well-formed sentence', have the logic programming flavor to them, at least as we presently understand grammar, since just about everyone (or at least Chomsky & Harris followers) seems to think of it as some sort of assembly of prefabricated bits, subject only to the requirement that the bits fit together, & have their demands for neighbors satisfied. Intersecting this `degenerate goal' with the more interesting one of actually saying something is perhaps where CT will come in in a serious way.

  Avery Andrews

Command: print 14

Date: Sun Jan 05, 1992 6:08 pm PST

Subject: VERSION 0

(Bill Powers (920105.1400)

>GENERAL PROPOSAL:

>We really could use a way to transmit diagrams. The problem of course, is
>the vast array of different graphics systems out there, from PCs to MACs
>to workstations to mainframes. We need a very simple program that can be
>compiled on different machines and linked to drawing functions peculiar
>to the individual machines and printers. Once this was done we could send
>sketches back and forth and save billions of words.

>After we get Version Zero running, maybe we could think about a Version
>One with somewhat greater capabilities -- but the main thing is to
>transmit diagrams with labels even if the means is very elementary.

Version 0 already exists. It's our "PictureThis" Program, of course -- a full-featured but (very!) affordable WYSIWYG PostScript drawing/typesetting program for IBM-PC compatibles.

Advantages: It already exists, thoroughly debugged. I used it for CSG books.
- It has an extremely rich range of drawing functions -- no clunky drawings only slightly better than one can do with a word processor. Everything Bill wants and MUCH more.
- Produces drawing files (binary, VERY small) which can be viewed on-screen on a MINIMAL IBM-PC (one floppy, no mouse, 512KB) and Encapsulated PostScript files (pure ASCII, and pretty small, because of one- and two-letter codes for PostScript commands (i.e., "showpage" is "s")).
- Shareware version is FREE and we wouldn't expect Netters to buy the Registered Version ($65), since it isn't needed for this application (its extra fonts unneeded -- Shareware Version comes with Times family, Symbol, and even Zapf Dingbats).
- Pat doesn't have the inclination to work on another graphics program anytime soon.
- I assume PRACTICALLY EVERYONE has access to a cheap IBM-PC and a PostScript printer (as a last resort, in every medium- or bigger-size city, there is a typesetting service bureau which will print out 300-dot-per-inch drawings from IBM-format Encapsulated PostScript files for around $1.00 per page).
- PostScript emulators (IBM) are available for around $100 for dot-matrix printers and H-P LaserJet compatibles, or you can do a print-screen to a dot-matrix or H-P compatible from an IBM with CGA or Hercules (or, next month, EGA or VGA) graphics.
- Folks are welcome to code display PostScript for their own non-IBM machines which would put up what is in PostScript files created by PictureThis (probably not with all the bells and whistles -- but to do Bill's suggested rudiments wouldn't be much, if any, harder than working from his suggested ASCII lines). PictureThis PostScript files are quite straightforward. And they could produce rudimentary PostScript files on their non-IBM machines. The PostScript language is completely documented in inexpensive books.
- PostScript is an ever-farther-reaching standard, far beyond this Net.
- (FOR US:) If more people become familiar with using PictureThis, maybe they'll tell their friends how wonderful it is for page

layout, and we'll sell more Registered Versions.
Maybe somebody has others to list?

Disadvantages: You (not Pat) need something to keep you busy. So do some other folks on the Net.
Not EVERYBODY has access to an IBM-PC compatible or PostScript-compatible printer, and some of those folks who don't might like to write programs using Bill's specs better than using PostScript.
Assuming that some folks wrote decoders for their printers/computers to handle PictureThis PostScript files, we'd have to all agree on and stick to those drawing primitives decodable by all. (Shouldn't be too much of a problem, since PictureThis actually does a lot by using only a small subset of the total PostScript language.)
If you want MODERATELY simple drawings, Bill's scheme is simpler. (But you CAN do a lot with "ASCII-graphics," as has been showing up on the Net from time to time.)
PictureThis drawing files, being binary, must be converted to ASCII (BURNed???); PostScript files (ASCII) made with PictureThis cannot be displayed in PictureThis.
Bill's specs could become a standard, far beyond this Net.
I'm sure you all have others to list. Bill, your own programming expertise and free time (?) compared with those of most others on the Net might bias you toward hacking, while others might opt to spend a bit of money instead (say, for a $500 IBM-PC or a PostScript emulator program).

So, in the absence of Version 0.1 and its patch programs, Version 0 is here if anybody is interested. We normally charge $15.00 for the Shareware Version, with complete docs on-disk, a tutorial, etc. -- for Netters, it's only $5.00 postpaid ($10.00 outside North America, U.S. funds, no credit cards) from: HortIdeas Publishing, 460 Black Lick Rd., Gravel Switch, KY 40328. Specify 3.5" or 5.25" disks.

Greg & Pat Williams

Date: Sun Jan 05, 1992 7:06 pm PST
Subject: acquisition of syntax

on Nevin, Thu, 2 Jan 1992 08:55:03 EST (grammar learning)

There's a lot there to contemplate, but two things come to mind at tbe moment:

>A perceptual signal comes into a nascent foo-recognizer. It sends
>out a signal as though (anthropomorphically speaking) it were
>transmitting the news "I perceived foo" (or just "foo"). Other ...

This and the subsequent material about the nascent foobar recognizer seems to me more like the genesis of a PS grammar than of the kind of dependency grammar you prefer.

The approach sketched also seems to be very much in the `autonomous syntax' tradition, in the sense of syntactic knowledge being acquired by distribution

alone, without the aid of semantic information. Is this what you mean?
What about `semantic bootstrapping'.  E.g., knowing what `eat' gives one
a quick way of concluding that it takes two arguments, which is very useful
if one is learning a language such as Japanese with massive omission of
nominals (in positions where English would use pronouns). Semantic
bootstrapping has been advocated by all sorts of people, including hardline
orthodox generativists like Jane Grimshaw, and informal functionalists
like Bob Dixon (and also Jim McCawley).

Avery Andrews

Date: Mon Jan 06, 1992 4:06 am PST
Subject: out for a bit

I will be reading but not responding much for a while.

B ruce Nevin
bn@bbn.com

Date: Mon Jan 06, 1992 4:23 am PST

[From Chris Malcolm]

Gary Cziko writes:

> Funny how "I couldn't care less" and "I could care less" mean the same
> thing, even though one has a "not" in it.

Only to North American English speakers.

Date: Mon Jan 06, 1992 1:40 pm PST
Subject: Recursioh n, graphics

[From Bill Powers (920106.0800)]

Avery Andrews (920105) --

Thanks for insight into PROLOG. I hadn't realized that it's a
fundamentally recursive system. Your explanation of how it works shows
why the brain isn't likely to use recursion (aside from lacking a stack
that can save the machine state (!) after each recursion).

>The function is defined as a relation, such that factorial(X,Y) is true
>if X is the factorial of Y. If we make the `query' `factorial(0,1)',
>the prolog system will respond `true', if we query `factorial(1,0)' it
>will respond `false', while if we query, say `factorial(2,X)', it will
>respond `true, for X = 2'.

Yes, this is truly a relation, in that it has more than one output.

Finding an inverse factorial function with a control system is done by
making the input function the factorial function. The system's comparator
would receive Y as a reference value and X! as the perceptual signal. The

perceptual signal would come from the input function, which receives X as an input and computes X! as the perceptual signal. The error signal, e = (Y - X!), would be the basis for changing X: X <-- X + k*e, where k is selected to allow convergence for the largest value of Y (rather than oscillation). If convergence (zero error) is achieved, X will have the value such that X! = Y. Interestingly, if Y is NOT the factorial of anything, X will become the number whose factorial is nearest to Y. The perceptual function, in this case, has to truncate the input to an integer.

Detecting the truth value is now equivalent to detecting whether the error signal is zero. This can be done in a number of ways, the simplest being for a higher-level system to receive a copy of the error signal as perceptual input. This possibility (of higher systems perceiving error signals of lower systems instead of their perceptual signals) is not part of the "formal" HCT model, but it's been considered several times, for other reasons (one being model-based control).

If instead of the factorial, we want the input X to be the square root of the reference signal Y, we simply change the input function so it squares its input. Now the zero-error condition is Y = X*X. The error signal, as before, is multiplied by a factor k small enough to allow convergence, and is added to the current value of X: X <-- X + k*error. If Y is a negative number, the error won't go to zero.

If we put a slowing factor into the output, so the actual correction is only half the computed correction on each cycle, we will obtain a stable control system, and also an exact replication of Newton's Method for calculating square roots. X can start with any value.

```
 1. p = X*X
 2. e = (Y - p)/2
 3. X = X + e
 4. if (abs(e) > minimum) goto 1
```

We can set up a similar control system that will find a word X such that Y is the meaning of X. The reference signal is Y, the meaning. The input to the perceptual function is X, a variable word. The input function, using the same principle, finds the meaning of X (by reference, I suppose, to memory, using X as address and obtaining the stored meaning of X as a previous perception of something). The perceptual signal is then the meaning of whatever word X is present at the input. The error is the perceived meaning of X compared with the reference-meaning Y. If this error is not zero, an output must be generated that changes X -- that tells a lower-level system to find another word. This process will continue until a word is found whose meaning matches Y. If no such word is found, the error signal will never go to zero, and a higher system will detect that there is no word with that meaning.

Obviously, the practicality of this model would depend very much on how a difference between the meaning of a word and the wanted meaning is converted into a process for finding another word. This may be somewhat simplified by remembering that above the level of events (at the most) words are only signals indicating THAT a particular word has occurred, and that words are used for the most part as names of CLASSES of perceptions, not specific ones. When a wanted meaning is specified, the initial state is maximum error (no word at all being perceived). This error signal might be used to invoke a very large general class of words, from which any one will do as the first trial input. This feels somewhat right, in that when we search for words with the right meaning, we do get a sense that a word is only approximately right, more right, almost right, and just right. So there seems to be some space in which errors help us move toward better selections.

>The difference between this and interesting cases of control, I
>speculate, is that in control, the definition of the goal does *not* in
>and of itself provide the means for achieving it: the reference levels
>constituting the desire for the sensation of beer do not contain within
>them anything that implies getting out of chairs, opening fridge-doors,
>etc. Whence reorganization, etc. Perhaps this a sort of degenerate
>case of control, but the flavor is different.

Your speculation is correct. The means of achieving a goal is through selection of sub-goals, and is implemented in the output function, the rule that converts error into a change of lower-order goal.

>Many important linguistic goals, such as `produce a well-formed
>sentence', have the logic programming flavor to them, at least as we
>presently understand grammar ...

These can all probably be transformed to the input-controlling mode: "Produce a sentence such that its perceived form matches a specification for 'well-formed'." Parsing is a perception-like, not an output-like, process. I have a vague concept of a level at which the perceptual functions parse their inputs (perhaps on many bases at the same time, in parallel), producing signals describing the sentence in linguistic terms. Control (by reshuffling the elements of the sentence) is then a matter of making the descriptions match those that are intended. I think this is something that just has to be tried, to see what the real problems are.

> ... just about everyone (or at least Chomsky & Harris followers) seems
>to think of it as some sort of assembly of prefabricated bits, subject
>only to the requirement that the bits fit together, & have their demands
>for neighbors satisfied.

Again, this can be transformed into a CT model. The bits don't demand that they fit together or have certain neighbors: something that perceives the bits and their relations institutes these demands in the form of reference signals. Every "requirement" can be transformed into perception of some aspect of the bits, and a corresponding reference level for those aspects. One of the secrets of building a CT model of something is to recognize that all properties of the things being controlled are put there by some perceptual process. It's necessary to get away from the descriptive mode that attributes agency to parts of speech -- for example, the phrase "adjectives modify nouns" gives adjectives a property they can't actually have. Operators don't "take" arguments -- that's just a figure of speech, a metaphor. To build a model we have to get rid of the metaphors.

Greg Williams (920105) --

I thought of PictureThis right away, of course. But it has some severe disadvantages, which you noted, for this particular application. One of the main requirements, I think, is that the graphics be communicable as ASCII strings -- few people will be able to transmit or download binary files. The net won't transmit them. Few people will be able to make anything of a PostScript ASCII output, lacking PostScript (Display or otherwise) and lacking a laser printer. Transmitting bit images via BURN would be horribly wasteful, so that leaves us needing some simple command language and a program for reconstructing diagrams.

>I assume PRACTICALLY EVERYONE has access to a cheap IBM-PC and a
>PostScript printer (as a last resort, in every medium- or
>bigger-size city, there is a typesetting service bureau which
>will print out 300-dot-per-inch drawings from IBM-format
>Encapsulated PostScript files for around $1.00 per page).

There isn't any such thing as a cheap PostScript printer. Even if buying the printer doesn't cost "much", operating it is costly. Many of our Netters probably don't ever use a PC -- they have Sun workstations and the like, or work directly on VAXes, or on mainframes, with terminals instead of computers. And $1.00 per page for looking at diagrams connected with this net would get damned expensive after a few days, not to mention inconvenient when you have to run into town with a floppy just to see what someone sketched as a 3" x 4" diagram.

Don't get me wrong: I HIGHLY RECOMMEND PictureThis. At a laughable cost, it will produce professional-quality output, and it's very easy to use. I just don't think it would be what this Net needs right now for communicating simple diagrams in a way that essentially everyone will be able to take advantage of.

As to my fees for beta-testing BURN, it's a funny thing: they come out to be the same as the shareware price, to the penny. Plus a $1 fee for having to refer to yet another program by yet another cute name, pal.

Chris Malcomb (920106) --

The secret of North American language use is that phrases like "I could care less" aren't really phrases. They're words with spaces in them. Another way to spell the same word is "SO?" Bruce Nevin is right: language becomes a thing in itself, the general tendency being to reduce it to a grunt. Linguistic entropy.

Avery Andrews to Nevin (920105b) --

>What about `semantic bootstrapping'.  E.g., knowing what `eat' [means]
>gives one a quick way of concluding that it takes two arguments,...

Just what I was proposing a couple of days ago. How come every great idea I have already seems to have a name?

Best to all,

Bill P.

Date: Mon Jan 06, 1992 1:40 pm PST
Subject: Re: VERSION 0

[Martin Taylor 920106 12:30]
(Greg Williams 920106 and Bill Powers920105.1400)

I posted my demurral to Bill's proposal for diagrams before seeing Greg's response. I think Encapsulated Postscript is a quite acceptable solution. It will work with most graph and word-processing applications on Macs, and if there is a PC program available, that covers another large chunk of the market. I don't know about UNIX workstations, but I would be very surprised if most of them could not handle it.

Martin Taylor

Date: Mon Jan 06, 1992 1:42 pm PST
Subject: Re: diagrams

[Martin Taylor 920106 12:00]
(Bill Powers 920105.1400)

Bill proposes the generation of a new language for sending diagrams in the mail. Agreed we need some way of doing this, but I don't think the generation of a new language specific to CSG-L is a good idea. Graphics languages have been being created since the dawn of electronic computation, and many people have studied their embedding into mail systems. Before creating a new one, I think there should be some consideration of what is available (I don't know, myself, what is available). The usual history of a graphic language is that someone describes a nice simple language such as Bill proposed, and then it is found to be inadequate for some purposes and people add new constructs or a one-level macro or subroutine operation, and before you know it, you have another complex monstrosity that no-one outside the original small group uses. Eventually it dies.

Around 1970, for example, the graphics group at UofToronto developed a language called GPAC, which looked a lot like Bill's proposal. We used it for the same kinds of purpose as Bill is proposing. It worked fine for a while, and we even used it for publication graphs. But we found we needed things like dashed lines, different line-widths, new primitve forms, co-centred and colinear objects, and so forth. Without them, it was a pain to use.

It seems to me likely that we have three major types of system to worry about: MAC, PC, and UNIX workstation. The MAC is easy, because there are all sorts of graphics programs that produce PICT files. I would not be at all surprised to find that there are available translators from the PICT format to the PC world. If not, then a slightly less frequently available format (in the MAC world) such as TIFF, which is accessible to most UNIX workstations (I think) and to PCs could be used. The advantage of such a format is that it is an existing standard, and furthermore the creation of pictures is not limited to simple line drawings, but can include scanned objects and so forth. It seems to me that the question is not who and how to program a new language for lots of machines, but to look around and see which formats are most widely accessible, especially using public-domain translators.

Putting the formats into ASCII for transmission is also no great problem, since uuencode or its equivalent is available for just about every platform. Pictures in all sorts of format are transmitted every day all over the usenet (including full-colour photography), and are read by people using X-windows on Sun, Macs, PCs, ...

Martin Taylor

Date: Mon Jan 06, 1992 2:39 pm PST
Subject: Control of behavior

[From Rick Marken (920106)]

Here are some more thoughts on the "control of behavior" topic that has been discussed by Greg Williams, Ken Hacker, Bill Powers and Tom Bourbon. There have been some excellent points made. Let me just make this one little point: Without control theory it is impossible to understand the control of behavior. Every theory of behavior that I know of (other than control theory) rules out the possibility of control of behavior. This includes behaviorism!! Only control theory shows why it might be possible to control people at all -- let alone in the strong sense advocted by some behavior modification fanatics.

In everything that I have read about behavior control the only issue delt with is "are people controllable?". Those who say that people are not controllable give no model- based (ie - scientific) reason for believing that this is the case. Those who say that people are controllable propose a model of people that guarantees that people cannot exert control. If, for example, a behaviorist was organized according to his own model, then he could not control the people that his model says he can control.

In order to control, the behaviorist must be organized as a control system. He must be able to specify the kind of behavior he wants to perceive (have a reference signal for the desired behavior). He must be able to perceive the behavioral variable that is to be controlled. And, finally, he must be able to take action that will influence the behavioral variable, bringing it closer to the reference value when it drifts away.

Behaviorists (and others who say that people are "controllable") pay most of their attention to this last aspect of control -- they find that there are ways to influence behavior. What they ignore is how they, themselves, use this information to perform behavior control. Most important, they ignore their own reference levels for behavior. In fact, it is these reference levels that are responsible for the fact that the behaviorist acts to see one type of behavior rather than another. It is also these reference levels that make it possible for the behaviorist to control the behavior of another person (to the extent that he can -- this is what Tom described so nicely; once you know the reference level for a controlled variable, you can control the actions that influence the value of that variable by applying disturbances[incidentally, this works best if you are the only source of disturbance -- as you are in the tracking tasks]). These reference levels are also, ultimately, the reason that long term control of behavior will fail. The controllee cannot be required to bring variables to values that conflict with existing references for those variables or for variables that depend on those variables.

I find it rather amusing that behavioristic models fail because they cannot explain the very phenomenon that they claim to make possible -- control of behavior.

Ken Hacker -- again I ask for some specific recommendations for Chomsky readings about behavior control.

Greg Williams -- did you get my package of papers yet. I'm sure glad to see you posting on the net. I like your control by deception example. Deception is a great way to control people -- while it lasts. It seems to me that it would be hard to describe how this kind of control works at all without control theory.

One last little point -- I think one of the nice things about control theory is that it helps us see that people who want to control other people are neither evil freaks nor benevolent saints; they are just control systems,like you and I. Control theory shows that the essense of being alive is being in control. We all control all the time. When the objects of control are not control systems themselves (things like picks, shovels, soldering irons) then things work out ok. When the objects of control are control systems that cannot perceive variables at as abstract a level as we can (things like horses and LITTLE children) then we can still exert some relatively consistent control. But when we try to control control systems that are like us (other people) we can create ENORMOUS problems. In fact, it is quite natural to want to control -- and that includes control of the behavior of other people. Control has produced enormous success (when we try to control objects) -- and enormous catastrophe (when we try to control the behavior of other people). The value of control theory is that it helps us see why it is not always such a hot idea to do what comes naturally -- at least when it comes to dealing with other people. Sometimes it's a good idea to "lighten up a little" -- control the amount you control. Maybe that's one of the basic "values" of control theory.

Hasta Luego

Rick

****

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Date: Mon Jan 06, 1992 2:57 pm PST
Subject: Scientific Explanation

[from Gary Cziko 920106.1620]

Greg Williams:

As promised, I shared your correspondence with philosopher of science Patrick Maher <p-maher@uiuc.edu> on my campus. Here is his reaction:

>I don't have the time to go into Williams' ideas in detail. But the
>issues he's worrying about have been discussed extensively under the
>headings of scientific realism, and the role of theories in science. Van
>Fraassen's book _The Scientific Image_ is a sophisticated statement of
>an empiricist view that allows theories (models) to play a role in
>research. For further discussion of that, I'd suggest Churchland and
>Hooker (eds), _Images of Science_.

>
>Patrick

I am not familiar with either of these books, but I will try to take a look soon.--Gary

Date: Mon Jan 06, 1992 3:29 pm PST
Subject: Re: language etc.

[Martin Taylor 920106 17:30]
(Bill Powers 920104.0900)
>
>
>Re: information.
>
>Information in its nonstatistical sense gets us into epistemology,
>doesn't it? That is, to say that message A contains information about B
>implies that you have a way of knowing what B really is, so you can say
>in what respect the message informs us about B. It's also implied that
>you can tell when the information about B is wrong. In any case, the
>presumption is that there is something objective to which the message can
>refer, but even more that we can know what that something is in some way
>other than relying on what the message has to say.
>
No, I don't think there is any such presumption. Since you have used A and B to refer to message and referent, I'll use S and H to refer to speaker and hearer. From the viewpoint of H, S has emitted one message out of a range of possibilities. It is "about B" in the sense that H has some <model|imagined percept|concept> that is labelled "B", and the message received by H modifies that <model|imagined percept|concept>. H cannot tell that the message is wrong about the <real-word-if-there-is-one> thing that B refers to. But H can tell if (1) Message A has an improbable structure whereas a likely modification can give it a probable structure (thus correcting a mis-speak, for example), or if (2) Message A affects the <model|imagined percept|concept> labelled "B" in a way that is improbable given whatever else H believes about B.

> One could just as
>easily transfer this argument to the question of perception. A perceptual
>signal is a message. To what does it refer, and how can the system
>containing the perception know that?
>
Exactly why I see communication as a feedback process. Just as the world's reactions to one's actions affect one's percepts and bring them to a desired state if the world is appropriately structured (i.e. one has developed an internal structure that enhances the likelihood of appropriate actions), so the reactions of H to the messages of S will be appropriate if S has internal structures that adequately match H's behavioural structures. In my pre-PCT nomenclature, S must model H in order to generate the message that will have the desired effect on H. H must model S in order to determine the effect S desires (though this latter modelling can sometimes be omitted if the effect is automatic--uncontrolled).

The underlying catch-phrase is "One never knows." But one can believe, and can test that belief by trying to manipulate the world in ways that would not work if the belief were false. That seems to me to be as far as one can go in interpreting the word "refer." A perceptual signal allows, evokes, assists, induces ... action. That is what it refers to, I think.

I don't know whether you should say that information is non-statistical, unless you have a very specific Newtonian view of what statistics means. What "real" probability out there does your "statistical" probability refer to? The same question you asked arises here, too.

I would prefer the term "non-frequentist" if you must have any such term. But I'd rather leave it out entirely, or use "subjective information" to go along with "subjective probability." Even those are a bit funny, since in my view the only "objective" version of probability is what is labelled "subjective." Other approaches always seem to fall into a foggy void when you get too close to them.

Martin

Date: Mon Jan 06, 1992 7:36 pm PST
Subject: Re: diagrams

Your idea about an ASCII markup diagram language is great; the problem is that it's been done already. In fact, there are a variety of markup schemes for diagrams, mathematics, pictures, music, flowcharts, etc. The one I use is the LaTeX (The LaTeX User's Guide, Leslie Lamport, Addison-Wesley 1986) `picture' environment. LaTeX is written in the TeX (The TeXBook, Don Knuth, Addison-Wesley 1970) typesetting language. Together they provide an excellent environment for all kinds of technical writing, including math and diagrams, which is public domain and prints to most all laser printers.

There are other good programs that work with LaTeX. GNUPlot is a general function plotter; TeXCad is a WYSIWYG drawing tool. They both write LaTeX (among other things) and are public domain.

The basic picture syntax is relatively simple:

```
        \put(x-coord,y-coord){\object_command}
```

There are a variety of \object_commands for fdrawing variuos boxes, ovals, circles, lines, arrows (vectors), and text. For example, the following demonstration draws a little cart with the word `car' inside it:

```
\setlength(\unitlength}{1mm}    % Let's work in millimeters
\begin{picture}(50,39)  % A picture that big, in mm
\put(15,20){\circle{6}} % Left wheel: circle radius 6 mm, center (15,20)
mm
\put(30,20){\circle{6}} % Right wheel: circle radius 6 mm, center (15,20)
mm
\put(15,20){\circle*{2}} % Left hub: radius 2 mm filled circle inside wheel
\put(15,20){\circle*{2}} % Right hub
\put(10,24){\framebox(25,8){car}}       % Body of car with word `car' inside
\put(10,32){\vector(-2,1){10}} % Handle: vector slope -1/2 length 10mm
\end{picture}
```

The follwoing code produces a picture of a control system, from my article "Control Theory and Cybernetic Ontology" (Techincal Report from

Principia Cybernetica, 1st PC Workshop, Brussels 1991). Gary Cziko's
seen the final output, and can say what it looks like.

```
\unitlength=1.00mm
\linethickness{0.4pt}
\begin{picture}(135.00,80.00)
\put(65.00,59.79){\framebox(20.00,9.89)[cc]{Comparator}}
\put(85.00,40.00){\framebox(20.00,10.32)[cc]{}}
\put(45.00,40.00){\framebox(20.00,10.32)[cc]{}}
\put(55.00,48.18){\makebox(0,0)[cc]{Input}}
\put(55.00,42.16){\makebox(0,0)[cc]{Function}}
\put(37.50,64.74){\oval(25.00,9.89)[]}
\put(37.00,67.96){\makebox(0,0)[cc]{Reference}}
\put(37.00,61.94){\makebox(0,0)[cc]{Level}}
\put(65.00,20.21){\framebox(20.00,9.89)[cc]{}}
\put(75.00,28.38){\makebox(0,0)[cc]{Physical}}
\put(75.00,22.36){\makebox(0,0)[cc]{Quantity}}
\put(37.50,25.16){\oval(25.00,9.89)[]}
\put(37.00,25.37){\makebox(0,0)[cc]{Disturbances}}
\put(50.00,64.52){\vector(1,0){15.00}}
\put(50.00,24.94){\vector(1,0){15.00}}
\put(65.00,30.11){\vector(-1,1){10.00}}
\put(55.00,49.89){\vector(1,1){10.00}}
\put(85.00,59.78){\vector(1,-1){10.00}}
\put(95.00,40.00){\vector(-1,-1){10.00}}
\put(60.00,55.05){\makebox(0,0)[rc]{Perceived Variable}}
\put(90.00,55.05){\makebox(0,0)[lc]{Error Signal}}
\put(95.00,48.17){\makebox(0,0)[cc]{Output}}
\put(95.00,42.15){\makebox(0,0)[cc]{Function}}
\put(75.00,34.84){\makebox(0,0)[cc]{Physical Laws}}
\put(20.00,40.00){\dashbox{1.72}(110.00,34.84)[rt]{System}}
\put(15.00,15.05){\dashbox{3.87}(120.00,64.95)[rt]{Environment}}
\end{picture}
```

Now this was produced by TeXCad, which generated all the decimal points.
You don't need them.

Now you may not want LaTeX, but you might find something else out there
better. It would just be a shame to invest a lot of time and effort into
reinventing some wheel.

Let me know if you want more information.

O-->
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| Systems Science, SUNY Binghamton NASA Goddard Space Flight Center
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V All the world is biscuit shaped. . .

Date: Mon Jan 06, 1992 7:39 pm PST
Subject: Re: VERSION 0

An advantage of LaTeX (or other) markup over Postscript is that it can
be printed on HP compatible printers, and even read a bit without being
compiled or dumped to printer.

O-->
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V All the world is biscuit shaped. . .

Date: Mon Jan 06, 1992 7:55 pm PST
Subject: BURN/VERSION ?

From Greg Williams

Re: BURN[, beta, BURN!] (Bill Powers)

You don't pay yourself enough. We were thinking of $5 at most for BURN. I expect the beta will ship within the next week or so.

Re: Version 0 (Bill Powers, et al.)

VERSION ? MULTIPLE-CHOICE QUIZ (Choose A or B)

A

1. Bill establishes specs for Version 0.1 and writes encoder for IBM-PCs and decoder for his (Epson FX-80) dot-matrix printer.
2. Netters with IBM-PCs use Bill's encoder. They use his decoder if they have an Epson-compatible program or access to a program to make their non-Epson-compatible accept Epson codes (such programs exist for some non-Epson dot-matrix printers and for HP LaserJet compatibles, but they are not free).
3. Netters without IBM-PCs opt either to write their own encoder or not. If they do not write their own, they will find it rather difficult to draw much via WYSIN (what-you-see-is-nothing), and are apt to give up trying. And they opt either to write their own decoder or not; if not, they must decode by hand or not at all.

B

1. The Net establishes A SUBSET OF PostScript as its standard. Bill writes a decoder for his printer or buys a PostScript emulator (NOT NECESSARILY A PostScript PRINTER (though they are down to @$1500 now) OR EVEN A NON-PostScript LASER PRINTER.
2. Netters with IBM-PCs use PictureThis (with restraint) to encode their drawings, WYSIWYG-fashion, and send both .EPS and (burned) .DRW files to the Net. Those with IBM-PCs can load a .DRW file (after unBURNing it) into PictureThis and view the drawing on-screen, do a screen dump (highly detailed in EGA or VGA mode, coming next month in PictureThis Version 4) if they don't have a PostScript-compatible printer, or print the .EPS file to a PostScript-compatible printer. NOTE: both BURNed .DRW files and .EPS files can be embedded in ordinary posts; UNBURN knows where to start and finish. They must use Bill's decoder or a PostScript-compatible printer/program-and-printer to view drawings done by non-IBM Netters.

3. Netters without IBM-PCs opt either to write their own encoder and/or decoder for the PostScript subset we want, or not.

If B is chosen rather than A:

1. Bill doesn't need to write an encoder, but he still needs to write a decoder or buy a PostScript emulator to see PostScript files generated by Netters without access to IBM-PCs. (See, you're not out of a job, Bill! It's about as easy to deal with a PostScript-subset as to deal with your encoding scheme, claims Pat.) SO: Bill writes decoder, no encoder, and the drawings are easier to do (for him) and nicer.
2. Netters with IBM-PCs can see and get hard copies (via screen dump) of drawings generated by other PictureThis users. They must use Bill's decoder if they have an Epson-compatible, or write their own if they don't have an Epson-compatible, to deal with PostScript-subscript files from non-IBM Netters (unless they have a PostScript-compatible printer). SO: Basically the same, but the drawings are easier to do (for those with IBM-PCs) and nicer.
3. SO: Similar (the encoding/decoding, whether dealing with Bill's spec or PostScript-subset), claims Pat. She says she would be willing to help some on dealing with a PostScript-subset, though "it's pretty obvious" and PostScript is super-well-documented (Bill's spec might be, too -- no cut intended!) In either case, the most thrilling part is text. (What did YOU have in mind in this regard, Bill?)

That's the bottom line, as I see it.

Greg

Date: Mon Jan 06, 1992 7:56 pm PST
Subject: Re: diagrams

Oh, also both TeX and LaTeX are available FOR FREE for ALL systems, including DOS, OS/2, Mac, and UNIX.

The only REAL disadvantage to TeX/LaTeX is that they can be rather involved in setup and learning curve. But conceptually they're dynamite, and highly portable and cheap.

```
O----------------------------------------------------------------------------->
| Cliff Joslyn, Cybernetician at Large, 327 Spring St #2 Portland ME 04102 USA
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| cjoslyn@bingvaxu.cc.binghamton.edu    joslyn@kong.gsfc.nasa.gov
V All the world is biscuit shaped. . .
```

Date: Mon Jan 06, 1992 8:36 pm PST
Subject: Carelessness; Control of Behavior

[from Gary Cziko 920206.2000]

Chris Malcolm 920205
>
>Gary Cziko writes:

>
>> Funny how "I couldn't care less" and "I could care less" mean the same
>> thing, even though one has a "not" in it.
>
>Only to North American English speakers.

I had forgotten about our netters outside of North American. I couldn't have been more careless. Or could I. Sometimes I could care less about being careless (or is it that I couldn't care less about not being careless?).

Seriously, I have a hard time believing that "My husband watches football on TV and I could care less" could mean that the lady likes to watch football, too, on TV. I would guess that to you this sentence just doesn't make any sense at all.

Rick Marken 920106

>In everything that I have read about behavior control the only issue delt
>with is "are people controllable?". Those who say that people are not
>controllable give no model- based (ie - scientific) reason for believing
>that this is the case. Those who say that people are controllable propose a
>model of people that guarantees that people cannot exert control. If, for
>example, a behaviorist was organized according to his own model, then
>he could not control the people that his model says he can
>control.

This is exactly what turned me away from behaviorism as a graduate student. One day I realized that if radical behaviorism was correct, then it could not possibly be used to do what the radical behaviorists wanted to do (control others' behavior) since their own behavior would then be controlled. The only way it could work was if radical behaviorism applied to everyone except the radical behaviorists. But then it wouldn't be radical behaviorism any more!

But I knew nothing then of perceptual control theory and Rick's post gives us yet another one of those delightful reversals that PCT is notorious for in explaining how behaviorists can't explain what they do without PCT (Bill Powers gave us another delightful twist yesterday in his argument that to completely control another individual you would have to become his slave).

But Rick, I remember reading somewhere Skinner's attempt to deal with this problem, but I can't remember now what his argument was (although I can remember not understanding it). Perhaps you or Wayne could refresh my memory.

--Gary

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Date: Tue Jan 07, 1992 2:50 am PST
Subject: Kent;Mary;Bill W.

From Tom Bourbon [920107 -- 1:57]
Kent McClelland [920103], your good news concerning the response
from _American Journal of Sociology_ to your manuscript on PCT
and power almost went by without comment. Congratulations! That
will be a nice article. When it is legitimate to cite you as
"in press," let me know -- I am citing your manuscript in one of my
own that will be mailed before the end of the week. And if you
need someone to give your revisions a quick read, I am avialable.
Bill Powers -- did your remark about a trip Mary and you plan through
New Mexico imply that she is recovering from her recent problems?
I certainly hope that was the case.
Bill Williams has been mentioned on CSG-L a few times in recent days.
He was here for two days, departing this morning. He was on his way
back to Boulder from an economics meeting in New Orleans. This was the
meeting where he leveled some serious fire toward his colleagues and
offered PCT as a model of persons, rather than the mushy version of
Skinnerian behaviorism they have embraced in recent years. Bill is
about to go back to work on his book -- a leaner, more focused
version than the one a few of us have seen.
Unfortunately, the day I wanted to get on the net while he was here,
we had problems in our computer center.
Best wishes.

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Date: Tue Jan 07, 1992 2:50 am PST
Subject: Visual updates

From Tom Bourbon [920107b -- 2:20]
Wayne Hershberger, just after I sent my previous post for the morning,
I opened the issue of _Science_ that came today. Have yuo seen the
article by Duhamel, Colby & Goldberg, "The updating of the representation
of visual space in parietal cortex by intended eye movements"? They
describe the fact that the receptive fields of many individual neurons
move with the eyes, so that after the movement, the rfs cover a new
part of visual space. But they have identified some cells whose
receptive fielda shift _before_ the movement. As they put it, "Parietal
cortex both anticipates the retinal consequences of eye movements and
updates the retinal coordinates of remembered stimuli to generate a
continuously accurate representation of visual space." They even
have some data on responses to flashes that seem to me to resemble
the sorts of things Scott Jordan did in his dissertation.
It looks as though the recalibration of coordinates of visual
space, prior to a saccade, that you fellows described, has just
been replicated, at the physiological level.

I would think that a letter to the editor, briefly describing your psychophysical work and its relevance to the physiological study is in order.
Best wishes.

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Date: Tue Jan 07, 1992 5:28 am PST
Subject: Graphics languages

I'm not an expert on graphics formats, but in addition to portability it seems desirable to use one that can easily be pulled into a drawing packages and edited. I don't think that PostScript is very good for this, nor is TeX and its variants. It might be worth considering formats like TIFF and GIF. Note that GIF is the format of choice for the pictures newsgroups and they are widely supported.

>I posted my demurral to Bill's proposal for diagrams before seeing Greg's
>response. I think Encapsulated Postscript is a quite acceptable solution.
>It will work with most graph and word-processing applications on Macs,
>and if there is a PC program available, that covers another large chunk
>of the market. I don't know about UNIX workstations, but I would be
>very surprised if most of them could not handle it.

So far as I am aware, DPS and other packages to view PS files are extra-cost options for many Unix platforms. GIF viewers on the other hand are widely available as freeware and shareware.

Bill (I guess I should sign this "the other Bill"?)
--
Bill Silvert at the Bedford Institute of Oceanography
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Date: Tue Jan 07, 1992 6:03 am PST
Subject: VERSION ? EMPIRICISM

From Greg Williams

Before the debate about a standard for passing diagrams gets much farther, I think we should stop and ask how many folks who do not have access to an IBM-PC compatible are interested enough in diagrams to get encoding/decoding of whatever sort going on their computers/printers. If it turns out that all or virtually all would-be diagram-passers have IBM-PC access, then the PictureThis option is a shoo-in, and we could simply pass around BURNed .DRW files only, with hard copies available as screen dumps or PostScript output, depending upon the individual's printing capabilities.

PLEASE respond to the above question, especially if you don't have IBM-PC access. Thank you.

Greg

Date: Tue Jan 07, 1992 6:34 am PST
Subject: VERSION GIF?

From Greg Williams

(Bill Silvert 920107)

I don't know about Macs, but there are inexpensive shareware drawing programs to produce .GIF files and to convert from, i.e., .PCX files to .GIF. I suspect that, being bit-mapped, .GIF files tend to be huge compared with vector-mapped files (including Bill Powers' proposed spec and either .DRW or .EPS files made with PictureThis). But if size turns out not to be a problem (I assume those at institutions aren't limited), .GIF files sound good for IBM-PC and UNIX users. But I'd hate to have to start downloading daily 150K posts from Bill Powers instead of his usual 15K.

A question: are .GIF files binary or hex?

Make that: binary or ASCII? Sorry.

Greg

Date: Tue Jan 07, 1992 7:02 am PST
Subject: Re: VERSION ? EMPIRICISM

Count one UNIX (IRIS; occasional SPARC) picture passer.

Date: Tue Jan 07, 1992 7:06 am PST
Subject: Response to graphics query

:Bill Cunningham 920107 0930:
Greg Williams--

IBM both at home and at work. Dot matrix printer both places. Access to laser printer at work, but not conveniently. Quick & dirty isfar more useful than slow & pretty.

The catch here is how I have to access the Ft Monroe mainframe. I can't upload/download files from home, and can only screenprint (which includes header/footer crap on each screen). Recently added download feature at work is regarded as technological marvel by locals. Am already having problems with files too big for local mail handler, suggesting that pictures had better be compressed to less than the thousand words they're worth.

Bill C.

Date: Tue Jan 07, 1992 10:58 am PST
Subject: Control of Behavior

[from Rick Marken (920107)]

Gary Cziko (920206.2000) says:

>One day I realized that if radical behaviorism was correct, then it could
>not possibly be used to do what the radical behaviorists wanted to do
>(control others' behavior) since their own behavior would then be
>controlled. The only way it could work was if radical behaviorism applied
>to everyone except the radical behaviorists. But then it wouldn't be
>radical behaviorism any more!

See, that's why you get a cushy position at a prestigious University -- because you're so smart. I just figured it out a couple years ago -- after reading and re-reading BCP.

>But Rick, I remember reading somewhere Skinner's attempt to deal with this
>problem, but I can't remember now what his argument was (although I can
>remember not understanding it). Perhaps you or Wayne could refresh my
>memory.

I bet you do remember it -- and that you didn't understand it because it made no sense. Skinner's answer was "reciprocal conditioning". The contingencies provided by the behaviorist shape the animal and the animal's behaviors (contingent on the behaviorist's actions) shape the behaviorist. Skinner thought of this as a passive process -- the behaviors of controller and controlled are selected by their consequences. Thus, from Skinner's point of view both the behaviorist and the animal are controllers (at least, the way he thought of control). It's not that easy, really, to show why Skinner's view is wrong -- especially without a model. But I think one way to show that it is wrong is to look at the phenomenon of "shaping". The result of shaping is that the animal does some behavior (say, a figure eight walk) and the behaviorist does some behavior (delivers rewards after each walk). If you call "figure eight walk" the reinforcer for the behaviorist and "reward" the reinforcer for the animal, then Skinner could easily "explain" that the behaviorist did what he did (rewarded animal) because he was reinforced with "figure eights" and the animal did what he did (figure eights) because he was rewarded. But during the shaping things were not so simple. The most puzzling behavior was that of the behaviorist -- who rarely repeated giving a reward when the animal repeated an "approximation" to the figure eight. The reward was usually given only after "closer and closer approximations". During the shaping period, what is shaping the behaviorist is not "figure eight walk" -- it never happens. The reinforcer is the ever changing "approximation to figure eight". This reinforcement changes after each occurance of a prior reinforcement. Somehow, the behaviorist's reinforcement (the animal's behavior) knows how to change in just the right way to only increase the behaviorist's probability of giving a reward after the "appropriate" changes in the animal's behavior.

The animal's behavior during shaping can more easily be viewed as passive selection -- because, in fact, it is. The animal is reorganizing and rewards (from the behaviorist) just slow this process. But why isn't the behaviorist's behavior also passive selection? One clue, I think, is the regularity of the response to "approximations" of the goal response. The behaviorist's responses are not perfect but there is a fairly regular relation between food delivery and approximation; an approximation that is perceived as "closer" is rewarded; one perceived as "farther" is not. There is no such regularity in the animal's behavior; what it does after getting a reward may tend to be a repeat of it's prior act but when this produces no reward it is likely to regress to a previous act or do something very different. The animal may be trying to control for reward -- but it's loop gain is VERY low (during shaping). The behaviorist is controlling for "figure eight behavior" and his loop gain is quite high.

Still, it is as diffcult to show that "reciprocal conditioning" is "reciprocal control" as it is to show that plain old operant behavior is actually control of reinforcement. The behaviorist can always say "I got the animal to do X because X was a reinforcer for my behavior". Simple demonstrations of the inability of external events (reinforcers) to control behavior are just not that easy to develop. Any suggestions regarding ways to show that "reciprocal conditioning" cannot be passive "selection by consequences" would be most appreciated.

Regarding the transfer of graphics files: I have access to a PC and just about any software available for it. I don't know much about graphics transfers but I'd like to try it -- see how it works.

Best regards

Rick

****

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Date: Tue Jan 07, 1992 12:15 pm PST
Subject: info

[Joel Judd]

Bill (920104), Martin (920106),

A couple of questions in the middle of this discussion--the word "epistemology" caught my eye:

Bill says:
>In any case, the presumption is that there is something objective to which the message can refer, >but even more that we can know what that something is in some way other than relying on what the >message has to say.
Martin disagrees giving an example of two speakers who rely on models of each other to deal with a messsage.

Bill asks to what a perceptual signal can refer, and how the system can know that.
Martin says this is why he sees communication as a feedback process:
>Just as the world's reactions to one's actions affect one's percepts and bring them to a desired state >if the world is appropriately structured (ie. one has developed an internal structure that enhances >the likelihood of appropriate actions)...

I'm starting to lose track of what is being claimed as individual responsibility and what's being foisted on the environment. Don't we all agree that the "world" is constituted in our perceptions? What does it mean to say "...the world's reactions bring [one's percepts] to a desired state if the world is appropriately structured..."? Is the ie. comment about "internal structures" mean these are synonymous with "world"? Is it accurate to say then that effective communication happens when I say something to J that I believe is what she would most likely understand given my perceptions of her perceptions?

Date: Tue Jan 07, 1992 12:16 pm PST
Subject: Re: Misc comments

From Ken Hacker {920107}

Bill, I agree in principle that what one does with situation is teleological, based on choices and inner-driven motivation and intention. My argument, like those of Vygotsky is that the motivations may be coming more from inner speech which has lost organization and instead has transformed what is in fact social into what one believes is personal. This occurs, for example, with people who are "programmed" into cults. The "snapping" or "deprogramming" when they come out is a liberation from the internalized collectivity. Even the Greeks realized that we are free only within boundaries. I agree with most of your assertions, but still maintain that real freedom comes not from adapting to situations but by creating them.   BTW, I am at NMSU in Las Cruces. I would love to talk with you and to invite you to speak with faculty here in communication studies, psychology, and computer science. We recently had Theodore Sebeok here and we had a wonderful interdisciplinary exchange regarding the origins of human language. Of course Sebeok thinks that everything is semiotics. I observe that theoreticians tend to do this in general. Everyone wants a grand theory. In my view, real explanations of human behavior will come about only through interconnections of various theories and that cybernetics and control theory have ONE explanatory niche, and a very insightful one at that. As a communication theorist, I am interested in how we can relate control theory to communication theory (not signal processing theory) with its focus on social interaction and human relationships.

I can be reached by phone at (505) 646-2801 (office), or 522-4615. Lunch is a great idea. You know like I do how New Mexico grows on you, right?

KEN

Date: Tue Jan 07, 1992 1:32 pm PST
Subject: Re: Carelessness; Control of Behavior

[Martin Taylor 910207 1545]
(Gary Cziko 920206.2000 on Rick Marken)
> One day I realized that if radical behaviorism was correct, then it could
>not possibly be used to do what the radical behaviorists wanted to do
>(control others' behavior) since their own behavior would then be
>controlled.

I am afraid that I fail to see the problem, and I don't think I am being too naive about it. As I interpret Rick and Gary, there is no middle ground between controlling and being controlled. One is either a controller or a controlee (one "l" or twoo for tea?). But this is surely the communication situation in which you have two coupled control systems, each in some manner controlling the other. Such a situation is either stable (cooperative) or unstable (competitive). The deceit situation is one in which one of the parties is necessarily unaware of the relationship, and naturally is unstable, as someone (Greg?) mentioned the other day (or was it Bill?).

Such a situation is not necessarily unstable if the unaware partner becomes aware of the situation in the absence of deceit. I can get you to close a window by asking "Would you mind closing the window, please?" provided you are cooperative (a reference level) and wish to incorporate accomodating me into the references that lead you to action. I could do it by deceit under similar conditions by saying "I'm cold in this awful draft" when you feel warm and cosy, but if the partner found you had a habit of doing that, the "cooperative" reference level might well change.

But I cannot see that this results in a problem for reinforcement theorists or those whom you tar with the brush of "radical behaviourists". It is very commonly said that the baby trains the parents by reinforcement just as much as the parents train the baby. They could, in their use of the term, say "controls" instead of "trains" without doing any more violence to the term than they do ordinarily. I don't see why that turned Gary off, or why people contributing here are concerned about it.

Martin Taylor

Date: Tue Jan 07, 1992 1:36 pm PST
Subject: Re: VERSION ? EMPIRICISM

Count me as a Mac and Sun person -- no PC.

Martin Taylor

Date: Tue Jan 07, 1992 1:37 pm PST
Subject: Re: VERSION GIF?

GIF viewers are readily available for the Mac (several differen versions).
GIF files are usually distributed in uuencoded form, meaning they are binary

but they are distributed as ASCII. GIF format is highly compressed (compactor on the Mac does nothiong with them). But I doubt very mush that they would be as compressed as a sdtet of vector drawing codes in whatever language.

Aren't there IBM-PC viewers and producers for Mac PICT files?

Martin

Date: Tue Jan 07, 1992 1:56 pm PST
Subject: Re: Control of Behavior

[Martin Taylor 920107 16:15]
(Rick Marken 920107)

To forestall a possible response to my posting of a few minutes ago to Gary, I quite agree with and accept Rick's analysis of the behaviour modification procedure. I was only trying to say that I don't see where there is any *extra* problem as compared to a simple S-R analysis. That approach won't work anyway.

But not everyone seems to agree with Rick (or perhaps it is a sign of the truth of what he says): "Simple demonstrations of the inability of external events (reinforcers) to control behavior are just not that easy to develop."

Don't I remember Bill Powers responding to Rick's "Wow" a few weeks back with something like "Wow--that's quite a reinforcement."?

Martin Taylor

Date: Tue Jan 07, 1992 1:59 pm PST
Subject: Brown on L2 Teaching

[from Gary Cziko 920107.1500]

A colleague of mine, anthropologist Jacquetta (Jacquie) Hill is spending the year in Thailand and Japan. While in Bangkok, she decided to take a course in Thai. In doing so, she ran into J. Marvin Brown who directs a language teaching institute and who has written the only published article I know of on language and PCT.

Jacquie has found the teaching method employed by Brown quite effective and brought me back this short description. I'd like to know what people like Powers, Taylor, Judd, Nevin, and Joel Walters (a former colleague now listening in on CSGnet and now hiding out at McGill University in Montreal) think about this application of CSG. I'll hold my tongue (or rather my fingers) until I get some reactions.--Gary

P.S. Some time ago Bill Powers's said Brown was going to join CSGnet. I believe this was based on a misleading impression I had provided. While I would very much like Brown to join us, I have no indication that this will happen.
===

JUST ONE LITTLE MISTAKE
J. Marvin Brown
16 September 1991

For 7 years the AUA Language Center in Bangkok has been teaching Thai by the 'Listening Approach' (that is, the 'Natural Approach' with no speaking by the students). The premise is that the only thing that keeps adults from acquiring a language perfectly (like a child does) is the damage caused by premature speaking (that is, trying to say things before they come by themselves). It hasn't been possible to enforce the policy of not speaking one hundred percent, but the evidence is clear: the less the students try to speak, the better the results.

Throughout history people have noticed that when adults and children go to live in a foreign country, the children end up speaking the language perfectly and the adults don't. With the coming of the age of science, it was only natural to wonder why this was so, and to try to do something about it. Now it seemed obvious to these early scientists that if children could get the new sounds right and adults couldn't, then the adults would need special help. So they developed a science to provide this help. The results were encouraging and the whole field of applied linguistics slowly evolved to where it is today.

But just where are we today? It's still true that children end up speaking the language perfectly and adults don't. Could it be that those early scientists MISSED something? Of course they were assuming that the adult brain is physically INCAPABLE of doing what the child's brain does; but they didn't KNOW this. Wouldn't it have been more scientific to just observe what adults and children do DIFFERENTLY? If they had, they would have seen immediately that adults typically struggle to say things from the very start, while children spend most of their time just listening. And of course everyone knows that BABIES go a full year without saying more than a few words. A real scientist might have wondered what would happen if adults were to do the same thing. But there is no record of anyone ever giving this a try. Not until recently.

In 1985 the AUA Language Center in Bangkok started doing precisely this in their Thai classrooms, and the results are now quite clear. Just as with babies and children, if adults LISTEN AND UNDERSTAND NATURAL TALK, IN REAL SITUATIONS, FOR A YEAR, WITHOUT TRYING TO SAY ANYTHING, then near-perfect speaking will come by itself (and this refers to pronunciation as well as grammar and usage). It seems that the difference between adults and children is not that the adult's brain can't do it RIGHT, but that the child's brain can't do it WRONG. Before children gain the capability of consciously manipulating abstract units, they can't put together anything new. All they can do is listen, and wait for the day when new sentences will come by themselves. They thus acquire the language only from what they hear; and everything they hear is right. But adults CAN try to say things they've never heard, and this is their downfall. For even when they happen to get something right, THE PROCESS OF MAKING IT UP WAS WRONG. And instead of acquiring from the perfect speaking that they hear, they learn from the imperfect things they say, and even worse, from THE AWKWARD STRATEGIES THEY USED WHEN THEY MADE THOSE THINGS UP.

But how is it possible for a SPEAKING ability to emerge from just LISTENING? The answer was provided by the pioneering work of William T. Powers (LIVING CONTROL SYSTEMS, 1989). He showed us that it's PERCEPTION,

not ACTION, that drives behavior. (Powers calls the stored perceptual
units 'reference signals'.) When we hum a tune, we're trying to make the
SOUND of our hum resemble our MEMORY of the sound of the tune (it's like
repeating or copying from a RECORDED perception instead of a LIVE one).
And the correctness of our hum depends on the correctness of our recording
(that is, of our reference signals).

Stephen Krashen made the breakthrough: "We acquire language by
understanding messages, and in no other way." And William Powers showed
that the brain does indeed do things in this way. But there was still one
little mistake that was keeping it all from working. The very thing that
makes an adult an adult was causing irreparable damage. While listening
was constantly sharpening up our reference signals, speaking was tending to
freeze them right where it found them. The lesson from the AUA experiment
is this. If you let your mouth get ahead of your ears, you'll be damaged
for life. But if you don't, you may eventually become a native speaker--or
very close to it.
==

Date: Tue Jan 07, 1992 2:03 pm PST
Subject: Re: info

[Martin Taylor 920107 16:20]
(Joel Judd 920107 13:20)
>
>
>I'm starting to lose track of what is being claimed as individual
>responsibility and what's being foisted on the environment. Don't we all
>agree that the "world" is constituted in our perceptions? What does it mean
>to say "...the world's reactions bring [one's percepts] to a desired state
>if the world is appropriately structured..."? Is the ie. comment about
>"internal structures" mean these are synonymous with "world"? Is it
>accurate to say then that effective communication happens when I say
>something to J that I believe is what she would most likely understand
>given my perceptions of her perceptions?
>
Can't speak for Bill, but in his discussions with Wayne, I think I agree
with him, and my posting was trying to maintain a position consistent in
communication with mine (and I think his) on perception. I assume that
there exists something outside ourselves, but it can be known only through
our perceptions. Our perceptions can be constructed only through the feedback
of our actions to our sensors, but we can develop internal things (which
I called structures to avoid words like "simulated worlds" "world models"
"imagined worlds") that enable us to perform as if there were certain
objects and relationships in the (unknowable) world and not get into too much
trouble by doing so.

Your example of effective communication needs extension. You must believe
that she has interpreted your message in a way sufficiently close to what
you intend, but in addition, you must believe she believes that you so believe,
and you must believe one more level of the recursion (only one--this isn't
an infinite recursion according to our Layered Protocol analyses). If
you like to substitute "perceive" for "believe" I won't complain, though
I might think about it a little.

Does this clarify?

Martin

Date: Tue Jan 07, 1992 2:04 pm PST
Subject: Diagramming language vs. ASCII encoded graphics

Unless I'm missing something crucial, it seems to me that a great distinction is being lost in the discussion of pictures. In writing and printing there's a huge difference between simple line art and diagrams and more complicated pictures. Line art is much smaller and simpler, and should be able to be communicated without all the fuss of device drivers, ascii encoding, huge embedded files, etc.

Here's a diagram:

```
        Reference Level ---------> Comparator----
                                   ^            |
                        Perceived  |            | Error
                        Variable   |            | Signal
                                   |            V
                                Input           Output
                                Function        Function
                                   ^            |
                                   |            V
        Disturbances -----------> Physical Quantity
```

I don't have to encode it in ASCII in a 500 K file, insert it into my message, extrract it on the other end and decode it. What Bill originally asked for was a language to describe DIAGRAMS. Like This:

```
NODE rl, "Reference level"
NODE comp, "Comparator"
NODE if, "Input function"
NODE of, "Output function"
NODE dist, "Disturbance"
NODE pq, "Physical quantity"
LINK rl, comp;
LINK comp, of; "Error signal"
LINK if, comp; "Perceived variable"
LINK of, pq;
LINK pq, if;
LINK dist, pq;
```

Either form is easily understandable, and can be a canonical form. So go ahead and choose GIF or TIFF or whatever, you need it for real pictures. But that's doing something else than transmitting DIAGRAMS in a simple and effective way.

O-->
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V All the world is biscuit shaped. . .

Date: Tue Jan 07, 1992 2:33 pm PST
Subject: Re: Brown on L2 Teaching

[Martin Taylor 920107 16:30]
(Gary Cziko 920107.1500)

Gary, that's a fascinating posting from Marvin Brown. I would never have guessed that one could acquire a second language simply by listening for a long time without trying to speak (it certainly hasn't worked with me listening to Italian movies). But one can't argue with success. Given that the method is successful (and may I be permitted to retain a little scepticism?) then the question is why it works. And here I would differ with what Brown/Krashen suggest.

An impression I have, both from much encounter with people whose native language is not English, and from my own experiences in France and Germany, is that the reference level the talker is trying to satisfy is the the listener understand. Accent is irrelevant, provided it isn't too strange. So is correct syntax. So, when the talker produces an incorrect sentence that is badly spoken but properly reacted to, all the references that are associated with external sensors (i.e. "world" percepts) are satisfied. There remains no error to correct.

When one only listens, all the feedback is in the "imagine" mode. The only reference-percept comparisons possible are those between an imagined utterance and some pattern derived from the heard examples. The comparison is probably vague and unstable (low information rate), but it is properly centred, and eventually it might be possible to correct the error in the imagined utterances and thus the real utterances once the student began to speak the new language. I would guess, if this is true, that faster learning would occur in situations where there is an opportunity for real world percepts to be corrected, they being more precise and less malleable than imagined ones. The problem would be to provide a reasonable control surface around the referent. The normal situation provides a wide area of very low gain (or perhaps that's better treated as a low error signal for a wide discrepancy between percept and reference). It is likely that a non-auditory mode would be better than using auditory feedback for this, such as requiring the student to match visual tracks of pitch and intensity to get the foreign intonation patterns right. I don't know whether it would work, having never tried such stuff, but the analysis seems to suggest that it might.

I've posted too many things today. I should shut up, but it's too much fun.

Martin

PS. The United Nations lists Toronto as the most cosmopolitan city in the world, so I have lots of opportunity to talk with people whose first language is not English. At one time there were over 50 languages used as home language course bases in Toronto schools. I think there are fewer now, because of fiscal restraint. I have a friend, for example, who is teaching elementary CAD-CAM to a class that consists mainly of Ethiopian women, which is not something I would ever expect, even here.

Date: Tue Jan 07, 1992 3:30 pm PST
Subject: Re: Response to graphics query

Have IBM-PC access, & (La)Tex & Postscrit.

Avery Andrews

Date: Tue Jan 07, 1992 4:24 pm PST
Subject: shut up and play yer guitar

[Joel Judd]

Martin (920107)

I think you clarified it.
By the way, I first read your PS. to the Brown comments and thought "So if the UN didn't say so, you wouldn't get to talk to so many non-native English speakers?" Just a little syntactic humor.

Gary (920107)

Some raindrops for Brown's parade:
I have problems justifying SLA pedagogy by saying "It's good for an infant, so it must be good for adults, too."

I don't know what he means by saying everyone hears "perfect" speaking if listening to native speakers. I certainly hear a lot of imperfect English out there. And I certainly wouldn't want to claim that children hear perfect English.

The Input Hypothesis is a "breakthrough"?! Then I've got a theory for 'car driving': in order to acquire driving skills, I need to understand what I'm being told in drivers class. As long as I listen long enough, I'll one day be able to hop into a car and drive almost "perfectly."

Seriously, what Brown has found out, sort of, [aside to those not familiar with language learning methods--the program described is derived from "The Silent Way" approach to language learning] is an aspect of learning which I think is unique to adults (ie. mature control systems) and that is, one tends to approach a novel task with one's current level of cognitive sophistication. The fallout of this seems to be that the more intricate the learning task, the more loath one might be to "retrace steps" and change what can be very entrenched systems. In the case of language learning, it seems that an adult wants it all at once. After all, why should I have to learn new sounds and lexemes, learn to express them in different orders than what I'm used to, learn to categorize my world in different ways, etc.? It takes too damn long! Just tell me the rules and let me memorize them, give me a dictionary to fill in the blanks, and I'll be off! I don't have two, three, or more years to be able to communicate effectively.

On the other hand, children make do with what they have, and seem to be satisfied with that (satisfaction not to be understood as complacent). They're also by no means "quiet," and are also engaged in the fundamental process of wanting to get along with those around them, being "enculturated," or what have you. And that is something which I would wager few adult learners want and/or need to do. The result is, as Martin pointed out, that many, if not most adult learners are satisfied with reducing error in pretty high level goals--buy some food, get a degree (pretty easy to do, too, what with word processors, secretaries, native-speaking friends and other university amenities), rent a room, etc.--all things which can be accomplished without coming anywhere near "native-like" fluency. It takes something different to effect changes in low level systems. That's where I think some emphasis should be placed in SLA research, if one wants to explain "near perfect" language acquisition.

Date: Tue Jan 07, 1992 4:27 pm PST
Subject: Control of Behavior

[From Rick Marken (920107)]

Martin Taylor (910207 -- Canadian time I presume?):

>>(Gary Cziko 920206.2000 on Rick Marken)
>> One day I realized that if radical behaviorism was correct, then it could
>>not possibly be used to do what the radical behaviorists wanted to do
>>(control others' behavior) since their own behavior would then be
>>controlled.

>I am afraid that I fail to see the problem, and I don't think I am being too
>naive about it. As I interpret Rick and Gary, there is no middle ground
>between controlling and being controlled. One is either a controller or
>a controlee (one "l" or twoo for tea?).

I think the confusion results from not having a model. My intuition (and Gary's too, I think) is based on the verbal descriptions of reinforcement theory that say organisms emit behaviors and those followed by reinforcement tend to "survive" and the others become "extinct". The organism is thus "shaped" by reinforcement. The organism itself has no control over the situation -- it is "controlled" by the "contingencies of reinforcement". How can such an organism control anything, let alone the behavior of another organism? Such an organism simply emits responses (for no purpose -- they just pop out) -- some of those responses are "giving reinforcement" responses (made by a behaviorist organism). The "giving reinforcement" responses are then reinforced by the occurance of "figure eight approximations". So we see the behaviorist eventually mainly emitting "shaping the behavior of a pigeon to do figure eights" behavior. The behaviorist is not in control, if this is the way it works. He is just being shaped by the reinforcing contingency (the figure eight behavior of the pigeon). Similarly, the pigeon isn't really in control either -- it is just controlled by the rewards coming out of the behaviorist.

The behaviorist conception of behavior has no place for internal references for particular behavioral results. They sneek purposes in as reinforcements. If the behaviorists agree that a reinforcement is defined by the organismn -- and changed by the organism at will -- then the he is very close to control theory. But as far as I understand it, reinforcement is a property of the things that happen to organisms; so the organism is not in control of reinforcement, the reinforcments control the organsism.

It may be impossible to build a system that actually behaves as reinforcement theory says organisms behave. But if you could (and with some tinkering with the assumptions I bet you could) then I think you would see that Gary and my intuitions are correct; if a reinforcment theorist were organized the way he imagined humans were organised, he might look like he were controlling behavior (when the reinforer was the behavior of an organism) but it would only be appearance. The "reinforcement theory" controller would have no way, for example, to change from having the animal do a figure eight to having it do a circle. As long as the animal was doing a figure eight, the controller would be stuck giving rewards for the figure eight behavior.

So I heartily agree with Gary. Every version of behaviorism that I know of, if taken seriously, rules out the human (or animal) ability to control (as we understand that phenomenon from a PCT perceptive) -- and, hence, to control behavior, the very ability that behaviorism claims to have provided to its practitioners.

Regards

Rick

****

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Date: Tue Jan 07, 1992 4:47 pm PST
Subject: Chomsky and other critics of control

>> Fri, 03 Jan 92 08:45:43 PST
>> "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
>> marken@AERO.ORG
>> Re: Control of behavior

> [From Rick Marken (920103)]

> Thanks to Bill Powers, Tom Bourbon and Ken Hacker for suggestions about
> references on control of behavior. Ken, could you recommend a particular
> one of Chomsky's works that has his views on behavior control. I know of
> his review of Skinner's "verbal behavior" and I seem to recall
> something in the NY Times review of books -- in the early '70s. Is there
> something of his that is more recent. This is exactly the kind of thing
> I'm looking for -- thanks for reminding me about Chomsky.

Rick, there are more sources I am sure I have buried somewhere, but here are few I found on the shelf rather easily:

Otero, C. P. (ed.) (1988). Langage and Politics. Montreal: Black Rose Books.

----> This book is a set of interviews with Chomsky stretching from 1968 to to 1988. Chomsky discusses language, academia, politics, and science. Some of Chomsky's claims are:

a) Intellectuals have a responsibility to expose lies and deception. This is the NY Review of Books essay -- Feb. 13, 1967).

b) Human sciences have been reconstructed on basis of mathematical theory of comm., cybernetics, technology.

{Note that Chomsky says that social and behavioral sciences are inferior to natural sciences and need natural science and mathematics tools}

c) The purpose of social organization should be freedom, not control.

d) "If I am interested in learning about people, I'll read novels rather than psychology."

e) Social sciences have become a technology of manipulation and control.

{Obviously, Chomsky uses "control" in a different sense than we do.}

f) While computers and automation can be used to free humans from menial tasks, they are instead used for weapons R&D.

{I think Chomsky has a good point here since over 70% of funded research in the U.S. is military related. I could get a grant easily if I tailored it specifically to battlefield strategies; if I don't do that, I can wish upon a star...}

To be continued -- someone needs my phone line...

Date: Tue Jan 07, 1992 4:57 pm PST
Subject: Re: Control of Behavior

[Martin Taylor 920107 Universal Earth Time]
(Rick Marken 920107)

I guess we have different ideas as to what constitutes being a behaviourist. My view is that the Watson school of behaviourism died out a sufficient number of generations ago that we don't need to take that school any more seriously as a religion than we take Druidism. My kind of behaviourist is the J.G. Taylor kind. This kind of behaviourist has no problem at all with mental models and related constructs such as intentions, provided that the construct can be justified by consideration of what can be observed entering the subject's sensors and leaving the subject's effectors. If parsimony of description is consistent with the notion of intention, then so be it: intention is a reasonable behaviourist construct.

I have thought of PCT as being a beautifully behaviourist theory, since it provides a very parsimonious description of many phenomena that are hard to explain without it, and it does so by reference to the sensing and effecting done by the organism, without reference to untestable a priori constructs.

As I said, I have no problem agreeing that simple S-R reinforcement theory is of little benefit, but also, I still don't think that the mutual control problem adds to its difficulties anything that was not there beforehand.

Be proud to be a behaviourist. The alternative is to base your psychology on introspection and faith. That can be quite misleading.

Martin

Date: Tue Jan 07, 1992 5:58 pm PST
Subject: Re: Control of behavior

From Ken Hacker [929197]

Chomsky points continued:

g) Scientists and technologies do not have as much conscience as they have a political function in centralized managment and controlling bureaucracies.

{I think that Chomsky is overgeneralizing, but I do wonder how many technologists have any sort of intellectual motivation to work against systems of dominance which are not of their own making, but which are willing to pay them big bucks.}

h) Democracy extends control vertically downward and academics are not helping that process.

{Studies in my discipline done on "CMC" or electronic mail indicate that rhetoric about computers and decentralization are more hype than reality. In reality, new communication technologies increase lateral communication more than decrease downward or increase upward communication.}

j) Academics constitute a "secular priesthood." As Henry Kissinger openly admitted, professors can gain political power by serving the elites.

-------> Chomsky, N. (1987). Ideology and Power: The Manague Lectures. Boston: South End Press.

"...they [academics-kh] have tended to see themselves as managers, either of managers of industry, managers of the state, or ideological managers."

A sociologist, Robert Boguslaw, wrote an interesting book about system design and social changes: The New Utopians, Prentice-Hall, 1965. Some of his claims are:

1. Any computer program or automation procedure/device circumscribes permissable human actions.

2. Psychology and social sciences make people less human by perfecting behavioral control techniques.

3. There is a historical contradiction of freedom and control. To the extent that predictability is increased along with performance reliability, system freedom is reduced.

I think that Boguslaw is interesting reading, but that the premise pinning his arguments is no longer true. That premise is that systems scientists work in total isolation of knowledge and concerns regarding human development, freedoms, creativity, etc. In fact, I would argue that we are seeing cross connections today among disciplines which are breaking down intellectual barriers and allowing us to get close to that strange assortment of knowledge domains which Whitehead argued leads to real insight.

I did not mean to go on too long with these observations. I hope this makes some contribution toward your search for criticisms of control.

KEN

Date: Tue Jan 07, 1992 6:33 pm PST
Subject: Re: Brown on L2 Teaching

Steve,

Thanks for the interesting article. I've passed it on to Doris Kadish. If the theory is valid, our students are already doing their best to be native speakers -- at least the ones I have in my composition & conversation courses!

Mark

Date: Tue Jan 07, 1992 6:34 pm PST
Subject: Behaviorism

[From Rick Marken (at home --920107)]

OK Martin, I admit I am a behaviorist. I study the behavior of the controlled perceptual variables of other organisms.

Ken Hacker -- Thanks so much for the Chomsky refs. I'll run down to the Beverly Hill library this weekend. I love reading radical literature in the Beverly Hills library. I'm just a born again parlor pink, I guess.

Obviously, things are quite boring at work so you'll probably be hearing from me tomorrow.

Best wishes to Mary. Hope to hear from you soon.

Hasta Luego

Rick

A quick PS -- just so the topic does not go away to sleekly.
I still say that there is no behavioral (or cognitive) model
of behavior that explain why people can try to control other
people, appear to control other people, but not really be control-
ling other people (other than control theory, of course).

Later

marken@aerospace.aero.org

Date: Tue Jan 07, 1992 8:19 pm PST
Subject: Graphics for the Rest of Us

[from Gary Cziko 920107.0845]

Greg Williams 920107

>Before the debate about a standard for passing diagrams gets much farther,
>I think we should stop and ask how many folks who do not have access to an
>IBM-PC compatible are interested enough in diagrams to get encoding/decoding
>of whatever sort going on their computers/printers.

I think the more basic question is how many people on the net would take
the trouble to decode files to see graphics, regardless of how they access
the net. This is a very busy network made up of very busy people. Just
keeping up with the reading can be tough for those of us with full-time
jobs. Taking more time to run special programs to view and print diagrams
may not be feasible for many of us.

This is why I have been impressed and pleased with what people like Bill
Powers and Bruce Nevin have been able to do in embedding figures using
keyboard characters directly in their messages. I have done this myself on
occasion and know how tedious it can be to produce such diagrams, but they
can't be beat for ease of reception.

I would therefore like to suggest that those people who need to complement
their words with pictures find or develop software that would make it easy
for them to create such "text-based" diagrams. Such diagrams are instantly
available to ALL netters without any special effort on their part. I would
guess that any graphics posted that would require extra decoding steps
would be seen by only a tiny minority of CSGnetters.

If more detailed graphics need to be shared, I am sure netters will find a
way to do this. I have received Macintosh SuperPaint figures from Kent
McClelland with no difficutly via e-mail. But why bother the general
CSGnet readership with this?

--Gary

Date: Tue Jan 07, 1992 10:02 pm PST
Subject: language

Re Powers (06 Jan 92 15:17:39 CST)

>Your explanation of how it works shows
>why the brain isn't likely to use recursion (aside from lacking a stack
>that can save the machine state (!) after each recursion).

But it does (though to a limited extent)! More on this in another message.

>Yes, this is truly a relation, in that it has more than one output.
And prolog uses a `chronological backtracking' method to (hopefully) grind out all the answers. When if a clause whereby a goal is attempted to be satisfied fails, the system then tries the next clause whereby the goal may be satisfied, and if one does get a final answer, the system can still be kicked (by responding with `;') into trying out the next untried possible path to an answer.

Prolog doesn't implement Newton's Method, because for the sorts of symbolic computations that it is intended for, there aren't known general methods to let you compute appropriate responses to errors.

>When a wanted meaning is specified, the
>initial state is maximum error (no word at all being perceived). This
>error signal might be used to invoke a very large general class of words,
>from which any one will do as the first trial input. This feels somewhat
>right, in that when we search for words with the right meaning, we do get
>a sense that a word is only approximately right, more right, almost
>right, and just right. So there seems to be some space in which errors
>help us move toward better selections.

The nearest I can get to a working model of this is this: the meanings available in the lexicon are stored as a tree, with the most general ones near the root, and the most specific ones out at the leaves. E.g.

```
             kill
         /    |       \
  murder    execute  euthanase
     |
 assassinate
```

(linguists draw their trees upside-down)

At each node, there are relatively few choices, and you choose the most specific word that expresses what you want to say. I have a recent paper about the selection principle, though the domain is selection of the correct morphological form on the basis of grammatical features, rather than the right word on the basis of the meaning. My guess is that the correct way to implement this sort of thing is via some kind of competitive reciprocal inhibition among massively paralell computations, but I have no idea at all of how to make anything like this work for serious linguistic analyses.

>The secret of North American language use is that phrases like "I could
>care less" aren't really phrases. They're words with spaces in them.
>Another way to spell the same word is "SO?" Bruce Nevin is right:

Not quite, since there is an open-ended class of variants of the form:

I could give a(n) X

for various choices of X.

Avery Andrews

Date: Tue Jan 07, 1992 10:18 pm PST
Subject: silent language learning

An interesting story. Prima facie objections are (a) people such as myself can live in places such as Australia for a very long time without picking up the slightest hint of the local accent (b) in cases of language death, there are often large numbers of people who have native competence in comprehension, but zero ability to speak (there is a book by Nancy Dorian on the Scots Gaelic that discusses this).

But there are possible explanations for these. First, moribund languages are typically associated with moribund cultures, and speaking the language is often taken as embracing the culture. In fact, parents trying to cut their children loose from a looser's culture is a (the?) major mechanism of language death. For the Australian case, to speak with an Australian language is basically either to take up identity as an Australian, or to make fun of the place, and for some reason I'm not inclined to do either. Interestingly, Australians in the USA tend to Americanize their accents quite fast, perhaps because the accent itself has little if any cultural significance.

Avery Andrews

9201B

Date: Wed Jan 08, 1992 2:52 am PST
Subject: Re: silent language learning

[From Chris Malcolm]

Avery Andrews writes (about silent language learning)

> An interesting story. Prima facie objections are (a) people such as myself
> can live in places such as Australia for a very long time without
> picking up the slightest hint of the local accent ...

Motivation is important, and not necessarily apparent to the speaker. For example, women tend to speak more clearly, and to speak a form of the language more biassed towards what they see as the form used by a superior social class. Research has shown that this is because women (on average in Western societies) are more concerned with upward social mobility, whereas men (on average) wish to display masculine virtues of toughness, independence, self-reliance, etc., which are associated with the tougher manual workers at the lower end of the socio-economic scale. This is manifested in its most extreme form by the kind of grunting and mumbling which some teenage males substitute for normal speech.

An interesting example of this kind of motivation in action is provided by the accent histories of my sister and I. We grew up in Scotland speaking with Eastern Lowland accents. At the age of 8-10 we spent a few months with a very rich upper-class English aunt. My sister rapidly acquired a lot of an upper class Southern English accent, whereas I picked up mild traces of the local Kentish accent.

In my mid-teens I idolised a emigre Pole who taught me chess and spoke horrible English. I temporarily acquired a mild Polish accent, which was sometimes embarrassing, because I didn't want him to think I was mocking or imitating him.

At the age of 20 we were both students at Edinburgh University, moving in politically left wing circles. My sister identified more strongly than I did with the "workers" and her accent, which began as much more Southern and upper-class than mine, gradually changed to become more Scottish and working class than mine, whereas I reacted to the cosmopolitan mixture of the University by becoming more neutral, the kind of speech which Scots think is English and English think is Scots.

These movements of accents were quite unconscious, and simply due to with which of the many different kinds of speech we were exposed to we identified. The point is that we were exposed to the same milieux of accents, but reacted very differently because of unconscious motivation.

Chris Malcolm
Edinburgh University

==

Date: Wed Jan 08, 1992 5:19 am PST
Subject: Brown study

This is very nice news, and deserves to be better known.

It accords with my experience, where I attained near-native pronunciation and intonation, as I was often told, in Modern Greek. I said little and felt I understood little for perhaps 8 months, then it suddenly began to come together. I was living in Greece, and the language was all around me. I went there (in 1964) expressly for this immersion experience, being frustrated with the standard class/lab teaching of German in my undergrad classes. (I also acquired much greater fluency in German while in Greece. Many Greeks are polyglots--linguists in the old sense of the term--and they would want to practice their English with me. I got to pretending that I didn't understand English. Then many of them began practicing their German with me. And there were many German tourists there too.)

A Penn classmate told me after she had gone on to study with Charles Ferguson at UCLA about his/her idea that L2 learners should start with "babbling" L2 intonation patterns, as children do. One of the often overlooked indicators of a foreign accent is non-native intonation patterns. This could give the L2 learners something to do without locking them into syllable-level mispronunciations. They could then work particular native sounds into their babbling at a later stage, e.g. pa-ta-pa-ta-pa with unaspirated, voiceless, tense consonants and the vowel farther back than for English "father". These would provide possible ways to modify the "listen only" protocol without endangering its gains.

The contrast of child language acquisition to adult L2 learning has
long been one of the cornerstones of Chomskyan biologicism (innate
language-acquisition mechanisms, etc.). Brunner chips at this with
his LASS. Here goes another big chunk.

        Bruce Nevin
        bn@bbn.com

===

Date: Wed Jan 08, 1992 7:05 am PST
Subject: Re: Control of Behavior

[from Gary Cziko 920108.0815]

Martin Taylor 920107 says:

>I am afraid that I fail to see the problem, and I don't think I am being too
>naive about it. As I interpret Rick and Gary, there is no middle ground
>between controlling and being controlled. One is either a controller or
>a controlee (one "l" or two for tea?). But this is surely the communication
>situation in which you have two coupled control systems, each in some
>manner controlling the other.

It seems that Martin and some others on this topic have regressed back to
the pre-PCT notion that behavior is controlled. The basic insight of PCT
is that we do not control our behavior, we control our perceptions.

Now if we do not control our own behavior, how can we possibly control
somebody else's? Doesn't make much sense to me. The most that we may
expect is that we may in certain situations be able to control the
PERCEPTION of another organism's behavior. And this is where the "coupled
control systems" of scientist and rat in Skinner box shows an important
asymmetry. I may be able to control my perception of the rats behavior
with respect to something like pressing a bar or grooming behavior, but
there is no way I can imagine the rat controlling its perception of the
scientist's behavior, except insofar as it directly relates to feeding the
rat (and even here the situation could be set up so the scientist is never
even seen by the rat). Rodents can not have a reference level for
manipulating the the complex behavior of a human (Mickey Mouse being the
obvious exception).

This same asymmetry may exist between an adult and a young child. But Bill
Powers's point is (and it seems right to me) that it can't be maintained
for long between two adults.

So this whole issue, although quite complex, becomes clearer to me when I
remind myself that we don't control our behaviors nor anybody else's. All
we can ever control is perception, not our behavior or the behavior of
another organism.--Gary

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Date: Wed Jan 08, 1992 7:50 am PST
Subject: GRAPHICS

From Greg Williams

(Gary Cziko 920107.0845)

>I would therefore like to suggest that those people who need to complement
>their words with pictures find or develop software that would make it easy
>for them to create such "text-based" diagrams. Such diagrams are instantly
>available to ALL netters without any special effort on their part. I would
>guess that any graphics posted that would require extra decoding steps
>would be seen by only a tiny minority of CSGnetters.

Great idea, Gary! Why didn't I think of that? Putting the onus on the diagram makers and NOT on the diagram readers is Right On, especially for those of us (like myself) who intend to remain readers only, not makers.

Greg

Date: Wed Jan 08, 1992 3:00 pm PST
Subject: library research

[Joel Judd]

Control Theory veterans:

Something posessed me to take a quick look in the Social Sciences Citation Index and see what kinds of people have been citing Bill. In 1991, from Jan-Aug, there were 14 citations of BCP (of which 3 were "in-house;" ie. Bourbon, Marken et al.) including one in the _Netherlands Journal of Zoology_(!) which I haven't looked at yet. In 1990, there were 20 (7 in-house). So for a book that's almost 20 years old, it's still getting around a little, and hopefully will some more.

There were a couple of specific references that looked promising, and perhaps relevant to recent posts. I don't remember if these authors have been mentioned before, but in a recent _Behavioral Science_ there was an article about "self-organization" that had some things to recommend it: some discussion of Maturana and "evolutionary dynamics," and a brief organizational description which seperated proposed developmental*explanations* from what is *observed* at different times in infant development. The ref is:

Kaplan, M. & Kaplan, N. (1991). The self-organization of human psychological functioning. _Behavioral Science_ _36_, 161-178.

Ed, David, others interested:

Since there was a brief go-around with the topic of suicide, this ref
caught my eye:

Baumeister, R. (1990). Suicide as escape from self. _Psycological Review_
_97_, 90-113.

This Baumeister also has a more recent co-authored article on binge eating
in _Psychological Bulletin_ volume 110, page 86. He cites both Powers and
Carver and Scheier in both (the Psychological Review volume has a Carver
and Scheier article on affect at the beginning).

Happy reading!

Date: Wed Jan 08, 1992 3:45 pm PST
Subject: Brown on L2 Teaching/Silent LL r

I've tried for a month or so now to get the PCT/HCT message
through the silent method, but alas my teacher has smoked me
out.

Brown speaks of "perfect" language learning in children as
opposed to adults. My reading of some of the SLA literature
is that a lot depends on which aspect of language we're
looking at. With regard to "new sounds" I would agree with
him and would add to this things like idioms, swear words
and other frozen expressions.

I'm not as sure as Brown that children spend most of their time
just listening. I've watched at least two of my own kids do a
lot of language play, perhaps you could call this visible
inner speech, with no apparent interactive intent (What would
PCT say about this?)

>Brown says: Before children gain the capability of consciously
>manipulating abstract units, they can't put together anything new...
>They thus acquire the language only from what they hear; and everything
>they hear is right.

Does this mean that children never say anything original?
Where are the linguists on this net?
And what's this about "hearing everything right?"
Chomsky and Lightfoot are pretty convincing in their poverty of
the stimulus arguments and
Labov estimates grammatical input at around 75%.

On a more positive note, the notion of
using listening as a way of sharpening reference signals
and speaking as freezing them is very interesting to me.
As an incipient control theory researcher, I've been thinking
about what some of the important controlled variables in
second language use might be. One obvious candidate, unless
my silent method of learning control theory has failed me,
is the speaker's first language. A PCT account of L1/L2
relations could pretty much handle the last 30 years of work

in contrastive analysis and the more current GB/parameter
setting reincarnation of the same ideas.

Date: Wed Jan 08, 1992 4:11 pm PST
Subject: Re: Behaviorism

[Martin Taylor 920108 16:00]
(Rick Marken 920107)
>
>A quick PS -- just so the topic does not go away to sleekly.
>I still say that there is no behavioral (or cognitive) model
>of behavior that explain why people can try to control other
>people, appear to control other people, but not really be control-
>ling other people (other than control theory, of course).
>
Which raises an interesting point: what aspects of a situation lead to
the illusion (delusion?) of control where none exists (a) from the
viewpoint of the (non)-controller, or (b) from the viewpoint of an
external observer. I see this question as the inverse of The Test.
Answering it might say something about why S-R theory had/has so
strong a hold on so many people's minds.

Martin

Date: Wed Jan 08, 1992 4:18 pm PST
Subject: Re: Control of Behavior

[From Rick Marken (920108)]

Gary Cziko (920108.0815) says:

>It seems that Martin and some others on this topic have regressed back to
>the pre-PCT notion that behavior is controlled.

Not me, I hope.

>   The basic insight of PCT
>is that we do not control our behavior, we control our perceptions.

>Now if we do not control our own behavior, how can we possibly control
>somebody else's?

>   Rodents can not have a reference level for
>manipulating the the complex behavior of a human (Mickey Mouse being the
>obvious exception).

These are excellent points. The control of behavior situation is asymmetrical
when the systems involved differ in terms of how "high" they go in their
perceptual hierarchies. An organism that can perceive a contingency (program
level, I think) can control this kind of variable (as Skinner did) and one
that cannot (like the rodent and small child) cannot -- indeed, the rodent
or child could not even notice that it's behavior was part of a contingency,

whether they had the means to influence that contingency or not.

>So this whole issue, although quite complex, becomes clearer to me when I
>remind myself that we don't control our behaviors nor anybody else's. All
>we can ever control is perception, not our behavior or the behavior of
>another organism.

I agree with your comments, Gary, but I think your conclusions are based on acceptance of PCT as a model of behavior. Of course, I accept that model too-- The problem that I think we are running into is the notion that there are versions of reinforcement theory that are equivalent to PCT. I think this is Martin's point when he says that the religion of behaviorism is dead and that present day behaviorism allows for intentions and such. I'm reluctant to accept this evaluation. I have heard TOO MANY times that modern versions of reinforcement theory or Pavlovean theory or whatever are completely compatible with PCT. As I've said before (like a year ago), if this is true then why don't I see any papers in the operant conditioning literature that report tests for controlled variables -- systematically? Why don't we see animal studies that measure loop gain and other control characteristics once a controlled variable has been established?

I believe that what reinforcement theorists (and other behaviorists -- and cognitivists for that matter) DO is more important than what that SAY, especially when working models are not a big part of their program. By this criterion, reinforcement theory has precious little to do with PCT. I think the notion that reinforcement theory (of any flavor) is compatible with PCT is the most insidious form of the "nothing but..." syndrome. It is a sure way of preventing an understanding of control theory. Since I think that understanding PCT (and, hence, human nature) is EXTREMELY important to the future welfare of humanity, I don't want to delay investigation of the model because people think it's already being investigated.

I am opposed to reconciliation between PCT and reinforcement theories because no such reconcilition is possible. The "control of behavior" issue gets right at the heart of the difference (philosophically if not methodologically) between PCT and other theories of behavior. Theories that claim that people can be controlled are inherently paradoxical-- because, if they are correct, they cannot apply to the would be controller. So, yes, Martin, I believe there is a difference between systems that can be controlled (non-living systems) and those that cannot (living ones). The mutual APPARENT control of control systems by one another is probably better called defacto cooperation. This type of control is strictly circumscribed -- each controlled variable (the behavioral variable of each system that is "controlled" by the other system) can only be kept at levels that do not conflict with the other goals of the system being "controlled". Control should be (and when dealing with the inanimate world, is) made of sterner stuff.

Hasta Luego

Rick

****

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Date: Wed Jan 08, 1992 4:24 pm PST
Subject: Re: Brown study

[Martin Taylor 920108 16:10]

A small anecdote--

My French is not good. Enough to carry on a halting conversation and to deal easily with hotels and the like. A couple of years ago my wife and I drove through France both ways between Germany and Spain, with a two-week period in Spain between the two crossings of France. On the way to Spain, my experience with French was as I described. Now, I speak no Spanish, and it turned out that in most of the places we visited, the people spoke no language I understand even a few words of. By dint of gestures and very hard listening, we managed to get along for the two weeks. But on returning to France, I found my understanding had become essentially perfect, in that I almost never missed a word, and I felt I could speak and be understood almost as freely as in English. It was a very liberating experience. Now my French is as bad as ever. I wonder whether the "silence" learning procedure might be related, although in this case I was neither hearing nor trying to speak French. All the same, I was trying hard to hear some sense in a foreign language, and that in itself may have made it easier to deal with French, in much the same way as exercise strengthens physical muscles.

Martin

Date: Wed Jan 08, 1992 6:57 pm PST
Subject: Re: Control of Behavior

[Martin Taylor 920108]
(Rick Marken 920108)
>
>-
>The problem that I think we are running into is the notion that there are
>versions of reinforcement theory that are equivalent to PCT. I think this is
>Martin's point when he says that the religion of behaviorism is dead and
>that present day behaviorism allows for intentions and such. I'm reluctant
>to accept this evaluation. I have heard TOO MANY times that modern versions
>of reinforcement theory or Pavlovean theory or whatever are completely
>compatible with PCT.
>

I didn't mean to imply that these theories are compatible with PCT. All I wanted to say was that the radical behaviourism of Watson et al. that claimed no scientific psychology could incorporate "mentalistic" ideas was long dead. I don't at all believe that the reinforcement theories about which you complain are equivalent to PCT. But there is an issue of what constitutes the delusion of control.

(Gary Cziko 920108)
>It seems that Martin and some others on this topic have regressed back to
>the pre-PCT notion that behavior is controlled. The basic insight of PCT
>is that we do not control our behavior, we control our perceptions.
>
>Now if we do not control our own behavior, how can we possibly control
>somebody else's? Doesn't make much sense to me. The most that we may
>expect is that we may in certain situations be able to control the
>PERCEPTION of another organism's behavior. And this is where the "coupled
>control systems" of scientist and rat in Skinner box shows an important
>asymmetry. I may be able to control my perception of the rats behavior
>with respect to something like pressing a bar or grooming behavior, but
>there is no way I can imagine the rat controlling its perception of the
>scientist's behavior, except insofar as it directly relates to feeding the
>rat (and even here the situation could be set up so the scientist is never
>even seen by the rat). Rodents can not have a reference level for
>manipulating the the complex behavior of a human (Mickey Mouse being the
>obvious exception).
>

I have a problem both with your characterization of me and with your analysis. So far as I can see, the rat IS controlling its perceptions--setting a reference level for hunger and achieving the appropriate perception. No, it doesn't (probably) have any internal abstraction corresponding to a model of the psychologist, but it is achieving its desired perceptions through the actions of the psychologist. The psychologist might or might not have a model of the rat, but does act so as to generate the intende perception of the rat performing figure-8's (or whatever).

Obviously, from one point of view, no control system can control anything but its own reference-percept difference, but from another viewpoint there is a closed loop, so that anything on the main data path through the loop can be seen as a surrogate for the percept, and in that sense any surrogate can be loosely seen as being controlled. And since no third party can identify any percept, the surrogate is the only thing that can be identified as being controlled. If that surrogate happens itself to contain (be?) a control system, then there will be all sorts of interesting interplays and dynamic behaviours.

If you want to be strict in the use of the term "control", then no control behaviour can be identified precisely, Rick's demos and The Test notwithstanding. You are still looking at surrogates, which appear to be good ones. If you want to look a little way back around the feedback loop, and accept the surrogate, then you can be less strict, and possibly see control where there is none, as well as where control really exists.

As for "control of behaviour", I suspect that there is some nomenclature problem here. Clearly one CAN control behaviour in the sense that I can put my finger on a predetermined point, as does the Little Man. Of course, what is controlled is the deviation of the finger from the point, and by means of that all the muscle percepts, etc. Nevertheless, the same argument applies as above, and it applies with more force when one considers mentalistic sport training, such as imagining the muscle feel of a golf swing.

I have always found it difficult to segregate the effects of elements within feedback loops, inasmuch as the dynamics is a property of the loop as a whole, not of any small bit of it. Where you have a point in the case of the control of behaviour is that the loop splits into very many branches that coalesce only where the error signal is computed. The more the branches, and the more equal their effect on the error signal, the less you can say that any one of them is what is being controlled. Hence it is proper to claim that it is misleading to talk of the control of behaviour; but it would not be improper if there were only one behavioural path by which the control system could counteract disturbances.

In the observable world, the surrogates for the controlled perception seem to be controlled, possibly imperfectly.

Martin

Date: Wed Jan 08, 1992 7:02 pm PST
Subject: recursion in language

Unattractive as it may seem, recursion is a genuine aspect of language, though subject to interesting limitations. There is an important difference between `central' recursion (a), and left and right `edge' recursion (b+c):

a) The person John gave a book to gave a picture to him.

b) Zack's mother's boyfriend's sister is Lisa Martin (a local track star - this sentence was actually produced by a 10 year old to explain the provenance of a pair of running shoes)

c) you think that I think that you think that John is an idiot. (c.f. Dante's Inferno, canto 13, line 25)

Edge recursions can be managed without much trouble to a substantial depth, before comprehension gives out in a welter of confusion due to inability to keep track of the relationships involved. Central recursions are much more difficult, and basically get to happen only once, in the sense that you can't normally put a central recursion inside another one. For example, all of the three below are reasonably comprehensible:

1a) the dog chased the cat that nibbled the steak.
1b) the cat that nibbled the steak was chased by the dog.
1c) the cat that the steak was nibbled by was chased by the dog.

But there is more of a difference between the following two:

2a) the dog chased the cat that nibbled the steak than John cooked.
2b) the cat that the steak that John cooked was nibbled by was chased by the dog.

Or even moreso:

3a) the dog chased the cat that nibbled the steak that poisoned the guest that John insulted.

3b) the cat that the steak that the guest than John insulted was poisoned by was nibbled by was chased by the dog.

Once upon a time, Chomsky argued that the facts of grammatical structure should be put into one basket, called `competence', where unlimited recursion was allowed, and that the limitations should go in another basket, called `performance' (where they would be safely contained to keep them from preventing theoretical linguists from having fun). But the limitations on recursion are obviously interesting clues as to how the gadgetry works (as usual, there is a literature on this sort of thing, which I am not up on).

Avery Andrews

Date: Wed Jan 08, 1992 7:03 pm PST
Subject: control and external reality

I'm getting the impression that external reality is somewhat undervalued in discussions of PCT, so here's a little story, for consideration.

When considering animals and the lower-level perceptual abilities of people, it seems useful to distinguish the perceptual variables that the organism is controlling in the PCT sense (`p-controlling', let us say), from the ecological variables upon which the organism's survival and reproductive success (inclusive fitness) depends.  (e.g. distance from hungry leopards, proximity to food-bearing plants & edible animals, etc.).  Inclusive fitness depends on `controlling' these external environmental variables in another sense, which we might call `e-control'. If psi-powers existed, organisms could actually achieve e-control, but they don't, so all they can do is approximate it via p-control, through a perceptual system that has been naturally selected to (approximately) pick up the ecologically significant features of the environment. P-control is what is relevant to understanding how the organisms works, e-control comes in if one wants to know more about why it is set up so as to work that way.

This is a viable approach for animals, and our own more animal-like subsystems, because we think we're smarter than animals are, and that, by doing some science, we can get a better grasp of the variables that are ecologically significant for them than they actually have. But when one gets to the higher levels of human cognition, the picture breaks down, for at least two reasons. One is that one should not assume that the investigator can get a better grasp of the relevant ecological variables than the people being studied have - this may be true in particular cases, but needs to be shown, not just assumed. Another is that humans are capable of creating complex ecological variables that are constituted solely by the perceptual variables of other people. E.g. `heretic'.

In the first case we loose reliable independent access to whatever, if anything, is being e-controlled, and in the latter case, what is being e-controlled is other people's high-level perceptions. In either case, one can't study the systems without already buying into rather elaborate preconceptions about how they work, which is not the case for low level perceptual systems.

Avery Andrews

Date: Wed Jan 08, 1992 10:26 pm PST
Subject: Language, control of behavior

[From Bill Powers (920105.1900)]

Rick Marken (920107) --

> Still, it is as diffcult to show that "reciprocal conditioning" is
>"reciprocal control" as it is to show that plain old operant behavior is
>actually control of reinforcement. The behaviorist can always say "I got
>the animal to do X because X was a reinforcer for my behavior".

I think that the key to sorting this out is to realize that the relationship is NOT symmetrical. You have almost said this in several places.

Each of us controls for perceptions. But we can't see the perceptions of other organisms: all we can see are their actions and consequences of actions, which we call their "behavior." We can apply disturbances to other organisms and watch how they adjust their actions. If we happen to have picked a disturbance of some variable that the other organism maintains at a relatively constant level over long periods of time, we can find regularities -- every time I do *this*, he, she, or it does *that*. It isn't necessary to understand what the other organism is controlling to find these regularities -- this is why behavioral science hasn't been a complete failure. At least in a statistical sense, repeated disturbances sometimes result in predictable actions. The predictions are poor and are usually misapplied, but they exist.

On the other hand, when another organism disturbs what we are controlling, we see immediately what controlled variable is being disturbed. We know what the perception is that's important to us. If we project our perceptions into an objective world, assuming that what we perceive must be equally self-evident to everyone else, the assymetry of the situation is obscured. But if we know that our perceptual worlds are "proprietary," as Wayne Hershberger puts it, we can understand that the other person is not aware of disturbing something we have under control -- the other person is assuming that our corrective actions are just a "response" to the disturbance, and is not privy to our controlled perceptions. So the other person, literally, knows not what he does to us.

The reason that Skinner could shape the pigeon's behavior was that Skinner was observing the pigeon's actions, and not the variable that the pigeon was perceiving and controlling. The pigeon, evidently, has no preference for the path along which it walks; that's only a means for (I assume) making the food appear. In ordinary cirumstances, the wild pigeon goes through a far more complex sequence of actions to bring the food into its vicinity (which we see as bringing it into the food's vicinity): it not only walks, but flies. Furthermore, the pigeon must treat the acquisition of food as a higher-order variable than flying or walking in any particular way. So the pigeon doesn't care how it walks or flies, as long as doing so is instrumental in giving it access to food (and doesn't bring it into danger). Finding food is a control process at a higher level than locomotion.

From Skinner's point of view, the controlled variable is the pattern in which the pigeon walks, as perceived by Skinner. This pattern is not actually a figure eight: it is any pattern that bears some resemblance, as Skinner perceives it, to an idealized 8. The means of affecting the perceived pattern is to supply food whenever something resembling an 8 is perceived.

But this is a misleading way to put it. In fact, the error signal in Skinner is the difference between the perceived pattern of walking and the reference pattern, an 8. The action variable must increase in value as the error gets larger, and decrease as it gets smaller. So the proper definition of Skinner's action is not "rewarding the pigeon" but "withholding food." It is the withholding of the reward that keeps the pigeon searching for a pattern of locomotion that will cause food to appear (which it does in the wild as well as in the lab). The less the pattern resembles an 8 (the greater Skinner's error signal), the longer food will be withheld. As the pattern comes to resemble an 8 more and more, the error becomes less and the period of withholding becomes shorter.

Note that to use any "reward" as a way of controlling behavior, you have to have the ability to withhold it. The withholding, not the giving, is what does the trick.

Clearly, Skinner cannot have any preferred pattern of withholding food. This has to remain freely variable, because it is the means of correcting the error between the perceived walking pattern and the reference pattern, the 8. So in Skinner, perceiving the 8 is a higher-order goal than perceiving any particular length of withholding time.

So in the pigeon, the higher-order goal is obtaining food, and in Skinner it is perceiving a particular pattern of walking. Both organisms will freely adjust the lower-order behavior as a means of controlling the higher-order one. So in the end, both get what they want: the pigeon gets its food, and Skinner perceives the 8. Neither organism controls the highest-order perception that the other is trying to control.

But each controls the behavior of the other -- that is, the actions or the lower-order variables that are the means each uses for control of the higher-order variable.

Joel Judd (920107) --

>I'm starting to lose track of what is being claimed as individual
>responsibility and what's being foisted on the environment. Don't we all
>agree that the "world" is constituted in our perceptions?

I think that Martin and I now agree: each person controls his model of the other person, which may or may not resemble the actual other. I would say that through communication we converge to a state in which each of us is satisfied that he/she understands what the other means and what the other understands. When all our tests are passed, that's the best we can do: we've reached a state of minimum error. That may be a long way from zero error in objective terms, but there isn't much we can do about that.

>What does it mean to say "...the world's reactions bring [one's
>percepts] to a desired state if the world is appropriately
>structured..."? Is the ie. comment about "internal structures" mean
>these are synonymous with "world"?

I interpreted that to mean that control of the world's reactions is possible only if the world is structured so as to permit it. When aspects of the world are not structured as required, control simple fails.

Ken Hacker (920107) --

>My argument, like those of Vygotsky is that the motivations may be
>coming more from inner speech which has lost organization and instead
>has transformed what is in fact social into what one believes is
>personal.

But "what is in fact social" is known to any human being only in terms of perceptions of the social. Nobody, not even a theoretician, is exempt from that. There's an active process of belief and adoption that has to take place in a person before what other people say and believe becomes one's own effective reference signal. And even then, what is adopted is only what one perceives other people to say and believe (and mean).

>I agree with most of your assertions, but still maintain that real
>freedom comes not from adapting to situations but by creating them.

You can agree with me about that, too. Adapting to situations means primarily coping with disturbances. Creating situations means that your higher-level systems can operate to achieve and create new inner goals rather than just defending existing goals against disturbance.

>BTW, I am at NMSU in Las Cruces. I would love to talk with you and to
>invite you to speak with faculty here in communication studies,
>psychology, and computer science.

I knew Las Cruces well in the 1960s. I was then at Northwestern's Dearborn Observatory, and did most of the design work on the Corralitos observatory (architecture to electronics), which is about 5 miles down the road to Deming and north about 6 miles. I used to spend at least two weeks of every summer there. I'd like to see the place again.

When Mary's up to travelling, I'll get in touch. It would be a pleasure to give a little talk based on my demos -- I assume you can lay hands on an AT-compatible computer. Preferably with a projection screen.

>You know like I do how New Mexico grows on you, right?

If we hadn't had kids in Colorado, Las Cruces would have been a likely spot for us to retire to.

Martin Taylor (920107) --

>I am afraid that I fail to see the problem, and I don't think I am being
>too naive about it. As I interpret Rick and Gary, there is no middle
>ground between controlling and being controlled. One is either a
>controller or a controlee (one "l" or twoo for tea?). But this is
>surely the communication situation in which you have two coupled control
>systems, each in some manner controlling the other ...

See first item above. It helps to avoid speaking about controlling "things" such as "other people." What is controlled is a variable. You can't control a car, although you can control its direction, speed, shininess, and so on. In the communication situation, neither control system controls the other control system. Each of them controls for some perceived aspect of the other system, such as indications of understanding. Two coupled systems may be controlling many variables at the same time -- it would be highly unusual for only one variable to be involved.

>But I cannot see that this results in a problem for reinforcement
>theorists or those whom you tar with the brush of "radical
>behaviourists". It is very commonly said that the baby trains the
>parents by reinforcement just as much as the parents train the baby.
>They could, in their use of the term, say "controls" instead of "trains"
>without doing any more violence to the term than they do ordinarily. I
>don't see why that turned Gary off, or why people contributing here are
>concerned about it.

Do I detect a toe being stepped upon? I believe that "radical behaviorism" was Skinner's own name for what he did.

What's wrong with the concept of reinforcement? The idea that it has some effect on an organism that makes the organism behave differently. The baby's cries or smiles would have no effect whatsoever on the parents if the parents didn't have reference levels for their perceptions of those things. "Training" is a poor word for what happens, implying as it does that an organism is caused to change its behavior (and here I mean its pattern of interaction with the environment, not just its output) by an external agency. The agency that changes behavior patterns is located in the organism (the reorganizing system), and behavior patterns never change unless the criteria for starting reorganization are met: error inside the organism. All we can do from outside the organism is to create conditions that would call for reorganization if the organism is disturbed by what we have done. We can't "train" it if it doesn't care about the effects of our efforts. The term "training" hides the fact that learning is a skill of the organism, not something that is done to it.

Taylor(920107b) --

>Don't I remember Bill Powers responding to Rick's "Wow" a few weeks back
>with something like "Wow--that's quite a reinforcement."?

If I did, it was intended as a joke, like looking at a fire in the fireplace and saying "Wow, look at all that phlogiston escaping." I consider the concept of reinforcement to be a major misinterpretation of how behavior works. Despite its popularity.

Gary Cziko et al (920107) re J. Marvin Brown.

I think Brown is onto something. As I have gleaned from the net over the past year, one of the main problems with second language acquisition is learning to utter the language fluently. Two nights ago, on PBS, I heard two Koreans commenting on reunification, etc. One of them, a woman in perhaps her forties, spoke with the typical bad accent that makes it a real strain to understand the words, much less the sense. The other, a man in his fifties or sixties, spoke with hardly a trace of accent and was very relaxing to listen to, by comparison. Both had excellent colloquial vocabularies and good syntax. If the object of communication is to obtain signs of understanding from the hearer, the woman must have great difficulties in conversation, while the man would have an easy time of it.

Marvin speaks of the bad strategies adults use in speaking a foreign language. I believe that this means the strategies of articulation even more than of grammar. When you try to speak a language too soon, before you have a clear perception of normal auditory consequences of articulation in that tongue, you try to adapt the articulations of the first language to those of the new language, using the same kinesthetic control systems -- and especially when an oriental does this to English, it's a mess. What you experience and remember is the mangled articulation, which you imagine to be adequate because it *feels* adequate and in the first language it is adequate. There is no error, and no reorganization. This forever gets in the way of clear speech.

But if you spend a lot of time hearing the sounds of the second language as native speakers produce them, you are building up stores of reference signals for how your own speech should sound. This gives the sound of the speech primacy over the feel of speech. When you then essay to talk, you can hear what your habitual articulations produce and perceive that it is not like the speech you've been hearing. This leads to reorganizing the kinesthetic aspect of speech until the sounds it produces are correct, which is what is really required to speak like a native.

This also speaks to Martin Taylor's comment,

>An impression I have, both from much encounter with people whose native
>language is not English, and from my own experiences in France and
>Germany, is that the reference level the talker is trying to satisfy is
>th[at] the listener understand. Accent is irrelevant, provided it isn't
>too strange. So is correct syntax.

Joel Judd says

>I have problems justifying SLA pedagogy by saying "It's good for an
>infant, so it must be good for adults, too."

>I don't know what he means by saying everyone hears "perfect" speaking
>if listening to native speakers.

If you think of this in terms of learning articulation after experience with the sounds of native speech, it may make more sense.

I'm sorry to hear that Marvin isn't going to join the net. I met him some years ago -- he came to my house to learn more about CT. I think he is a brilliant and original thinker, and he really understands the theory.

Cliff Joslyn (920107) --

Excellent comment on diagrams. We aren't trying to produce art or publishable figures, but only to communicate relationships that 2-D diagrams can express better than linear speech can. My only objection to making diagrams in the usual way at the keyboard is that it takes so long, especially if you want to show anything moderately complicated. And when you're limited to an 80 x 25 screen, the resolution is pretty poor. You can't make several lines terminate at the same point, which creates ambiguities. And so on. I'll put in a little more work on a vector-type encoder/decoder program -- it's really not looking difficult. Maybe I'll try to combine the decoder with a file-displaying program so you can read the mail with figures embedded. That way all you have to do is use the decoder as a file-reader and the figures will magically appear in the text without any fuss. We'll see.

Martin Taylor (920107.1728) --

>My kind of behaviourist is the J.G. Taylor kind. This kind of
>behaviourist has no problem at all with mental models and related
>constructs such as intentions, provided that the construct can be
>justified by consideration of what can be observed entering the
>subject's sensors and leaving the subject's effectors.

My problem with behaviorism is precisely that it purports to observe what leaves the subject's effectors, when in fact "behavior" is always reported in terms of consequences of effector action. Behavioristic explanations always assume that if there is some regular effect of motor activity, the motor activity must also have been regular. This is the only way in which one can account for repeatable consequences without using the CT model.

But between the neural signals entering effectors and the repeatable consequences that we call behavior, there are many disturbances that enter the causal chain, so many that repeating the same command to the muscles would probably never cause the same consequences twice in a row. This fact is routinely ignored by people who call themselves behaviorists. Their arguments uniformly depend on a regular causal chain all the way from the nervous system to the final external effects or patterns. SR theory would make no sense without this assumption; neither would any cognitive theory that assumes that the brain commands actions (intentionally or otherwise).

This assumption of a regular causal path is seldom actually discussed; it is too fundamental to make note of. Instead, we find behaviors being labeled in terms of what the muscles accomplish, with causal explanations then connecting the brain or a stimulus directly to the effect, skipping over the question of the details in the causal chain. "Bar-pressing behavior" is measured not in terms of what the muscles do, but in terms of what the bar does. In fact, it's hard to talk about any behavior without doing this: driving a car, scratching an itch, reading a book -- when you stop to apply control theory to any such behaviors, it's clear that the behavior-word doesn't actually describe what the organism is doing with its effectors. It describes a recognizeable, repeatable outcome created (in fact) by highly variable outputs from the effectors. So what we call behaviors are almost always really perceptions under control. The effector outputs vary just as they must to make the perceived outcome consistent.

So when you say "I have thought of PCT as being a beautifully behaviourist theory," I wonder if you have thought through what behaviorism claims to be about, as opposed to what control theory would say it is really about.

>Be proud to be a behaviourist. The alternative is to base your
>psychology on introspection and faith.

I don't think that's the only alternative. As, perhaps, you can now see.

Avery Andrews (920107) --

>The nearest I can get to a working model of this is this: the meanings
>available in the lexicon are stored as a tree, with the most general
>ones near the root, and the most specific ones out at the leaves.

I think this is progress. A tree of words can also be seen as a set of categories. I can see how the first move in finding a word with a desired meaning would be to select a large category; successively reducing error would then amount to selecting subcategories. As several higher-order systems would be contributing to this selection process at the same time (for perceptual meaning, correct syntax, emotional content, nuances and connotations), the intersection of categories would eventually narrow the search to a small set of words indicating narrow categories.

One interesting implication is that the category level of perception as I have defined it has internal structure -- I think that some of Bruce Nevin's ideas have hinted at the same thing. The structure could be expressed as trees or as classes, subclasses, and so on. So I get a vague hint of how errors at a higher level could lead systematically to narrower and narrower categories that converge on a category that will satisfy all the higher-level requirements at the same time.

> ... there is an open-ended class of variants of the form:

>   I could give a(n) X

>for various choices of X.

Funny, all the variants I can think of would take "a", not "an". All the good Xs seem to start with consonants.

Chris Malcomb (920108) --

>...women tend to speak more clearly, and to speak a form of
>the language more biassed towards what they see as the form used by a
>superior social class. Research has shown that this is because women (on
>average in Western societies) are more concerned with upward social
>mobility, whereas men (on average) wish to display masculine virtues of
>toughness, independence, self-reliance, etc., which are associated with
>the tougher manual workers at the lower end of the socio-economic scale.

See my comments on the Korean speakers, above. The one who was hard to understand was a woman. So the research that provided your explanation is clearly either wrong or worthless: theories that are any good are disconfirmed by a single counterexample.

Bruce Nevin (920108) --

Re: Marvin Brown

>It accords with my experience, where I attained near-native
>pronunciation and intonation, as I was often told, in Modern Greek. I
>said little and felt I understood little for perhaps 8 months, then it
>suddenly began to come together.

I take it that you agree with my interpretation -- What Brown is achieving is a set of good reference signals for how the language should sound, followed only later by learning the articulations needed to make it sound that way. Learning syntax can be done from reading. But speaking has to come from listening.

Gary Cziko (920108) --

Your reply to Martin Taylor says what I said above, only without digressing. Right on.

Best to all,

Bill P.

===

Date: Wed Jan 08, 1992 11:58 pm PST
Subject: Re: Behaviorism

From Tom Bourbon [920109 -- 0:27]

Apparently our campus was cut off of the networks during Monday and Tuesday, the 6th and 7th, so I missed a major portion of the conversation on behaviorism and control of behavior. During that period, I posted a few items and I wonder if they got through. As a check, Wayne Hershberger, did you see my post on an article in the recent issue of _Science_? And Kent McClelland, did you see my post concerning your manuscript?

Martin Taylor [920108], you must not have seen my posts [920103, two of them] in which I informed Rick of two articles, "Mentalism, behavior-behavior relations, and the purposes of science," and "On mentalism, methodological behaviorism, and radical behaviorism." Else you might not have said that the form of "radical behaviorism that says you cannot incorporate 'mentalistic' ideas was long dead." That simply is not true. In both articles I cited, the authors (avowed radical behaviorists) reject any form of mentalistic explanation -- and they include any form of neurological model (e.g., the reflex arc, biochemical changes, and the like) in the category of "mentalistic." (Because those putative explanations represent ideas, in the heads of theorists, rather than observable behaviors.)

Furthermore, they denounce "methodological behaviorism" as scientifically inadequate.

The community of radical behaviorists is alive and active. Steven Hayes, the coauthor of the paper on "Mentalism, behavior- ...," was one of the major figures in establishing the American Psychological Society, as an alternative to the American Psychological Association. One reason he was disaffected with the APA was the influence of people who claimed to be "methodological behaviorists," or "cognitive behaviorists." He is a behavior analyst, a behavioristic therapist,

who seemingly has little use for clinicians and other practitioners
who wrap themselves in imitation-behaviorist cloaks.
Martin, seek out some recent issues of the journals, _Behaviorism_,
and _The Behavior Analyst_. After reading them, see if you still
wish to assert that really radical radical behaviorism is dead and
buried. I think you will see, too, why some of us believe there can
be no thorough rapprochement between radical behaviorism and PCT.
Now, the leap of faith -- is this forsaken campus back in contact
with the real world, or are we once more lost in electronic limbo?

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Date: Thu Jan 09, 1992 5:03 am PST
Subject: Re: Language, control of behavior

[From Chris Malcolm]

Bill Powers writes:

>Chris Malcomb (920108) --
>
>>...women tend to speak more clearly, and to speak a form of
>>the language more biassed towards what they see as the form used by a
>>superior social class. Research has shown that this is because women (on
>>average in Western societies) are more concerned with upward social
>>mobility, whereas men (on average) wish to display masculine virtues of
>>toughness, independence, self-reliance, etc., which are associated with
>>the tougher manual workers at the lower end of the socio-economic scale.
>
>See my comments on the Korean speakers, above. The one who was hard to
>understand was a woman. So the research that provided your explanation is
>clearly either wrong or worthless: theories that are any good are
>disconfirmed by a single counterexample.

The theory that provided my explanation does not assert that _all_ women
or _all_ men behave in the way described, merely that there is a general
tendency. That was the intended significiance of my repeated use of "on
average", which I think is quite clear. Women are on average smaller
than men. This is true, and the millions of individual counterexamples
neither contradicts it nor removes the utility of the fact. If you are
really interested in numbers I could find them, but all the studies
supporting the view described above have been local, e.g., inhabitants
of a small island, assistants in a large department store, dwellers in a
housing estate, students at a university, etc., and the figures are
therefore only of local significance. But as far as I know (I speak from
knowledge gained from an introductory course, this isn't my field) all
of the studies which have looked at this have confirmed the _general_
tendency, and none have disconfirmed it.

By your criterion (good theories are disconfirmed by a single
counterexample) a great many sciences have few if any good theories.
This is a peculiarly Procrustean view of science which I hope on
reflection you will abandon. Indeed, I suspect that it is in the nature

of some sciences to have _no_ theories of this kind -- any historians or philosophers of science care to comment here?

Chris Malcolm

Date: Thu Jan 09, 1992 7:46 am PST
Subject: GRAPHICS FOR WHOM?

From Greg Williams

>Bill Powers (920105.1900)

>My only objection to making diagrams in the usual way at the keyboard is that
>it takes so long, especially if you want to show anything moderately
>complicated. And when you're limited to an 80 x 25 screen, the resolution is
>pretty poor. You can't make several lines terminate at the same point, which
>creates ambiguities. And so on. I'll put in a little more work on a vector-
>type encoder/decoder program -- it's really not looking difficult. Maybe I'll
>try to combine the decoder with a file-displaying program so you can read the
>mail with figures embedded. That way all you have to do is use the decoder as
>a file-reader and the figures will magically appear in the text without any
>fuss. We'll see.

Gary's suggestion takes care of your objection:

>(Gary Cziko 920107.0845)

>I would therefore like to suggest that those people who need to complement
>their words with pictures find or develop software that would make it easy
>for them to create such "text-based" diagrams. Such diagrams are instantly
>available to ALL netters without any special effort on their part.

If you wrote a decoder, I assume it would work for (1) IBM-PC users and (2) perhaps for those who could compile the source for their computer, assuming they could/would do it. And we're back to all of the issues discussed over the past few days. I urge you to concentrate on a program for yourself (other IBM-PC users could get it from you, too) to help you draw pure-ASCIIly.

Greg

P.S. If there are terrible ambiguities in a particular ASCII drawing, several possible remedies suggest themselves: add qualifying text, do it better in a high-res drawing program and USPS a hard copy to those most involved in the particular discussion (others could request it from you), or speak directly with those who are having problems with the ambiguities.

Date: Thu Jan 09, 1992 8:12 am PST
Subject: misc replies

[Joel Judd]

Avery (920108)

For (logical) problems with the performance/competence distinction, see

Campbell & Bickhard (1986), _Knowing Levels and Developmental Stages_, especially pp.64-67. The whole book's only 132 pages, and you might want to read it since Bickhard employs a lot of jargon but also concentrates on Piaget and Chomsky in this book. His main criticism of Chomsky stems from the latter's inability to explain language epistemology.

Chris (920109)

You said in reference to Bill's falsification comment:
>This is a particularly Procrustean view of science...
Actually, from my armchair view of philosophy, it's a particularly *Popperian* view, isn't it? The more I look at SLA "theories" the more I get the feeling that opposition to falsification is a desire to hang on to theories that (a) either weren't good explanations [theories] in the first place (or have the weight of some large research-funding machine behind them and hence are costly to drop), or (b) really weren't explanations at all. There was a long talk given by John Schumann at a recent SLA conference questioning this whole falsification thing. One of the things that falsification implies is that the theory being tested is offered in such a way as to be testable. If you have vague, amorphous theories, then it's easy to have vague, amorphous support for them, and difficult to encounter specific, useful criticism as evidence against. Probably the best SLA example (to continue to flog a dead horse's skeleton) is Krashen's Monitor Model. But I don't see ANY good SLA THEORIES. I've said this a couple of times without comment from interested parties on the net, so I'm increasing my reference level for 'boldness' !

Bill on Brown (920108)

I'm not opposed to implementing the helpful aspects of, shall we say, "passive" experience. But I'm sure there's more to the class than the description gives. I think the message in people's comments on this topic centered around one's _willingness_ and _desire_ to develop good language skills. For learning, I think this is something that should be brought out in the open at the beginning of one's learning experience, and finding an explicit, effective way to do so should be a basis of any learning program. I'm talking about making the learner aware of the need to be aware of error signals. In other words, taking the time with a learner to find out "What kind of speaker do you want to be?" (everyone has some image of a speaker of the L2) and then saying "OK, if that's what you want, here's the kinds of things you're going to have to be aware of..." That's where Silent Way techniques can be useful--when a learner can make use of them. There's also anecdotal evidence, from respected linguists as well as people like me, of the uselessness of being in an L2 environment JUST BECAUSE it's the L2 environment, and getting virtually nothing out of it.

Perhaps this is the beginning of a definition for one of the most misunderstood and misleading of SLA concepts: aptitude. Maybe language aptitude is simply the "plasticity" which comes from knowing how to be aware of error in L2 perception. Those that know, become effective employers of the subtleties of language (and probably learn well regardless of the method, such as the guy I mentioned who learned English from comic books). Those who don't know how, never do really "catch on," regardless of the number of vocabulary words they memorize or syntactic rules they learn. These may benefit from "learning how to learn," but, and here's the kicker, only if they *want* to. If speaking like an "English speaker" disturbs too many controlled variables for L1 culture, then I think it's going to be hard.

Date: Thu Jan 09, 1992 8:35 am PST
Subject: Re: Language, control of behavior

[Martin Taylor 920109 11:00]
(Bill Powers920105.1900)
>
>
>Note that to use any "reward" as a way of controlling behavior, you have
>to have the ability to withhold it. The withholding, not the giving, is
>what does the trick.
>
Surely that's a bit specific, given your general orientation. If the
igeon is conceived as controlling for some level of something that the
"reward" affects, it is neither the witholding nor the presenting of
the reward that counts, but the doing of something that differentially
affects the pigeon's percept level or whatever it has the reference for.
If it is a hunger percept that goes from starve to bloat, and the
experimenter presents foodm the experimenter also has to be sure the
pigeon's current perception of hunger isn't on the bloat side of the
reference. The "reward", it seems to me is indeed a reward, in that it
provides the pigeon with the ability to move its percept toward its
reference. Neither presenting nor witholding is important per se, but
the povision by the experimenter of an environment in which there is
a difference between conditions in which the pigeon can affect its error
signal and one in which it can't.

Martin

Date: Thu Jan 09, 1992 8:43 am PST
Subject: Re: Behaviorism

[Martin Taylor 920108 11:30]

Tom Bourbon points out that the psychological Flat Earth Society is alive
and well. I am astonished, since I was taught 35 years ago that its
exponents had died long since (in an academic sense), and I had never
come across a live one in my subsequent career as a psychologist. One
lives and learns. Who do they worship? Caedmus? Anyway, if they exist,
then my preious comments to Rick are retracted. They give behaviourism
a bad name, as the Nazis gave the swastika a bad image.

(I mailed privately to Tom, but reconsidered making my comment publicly)

Martin

Date: Thu Jan 09, 1992 9:41 am PST
Subject: Re: Language, control of behavior

Ken Hacker [920109]

In response to Bill on control sources:

There is work in psychology and mass communication regarding the Sleeper Effect which demonstrates that people can remember ideas and not the sources of those ideas. Also, if they reject the ideas because of the sources, as they forget the sources (reason for rejecting the messages), they are less resistant to the messages. Some of my own observations of political discourse formulations indicates that people have political opinions, but do not have clear understanding of where those opinions come from. Studies of political decision-making have also shown that people internalize heuristis wwhich are socially constructed. Those heuristics may seem personal after a while. Thus, yes, but no, and I enjoy your feedback. KEN

Date: Thu Jan 09, 1992 2:12 pm PST
Subject: Behaviorism, reality, software

[From Rick Marken (920109)]

Martin Taylor (920108 16:00) says:

>Which raises an interesting point: what aspects of a situation lead to
>the illusion (delusion?) of control where none exists (a) from the
>viewpoint of the (non)-controller, or (b) from the viewpoint of an
>external observer.

I believe that Wayne Hershberger and a student did a research paper on this -- and it is published in Wayne's "Volitional Action" book. It is very easy to demonstrate the illusion of control -- just ask someone to track your moving finger with theirs. If they are willing to do that, then you can control the position of their finger -- you will experience control and your actions will look like control to an observer. Interestingly, the "subject" will not feel controlled until your disturbances (finger movements) require action that produces a perception that conflicts with their ability to control other variables (Bill's example of moving the subject's finger close to a hot soldering iron comes to mind). In such a case, the subject will probably notice your disturbance as an effort to control them and you will notice a loss of control -- especially if you REALLY want the subject's finger to be close to the soldering iron.

Avery Andrews (920108?) says:

>I'm getting the impression that external reality is somewhat undervalued
>in discussions of PCT

External reality, if it exists (and I think there is good reason to believe that it does) is accessible only as perceptual experience. Science provides models of the causes of regularities and relationships in perception -- and we think of these as models of external reality (I think that they are) -- but they are not external reality itself. The fact that these models often must be changed to account for newly discovered perceptual experiences suggests why is probably not a great idea to treat any current model as "reality". I think PCT emphsizes the fact that it is perception, not reality (or the current model thereof) that is controlled because there is no reason to expect that any two organisms will map "reality" into perception in the same way. Thus, the test for the controlled variable is an attempt to map the perceptual variable(s) controlled by an organism into the perceptual variables of the researcher.

I think you were also saying that there are variables in "reality" -- past the organism's perceptions, so to speak -- called "ecological variables" -- that influence the organism's ability to control and survive. I think these are the constraints (and disturbances) that influence the organism's ability to control -- but are not, themselves, part of the organism's perceptions, though they could be part of the observer's perceptions. The electronic relay circuit that determines how many times a lever must be pressed before food is delivered, is part of this "ecological" reality, I think. These kinds of variables are certainly part of the control model. I talk about them as though they were part of the organism's "external reality". Some of these variables are part of my perception (the housing for the circuit, the setting of the counter) and some are models (the electrons that flow through the coils when the relay is closed). But they are included in a PCT explanation of behavior.

Software Note -- While looking though the "Human factors society bulletin" last night I noticed an ad for CONTROL THEORY DEMO software for PCs (286/386) The ad said it has the following features:

Man in the loop control
Graphical performance feedback
Real time visual tracking

Step response task
Compensatory & pursuit tracking
Simultaneous dynamic systems [I don't know what that is off hand -- RM]

Variables:
Control order & gain
Prediction/quickening & time delay
Sum of sines disturbances

Analysis

Time history plots
Mean,sd, & RMS error
Power spectrum data & plots
Bode plots

I will try to get my company to buy this package. It's not cheap -- they want $299.00!!!!!

The only thing this package seems to do that PCT people don't do is Power Spectrum and Bode plots of data. I bet they have little to say about testing for controlled variables. And I don't know if they do any modeling. But I want to look at this just to see what "real" control theorists are up to in psychology.

If you happen to be rich you can order a copy of this software from:

Engineering Solutions
5688 Duquesne Place
Columbus, Ohio 43235

phone 614-459-0344

Hasta Luego

Rick

Date: Thu Jan 09, 1992 11:00 pm PST
Subject: Re: Brown on L2 Teaching/Silent LL r

[from Gary Cziko 920109]

The mesage that started:

>I've tried for a month or so now to get the PCT/HCT message
>through the silent method, but alas my teacher has smoked me
>out.

Should have started:

[from Joel Walters 920109]

(Joel, do you get the hint?)

Joel Walters was at the University of Illinois in 1979 when I arrived and is now at McGill University on sabbatical from Tel Aviv University. Now I have another Joel [Judd] to keep me company.

Joel's message provides a good example of why we should identify ourselves at the beginning of the message text since INJ3000 <INJ3@musicb.mcgill.ca> is a bit too impersonal for CSGnet. (Joel, can't you at least get the INJ3000 name field changed to Joel Walters?)--Gary

==
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==

Date: Thu Jan 09, 1992 11:55 pm PST
Subject: Brown's Language Teaching Method

[from Gary Cziko 920109.22.45]

I was pleased to see all the reaction to my posting of J. Marvin Brown's ideas on language teaching by keeping the ear ahead of the mouth. Lots of interesting interpretations and personal experiences have been recounted (if things ever slow down on the net I may even give my own account of trying to master French, German and Spanish).

I just want to give a little bit more information on the method in response to Joel Judd (920109):

>There's also
>anecdotal evidence, from respected linguists as well as people like me, of
>the uselessness of being in an L2 environment JUST BECAUSE it's the L2
>environment, and getting virtually nothing out of it.

Obviously if this method of not speaking L2 before lots of listening is to work, it must actively engage the students in listening. Jacquie Hill mentioned that the methods uses lots of ways to gets students to listen and try to understand what is going on. One way is to have two native speakers engaging in a conversation with a small group of learners. The learners need to listen and understand but there own interaction initially is in THEIR OWN NATIVE LANGUAGE. Of course, this can only work when all students are already fluent in the same common language.

So this is not just "passive listening" (whatever that is). Trying to understand a foreign language takes LOTS OF WORK. There is lots of error and lots of reorganization going on even if there is no overt behavior.

I'm sure that there must be lots of other interesting ways that Brown uses to get student's to listen and try to understand, but I believe that Jacquie is now back in Thailand and so I'll to wait until the summer before I can find out more about Brown's method (by which time she should be speaking fluent Thai!)--Gary

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Date: Fri Jan 10, 1992 8:23 am PST
Subject: Re: Behaviorism

From Tom Bourbon [920110 -- 2:22]

Martin Taylor [920109],

You asked, "who do they worship," with reference to the radical behaviorists you were taught were dead and gone. The answer, of course, is simple: B. F. Skinner. His work is cited extensively -- as radical behaviorism.

In your private post, you said radical behaviorists might exist, but that "they can hardly be considered worth examining." On that point, I believe you are mistaken. It is precisely the fanciful assumption, and pronouncement, that radical behaviorism is dead and gone that causes most psychologists to be unaware that the errors of radical behaviorism live on -- in methodological and cognitive behaviorism, in cognitive and neuroscience, and in assertions that behaviorism and PCT can be reconciled. Their linear S-R model of causality, which they claim to have abandoned, abound. An article in _Psychological Science_, the organ of the American Psychological Society, makes my point.

The APS wished to commemorate Skinner, so they commissioned an article in the journal. It was written by Robert Epstein, who collaborated with Skinner on a number of publications, a number of them involving animals conditioned to "simulate" allegedly cognitive behavior. The series of articles was intended to show that theoretical assertions concerning cognitive processes are unjustified -- that operant conditioning accounts for all putatively cognitive processes and phenomena.

In his article [R. Epstein (1991). Skinner, creativity, and the problem of spontaneous behavior, P.S., 6, 362-370], Epstein repeatedly reveals the linear causal model that lurks in every variety of behaviorism. That model is incompatible with the understanding of behavior in PCT.

A few brief, but clear, examples, from one who does not present himself as a radical behaviorist:

"A large number of factors converge on an always active nervous system to produce behavior. As thresholds are passed and firing rates increase , circuits controlling the occurrence of many different behaviors are activated." (p. 362) Linear causality, with behavior as a product at the end of a causal chain. By the way, Epstein's references to nervous systems, thresholds, firing rates and the like clearly marks him as a non-radical behaviorist: all of those terms would be considered "mentalistic."

In a discussion of "shaping:" "You'd like a hungry pigeon to turn in circles. You wait for almost any approximation at first ... . Then you immediately operate a feeder and the pigeon eats." . . . "If you continue to operate the feeder at judicious moments, within a few minutes the pigeon will turn in full circles -- while continuing to engage in other behaviors as well." I like this section -- it clearly reveals the notion that the behaviorist must "come to be under the control of that which the behaviorist would control." Notive the casual allusion to starvation as the coercive force behind a version of conditioning whose champions audaciously claim that they use "positive reinforcement" -- the providing of good things to good little rats and pigeons. The control-theoretic aspects of the behavior of the behaviorist are also evident -- who would "like" to see that hungry pigeon turn in circles? Who decides whether the moment is judicious?

"Modern" behaviorists assert that no one believes in S-R causality, but in itsplace they substitute Skinner's old phrase in which "features of the environment" are said to "occasion," or to "set the occasion for," something called "a behavior." Polite euphemisms, designed to deflect the charge that reflexological concepts underlie all of behaviorism. In his article, Epstein appeals to "occasioning," on many occasions, to support the operant-conditioners' assertion that operant behavior is emitted, not elicited.

Finally, "A rigorous analysis of shaping would seem to be within reach. Critical to the shaping procedure is the repeated and systematic witholding of reinforcement". (p. 368) Not often does one find so crisp a declaration of the driving principle in operant conditioning. Coerecively establish severe intrinsic error in your animal, never allow it to eliminate all of that error, then claim that everything the animal does is "occasioned" by "features of the environment" that happen to resemble some of the features that were present when first the animal was conditioned.

I fail to see how Epstein's treatment differs from those of

the radical behaviorists of yore, or those of today, save for
his frequent appeal to "mentalistic" constructs. There is not a
hint of a model that would explain the behavior of an animal
driven to desperate lengths by forced starvation. And there is
no sign of a model for the control behavior of either animal,
or psychologist.
   Some time back -- at least 35 years ago, if your training
is any indication, Martin, psychologists of many stripes began
to assert (perhaps as a wish) that radical behaviorism was dead.
That claim is uttered with some evidence of satisfaction by many
who like to speak of the allegedly "successful cognitive
revolution" in psychology. It ain't so. And the same mistaken
ideas about behavior flourish under different names throughout
the behavioral and life sciences. Submit a manuscript in which
you describe control-theoretic modeling of behavior and you
will see what I mean.

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Date: Fri Jan 10, 1992 11:05 am PST
Subject: subjective information

[From: Bruce Nevin (920110 0734)]

Martin, what do you mean by subjective probability?

Your response (910203 17:15) seems to agree when I equate it with
perceptions of likelihood (however determined) of other perceptions:

>>As I understand it, we PCT folk might think of subjective probability the
>>perception of likelihoods associated with other perceptions. The
>>evidence on which these perceptions of likelihood (or reasonableness,
>>etc.) is based may include frequentist probabilities, but need not.

>I think it might be better to think in terms of the "imagination" loop,
>to distinguish the perception of likelihoods of possible "other perceptions"
>from some externally controllable perception, though, of course, one aspect
>of control is to bring to a maximum the likelihood of a desired perception.

The last clause confuses "likelihood" with the error signal. One aspect
of control is to bring to a minimum the difference between a perceptual
signal and a reference signal, that difference being the error signal.
Likelihood in this sense is your perception, not the organism's.
Are you claiming that the error signal output by an ECS *is* the
likelihood associated with the reference signal (the "desired
perception").

How might an organism come to have a perception that a given perception
is more or less likely, probable, or reasonable? Whence an organism's
judgement that it is either likely or unlikely that an imagined or
remembered perceptual signal (functioning as reference signal) may be
matched by a real-time perceptual signal? This perception of likelihood

or probability is not identical to the error signal resulting from mismatch, and though it might have a relation to the gain assigned to the ECS for that perceptual signal, that relation is neither simple nor direct (depending on higher-level perceptions: "hard to do" may result in higher gain or giving up; "easy to do" may result in being laid back or in higher gain for the sake of enjoyment of the satisfaction.)

A perceptual signal may be familiar: it may be combined with other perceptual signals in the input function of a higher-level ECS, whose perceptual signal is combined with others higher still, and so on, such that remembered perceptions that match the perceptual signal, and which on other occasions are used as reference signals, are available at each ECS.

(Aside to Bill: I find myself surmising here that recognition is a kind of inverse of control, matching memory to perception rather than matching perception to [reference] memory. Interesting thought. Error from mismatch in this sense should result in what we think of as behavior in response to the unfamiliar. Can the model do that.)

Or the perceptual signal might be to various degrees less familiar, due to failure to find a memory for a given ECS. (This failure could emerge at a higher-level ECS, where the perceptual signal at ECS<a> on level n evokes a remembered perceptual signal that goes up to ECS<x> at level n-1, but the concurrent perceptual signal at ECS<b> on level n evokes a remembered perceptual signal that goes up to ECS<y> at level n-1, where the perceptual input to ECS<x> requires other input that is not present (error results in exploration?) or where ECS<x> and ECS<y> perceptual signals do not converge to a single perceptual signal at some higher level but rather diverge to incompatible perceptions. There are other possibilities for unfamiliarity.)

In this understanding of your term "subjective probability," the perception of likelihood arises from the perception of error (in memory and imagination). Such error signals it seems would influence the setting of reference signals at higher levels (choices). But they have no relation to Shannon information, and provide no back-door means to salvage it even as "galumphry" that I can see. In particular, control systems do not "seek to maximize likelihood" as you suggested (as quoted above) unless your definition is that likelihood is inversely proportional to error-in-memory-and-imagination:

```
             error
likelihood = -----
               1
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What do *you* mean by "subjective information"?

Bruce Nevin
bn@bbn.com

Date: Fri Jan 10, 1992 11:31 am PST
Subject: Sheep in Wolves' Clothing

I want to do a (presumably orthodox) control-theory analysis of the situation

I described in my "Lies...." post, slightly embellished.

You might recall that you are my friend. I want you to get up from your ease and go out of the room. A minute ago, you said, "I just want to sit here and relax." I go out of the room and yell: "HELP! I cut my finger!" This is NOT actually the case. Regardless (here comes the embellishing), you reply with: "The bandaids are in the cabinet over the sink." To which I reply, "It's really DEEP!" and throw a towel with a bit of ketchup on it through the doorway, into your field of view. You jump up, run into the room I am in, and are greeted with cheers of "Happy Birthday!!" from a number of friends who have gathered around a candled cake on the table. You sigh deeply and say, "I thought you were really hurt!"

Now the analysis. In this episode, my reference level (of significance here) is perceiving you come into the room where I am. In other words, I am CONTROLLING for that perception. There is an error as long as I don't perceive you coming in; I am trying to reduce that error to zero. My actions taken in order to reduce the error are based on my model of your control hierarchy. I hypothesize that you have a high-level reference signal something like "perceive Greg as unharmed" (no error here at the beginning of the scenario). Suspecting that "perceive Greg as not in danger" will take precedence over another of your hypothesized reference signals (recently expressed verbally by you as something like: perceive lounging comfort), presumably due to the structure of some of your still-higher-order reference signals, which I don't analyze, but simply take as given (I've known you a long time!) for the rest of my model of your hierarchy. So I try to present you with a disturbance which is a bit unusual: a disturbance which will be treated by you as "perceiving Greg as in danger," which (according to my model) will result in an error in that loop which you will attempt to reduce to zero. But I realize that my model must be a bit skewed when you at first tell me about the bandaids, and DON'T come running. So, continuing to attempt to perceive you coming into my room, I present you with another disturbance, which I hypothesize you will treat as "perceiving Greg as in GREAT danger," after which you indeed come in, and act rather surprised.

Now, it seems clear to me that I was controlling for perceiving you entering my room. And I did perceive you entering my room, eventually. I hypothesize that my model (at least the second version of it) of your control hierarchy was correct to the extent that "perceiving Greg as in GREAT danger" was how you perceived my (second) disturbance, and reducing error in that loop took precedence over the "perceive lounging comfort" loop. After all, I DID perceive you as saying you were fooled into believing that I was cut. I suppose I could ask you outright about what you recall regarding the relative strengths of your committments to perceiving lounging comfort and not perceiving me in great danger at the time of this incident, but I'm satisfied -- as I said before, I've known you a long time. It should also be made explicit that part of my model of your hierarchy (version 2) goes something like: "minimizing perceiving Greg as in great danger in this situation REQUIRES getting up and going out to him."

One more time: I was controlling for perceiving you entering my room and you were controlling for perceiving me as not in great danger, which (my model 2 predicted) entails your action of entering my room. So I am NOT controlling YOU. However, your ACTION (entering the room) was DETERMINED jointly by MY control of MY perception and by YOUR control of YOUR perception (and by extraneous environmental disturbances, which I assume can be ignored for the purposes of this discussion). It is, of course, well-known by control-theory rubber-banders everywhere that one can, with another, co-determine that other's actions. The point I want to make is that, in this set-up, unlike in rubber-banding, both people are controlling for the SAME perception (YOU BEING IN MY ROOM), given that you-being-in-my-room is ENTAILED BY not-perceiving-Greg-as-in-great-danger-in-this-instance. One of YOUR loops is controlling for YOUR perception of your being in my room, just as one of MY loops is controlling for MY perception of your being in my room. If I am able to present you with disturbances, BASED ON MY MODEL OF YOUR CONTROL HIERARCHY, which are capable of maintaining an IDENTITY between control in one (or some) of YOUR loop(s) and MY loop(s), then I claim that I am, in effect, CONTROLLING BOTH YOUR AND MY PERCEPTIONS SIMULTANEOUSLY. I am seeing to it that you want what I want, and if I am controlling for what I want, then I am controlling for what you want, since they are the same.

It is a huge irony that B.F. Skinner, who so wanted to "know how to work organisms," was obsessed by the idea of prodding his subjects from the outside only, when he could have done his deeds much more efficiently by using the test for the controlled variable (invented by Bill Powers, who so wants to "know how organisms work") to build serviceable maps of his subjects' control hierarchies so he could better control their perceptions, at least in the devious sense discussed above. But how thoroughly devious is that sense, in the end? In the end is a sigh of relief and a birthday party, not a "boy who cried wolf" moral. When Bill argues that such control of others' perceptions, without physical force, is possible only in the short run, I think he greatly underestimates the complexity and subtlety of human interactions. Surely, all parties reorganize in something akin to co-evolution. Surely, the deceivers sometimes get their stories wrong -- but then promise "never to do it again." Surely, some people become untrusted by anyone -- and then move somewhere else. Control theory indeed provides us with reasons why such control of others' perceptions is difficult. But such control is NOT impossible, even in the long run. It is a dangerous myth that no one but you controls your perceptions. And it is a dangerous myth to believe that control theory (and, especially, the test for the controlled variable) are not prime tools for the kits of chronic deceivers. What do you think political polls are all about? "Hey, I want what YOU want! And here's how we're BOTH going to get it...."

Greg

Date: Fri Jan 10, 1992 3:14 pm PST
Subject: Behaviorism etc.,

[From Bill Powers (920110.1000)]

Greg Williams (920109)] --

>I urge you to concentrate on a program for yourself (other IBM-
>PC users could get it from you, too) to help you draw pure-ASCIIly.

OK, will do.

Joel Judd (920109) --

Re: Falsification

I think that Popper's idea of "falsification" is predicated on the prevailing view of theories as being primarily statistical. Statistical theories don't propose any models, so there is no positive way to verify a theoretical statement. All that significance does for us is to assure us that the experimental results probably didn't happen by chance. There is no a priori or logical argument against the result being a chance occurrance; it is reasonable to admit the possibility that chance played a part. This negative conclusion doesn't tell us that the hypothesis is reasonable, connected to a systematic world, or useful in any context other than the original experimental conditions.

Models, on the other hand, are tested by changing the conditions and verifying that the model still behaves as the real system does under the new conditions. The model provides an a priori systematic reason for the system to behave in some new way under new conditions, and commits us to specifying exactly what that new way will be. When the real system does behave that way, this is a positive indication of the model's worth. Of course one could argue that there is still a possibility that the real system behaved in the new way by chance, but if the standards for acceptance are set as high as they are in the physical sciences, this possibility goes beyond the bounds of reason: there's a qualitative difference between $p < 0.05$ and $p < 0.0000000005$. More likely is the possibility that the real system behaved in the new way for a reason other than the reason for which the model behaved that way. This does not involve chance; it says merely that the model needs to be modified, and that sooner or later circumstances will reveal the needed change. The modeling approach is fundamentally systematic, not statistical. Modelers assume that the underlying processes, whether we have correctly identified them or not, are systematic.

Thus I would say that I use the criterion of *testability*, not falsifiability. Falsifiability is a subset of testability that considers only the possibility of rejection. Testability also demands that an hypothesis that is not rejected be accompanied by a quantitatively correct prediction of new behavior in new circumstances. The kinds of theories Popper was thinking about never went that far.

RE: Brown's SLA methods

>I'm not opposed to implementing the helpful aspects of, shall we say,
>"passive" experience. But I'm sure there's more to the class than the
>description gives.

I'm sure there is, too -- Gary Cziko supports this conjecture today. I also think your approach is realistic: if the learner doesn't want to commit to the effort required for perfect learning, no method will work perfectly. But emphasizing how the language is supposed to sound is bound to improve pronunciation and intonation, and thus communication.

Martin Taylor (920109) --

>If the pigeon is conceived as controlling for some level of something
>that the "reward" affects, it is neither the witholding nor the
>presenting of the reward that counts, but the doing of something that
>differentially affects the pigeon's percept level or whatever it has the
>reference for.

I think you may be overlooking the most obvious thing that is affected by the "reward" -- perception of the reward. You reward the pigeon by giving it pieces of grain, because the pigeon is controlling for eating pieces of grain. If the pigeon's desire for eating grain is zero, you couldn't get it to do anything by giving it grain. The "desire" for grain is made up of (a) a positive nonzero reference level for ingesting an amount of grain, and (b) a perception of ingesting grain that is less than the reference level (i.e., an error). So the reward is identically the variable that the pigeon is controlling -- it is not separate from the grain; it IS the grain, or its perceptual equivalent.

>The "reward", it seems to me is indeed a reward, in that it
>provides the pigeon with the ability to move its percept toward its
>reference.

I claim that there is no difference between a reward and a controlled variable. The MEANS of obtaining the reward is not rewarding -- that is, the apparatus that connects the pigeon's pecking to the release of pieces of grains is not itself reinforcing or rewarding. It is the apparatus that provides the pigeon with the ability to move its percept toward its reference. The percept is controlled by using this means to affect the appearance of the bits of grain that the percepts represent. The pigeon will alter its behavior to bring the perceived amount of obtained reward toward the reference level. Behaviorists, however, reverse cause and effect, and claim that is the reward that is controlling the behavior.

The same is true even when no apparatus is used. The experimenter cannot administer rewards independently of the pigeon's behavior if that behavior is to be controlled. There must be a specific fixed rule that makes administration of the reward contingent on the behavior -- the behavior must be treated as the independent variable, because the reward is, because of the fixed contingency, the dependent variable. The experimenter must obey this rule as mechanically as the apparatus would if the result is to be control of the pigeon's behavior. So the experimenter becomes the pigeon's means of making the reward appear.

>Neither presenting nor witholding is important per se, but
>the provision by the experimenter of an environment in which there is
>a difference between conditions in which the pigeon can affect its error
>signal and one in which it can't.

Withholding is essential, because if one does not withhold there will be no error for the pigeon to correct by producing a reward. Withholding implies much more than merely not delivering the grain: it implies being in a position to assure that the pigeon can't get any grain from another source or by any means other than performing the act you want to see. An employer can't control the behavior of an employee by using a money reward if the employee can obtain money just as easily from another source. There is an elaborate system to make sure the money has to come from an employer -- there are laws, for example, against stealing money, and work requirements (and uniform business hours) that make it difficult to seek other employment while still earning money.

Behind the concept of reward (which is touted as "nice" control) there is almost always coercion. In the relationship between experimenter and pigeon, the coercion is so complete and so commonplace that the experimenter doesn't even notice it. He or she just picks up the pigeon and puts it in the apparatus. If the experimenter decrees that the pigeon shall have no food, the pigeon does not eat. It is helpless. The experimenter is too big, too strong, and too smart for it. This aspect of behavioral experiments with animals isn't even discussed except by animal-rights activists.

It seems ludicrous to me to claim some subtle and indirect form of control of behavior of an organism, when it depends on already having absolute control of the organism through direct brute force.

The notion of reinforcement or reward is the product of a figure-ground switch. Behaviorists who speak of reinforcing behavior are looking only at the sequence of events that begins with administering the reward, and ends with a change in behavior. They do not notice that giving a reward also implies not giving it, and that not giving it is meaningless unless they are the sole provider of the reward, and can physically (or by threat) prevent the organism from finding its own source of reward during the non-giving period. They also generally overlook the fact that the organism must WANT the reward (behaviorists usually do not admit that "wanting" can have any effect on behavior). They do not notice that their giving of the reward depends on the animal's behavior: they see themselves as controlling through deliberate giving or not giving, just as if they could administer the reward freely at any time they chose, which is not at all true.

The whole behavioristic orientation, it seems to me, is based on the idea that the outputs of the organism are caused by the inputs to it; therefore they see rewards as a causative force, and themselves as manipulators of that cause. They uniformly treat rewards as independent variables under control of the experimenter, overlooking the fact that once the contingency has been set up (by apparatus or by the experimenter's adoption of a fixed rule for administering rewards contingent on behavior) the reward has become a dependent variable, with the behavior the independent variable.

In behaviorial experiments with reinforcers, there is customarily an extreme degree of withholding of the reward. For example, animals are reduced to 80 percent of their free-feeding body weight (calculate what you would weigh) before food is used as a reinforcer. This is certain to turn on the reorganizing system. So the animals, which clearly have an intrinsic reference level for the nutritional effects of food and possibly for the taste of food itself, start reorganizing their own behavior, casting about for a new pattern of control that will result in the acquisition of food. It's very instructive, to a control theorist if not a behaviorist, to see how even wildly improbable motor behaviors such as walking in a path that reminds an experimenter of the shape of a number are rather quickly found if they prove instrumental in providing a seriously-wanted input. Although reorganization uses a very primitive form of control, in which the output is basically a random variation, it turns out to be a very powerful means of acquiring specific and efficient control systems in an environment with unknown properties.

Seeing how a pigeon can learn to circle alternately to the right and to the left in order to make a piece of grain magically appear, we can begin to understand how organisms manage to acquire very complex control systems that take advantage of properties of the physical world, without ever having any theories or any understanding of those properties. We can also see how even human beings could learn how to control the behavior of other people and of animals while under a complete misapprehension of what is actually going on. Oh, sorry -- I should have said a complete misappre(he or she)nsion.

Tom Bourbon (920109) --

You are so right: radical behaviorism is not dead. The language has changed, but not the model (which isn't even recognized as a model).

Best to all

Bill P.

Date: Fri Jan 10, 1992 4:46 pm PST
Subject: Re: Behaviorism

[From Rick Marken (920110)]

Tom Bourbon [920110 -- 2:22] Thanks for an EXCELLENT post on behaviorism and, more importantly, what might be called the "general linear model" of behavior in psychology. It was especially impressive considering the hour when you wrote it - 2:22(am?). Anyway, here is a hearty reinforcement; I hope it increases your rate of posting (it will if you are controlling for praise from me).

This quote from Epstein deserves repeating:

"A large number of factors converge on an always active nervous system to produce behavior. As thresholds are passed and firing rates increase , circuits controlling the occurrence of many different behaviors are activated."

Could there be a clearer statement of the opposite of the PCT point of view. Does anyone on the net imagine that ANY psychologist (other then the PCT variety) would disagree with this statement. I imagine that most psychologists consider the statement above to be a tautology -- or a basic assumption of the discipline. How else could behavior work?

Another part of your post that bears repeating:

>   "Modern" behaviorists assert that no one believes in
>S-R causality, but in itsplace they substitute Skinner's old
>phrase in which "features of the environment" are said to
>"occasion," or to "set the occasion for," something called
>"a behavior." Polite euphemisms, designed to deflect the charge
>that reflexological concepts underlie all of behaviorism.

I heartily agree. Since psychologists don't deal with working models, they can easily dismiss PCT as "nothing but ..." with verbal magic. It's tough to show that you have a new point of view when the old point of view is verbal smoke and missors.

>   the same mistaken
>ideas about behavior flourish under different names throughout
>the behavioral and life sciences. Submit a manuscript in which
>you describe control-theoretic modeling of behavior and you
>will see what I mean.

Amen.

Rick

****

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213 474-0313 (evening)

Date: Fri Jan 10, 1992 5:10 pm PST
Subject: Re: Yes

[From: Bruce Nevin (920110 1102)]

(Bill Powers (920104.0900)) --

Re: "Yes" all the counter examples you give involve quoting. When you quote a word associative meanings are not invoked, and the control systems involved in understanding a sentence containing that word are not invoked. Quoting a word refers to it as a word-perception. So I stand by my statement

>The _linguistic_meaning_ of the word "yes" is always bound up in its
>role in the second half of a question-answer pair. Linguistically, when
>you hear "yes," you must remember or imagine a yes-no question, and with
>yes you must be able to include the affirmative member of the yes-no
>disjunction given in the question.

In the translation example, to the extent that the linguistic meaning is involved, it is involved in both languages. "Si" might also mean "if" in Italian as it does in French, so understanding the question in Italian involves the reduction, and the finding of the corresponding reduction in English, even though a different reduction is involved in quoting:

        "And how do you say the word `yes' in Italian?" -->
        "And how do you say `yes' in Italian?"

If we both know that I am asking about Italian, this might reduce to:

        "And how do you say `yes'? -->

If I am asking a series of such questions, it might reduce to:

        "And `yes'? -->

Or even:

        `Yes'?

The thing about the reduction system is that all the zeroed words are *present*, in zero form, just as the plural morpheme is present in zero form in the sentence "I have seventeen sheep." The claim is that they are PRESENT IN THE UTTERANCE. The demonstration has been carried out, in detail, that their presence can be determined by a machine that knows nothing of the associative meanings of words, only the dependencies of word classes. Such a machine recognizes more degeneracy than we would (more ambiguity) in the reduced form of an isolated sentence. It eliminates most of the alternative readings on the basis of relations to other sentences in concurrent discourse. It could eliminate still more (this has not been implemented, to my knowledge, but Stephen Johnson, Lynnette Hershman, Ralph Grishman, Naomi Sager, and others are working on it) on the basis of relations to remembered discourse on the same subject matter (mostly determined by having same N-class words, same operators on N). I believe we do the same, with respect to remembered and imagined discourse. We use the associative meanings to ignore most of the few that remain, in most cases all but one.

More when I get time.

In haste,

Bruce Nevin
bn@bbn.com

Date: Fri Jan 10, 1992 5:17 pm PST
Subject: language learning behaviorism

[Joel Judd]

Rick, Tom, Avery, Martin, Gary (you'll get it twice) other linguists:

Lest there be any doubt that researchers into language teaching aren't getting the message from psychologists on learning and teaching, here's an example each from two recent books on language classrooms:
****
Chaudron, C. (1988)._Second language classrooms_. NY: Cambridge.

The following diagram (simplified for lack of a good GRAPHICS package--just kidding), taken from a 1974 text on teaching, forms the model for classroom teaching the book assumes:

```
PRESAGE VARIABLES\
                                    -> PROCESS VARIABLES   --> PRODUCT
VARIABLES
CONTEXT VARIABLES/       (CLASSROOM)
```
****
Richards, J. (1990). _The language teaching matrix_. NY: Cambridge.

In a discussion of "self-monitoring," one of the recent fads in teacher development, the following is given as definition and justification: "Self-monitoring or self-observation refers to a systematic approach to the observation, evaluation, and management of one's own behavior (Armstrong & Frith, 1984), for the purposes of achieving a better undertanding [of] and *control over one's behavior*." (p.118, emphasis and grammatical correction added)
****

The problem is that such ideas are mixed in with some genuinely useful ones. But without a better theoretical basis for reading such stuff, I can't imagine how teacher trainers, much less teachers, would come to better understanding of learning.

Date: Fri Jan 10, 1992 8:45 pm PST
Subject: Re: Behaviorism

[Martin Taylor 920110 20:00]

(Rick Marken 920110)

Rick approves of a posting by Tom Bourbon that I haven't seen because it bounced during a reorganization of our file system. But he says one thing that deserves comment:

>
>This quote from Epstein deserves repeating:
>
> "A large number of factors converge on an always active nervous
>system to produce behavior. As thresholds are passed and firing
>rates increase , circuits controlling the occurrence of many
>different behaviors are activated."
>
>Could there be a clearer statement of the opposite of the PCT point
>of view. Does anyone on the net imagine that ANY psychologist (other
>then the PCT variety) would disagree with this statement. I imagine that
>most psychologists consider the statement above to be a tautology -- or
>a basic assumption of the discipline. How else could behavior work?
>

In a sense, the Epstein quote DOES have to be true. It is not false. It is incomplete, and the essence of the problem is in the last sentence I quoted from Rick "How else could behaviour work?"

It seems to me that the argument is at a different level, rather like complaining about a description of a clockwork mechanism something like: "The spring is coiled tight and as it releases its energy, it forces gears to go round, some of which are connected to pointers on the clock's outer surface, and others of which are connected momentarily to an oscillating mechanism that ensures the gears turn at a constant speed." Such a description wouldn't be wrong. It would simply miss the point about what a clock IS. Similarly, the Epstein quote simply does not talk about behaviour at the level that is of interest to PCT. It says nothing about why these thresholds are passed, or of how they relate to things presumed to exist in the outer world...

Sure, behaviour could work in other ways, but the behaving organism would have to be made of other materials. In this group, we are talking about an emergent property of the events "As thresholds are passed and firing
>rates increase , circuits controlling the occurrence of many
>different behaviors are activated." It's just the same as saying
that understanding the interactions of quarks is a wrong description of why aspirin makes pain go away. It's not wrong -- just irrelevant.

Martin

Date: Fri Jan 10, 1992 8:52 pm PST
Subject: Re: language learning behaviorism

[Martin Taylor 920110]
(Joel Judd 920110)
>
> But without a better theoretical basis for reading such stuff, I
>can't imagine how teacher trainers, much less teachers, would come to
>better understanding of learning.
>
In the year I have been reading this list, I haven't come across anything that would qualify as an understanding of learning. The term "reorganization" is used as a surrogate; sometimes reorganization seems to be happening very fast (e-coli, if I understand the use of the term), and sometimes slowly and on a grand scale. Sometimes it seems to occur only in order to resolve conflict, and sometimes to facilitate the correction of error somewhere in the hierarchy.

Would it be possible to have a discussion of what it means to "learn" within PCT? If so, people like Joel would be in a much better position

I think part of my problem, to start a possible debate, is that reorganization seems to me to be largely a business of inverting the signs of the output from control systems, which should result in significant and abrupt changes in the reactions of all the related parts of the hierarchy, whereas much learning appears to be either one-shot (one exposure and you have "got it") or incremental (you get better gradually). Sometimes both see to appear, as in learning that has plateaus (Bill mentioned problems with this kind of thing soon after I started reading CSG-L). These are external appearances, and might well be misleading as to what is actually going on. But what, in PCT theory, really does go on in learning? Is it the construction of new elementary control systems? If so, where do the new systems get their references from, and why? Is it changes in gain of existing control systems? Is it in the random reassignment of reference signs? All or none of the above?

Martin
to help teachers to "come to a better understanding of learning," and speaking personally, I would find myself much more comfortable with PCT as a "theory of everything" in psychology.

Date: Fri Jan 10, 1992 8:55 pm PST
Subject: Re: Behaviorism etc.,

[Martin Taylor 910110 20:20]
(Bill Powers 920110.1000)

I think you said what I said, but in more words and with more clarity. My only point on which I quibbled with your earlier "pigeon posting" was on the dichotomy of "rewarding" and "withholding reward". Your response restates what I tried to say seemed to be going on, but you still assert the dichotomy where I see a continuum. That's fine. All I wanted to say is that the pigeon had to be provided a way of reducing its "hunger" reference signal that was differentiabl from other behaviours. You seem to think you are contradicting me by saying that it won't learn if that condition is violated. You aren't.

Martin

Date: Fri Jan 10, 1992 8:58 pm PST
Subject: Re: subjective information

[Martin Taylor 920110 20:30]
(Bruce Nevin 920110 0734)

Hey, I thought you were going to listen silently for a while!

But I'm glad you don't. You make me think hard about things I thought I understood clearly, and that can only be a good thing, even if "Moy broin herts."

Your question about subjective probability, likelihood, and the error signal comes right out of left field for me, because I could never have thought of confusing likelihood with an error signal. So now I have to think about why you make this identification, or why you think I do, and then try to compose a response that will satisfy both you and me. At present, I am at a loss, and must try to read through your posting to gather more clues--probably sleep on it. My view of the world is far from yours in some respects, even though it often seems close in other respects. Makes life interesting.

The weekend approaches, and I am going home. Maybe you will get an answer filled with unwanted } signs instead of letters.

Martin

Date: Fri Jan 10, 1992 9:33 pm PST
Subject: CSG business from Mary

[from Mary Powers]

Thanks for the concern and good wishes from numerous people. I feel fine and am up and around, thanks to a daily dose of rat poison (really - Warfarin) to keep my blood thin for a few months.

Saw an interesting German on local TV last night --) absolutely accent-free but his syntax was dreadful. Not a common arrangement, I should think.

I want to remind CSG members on the net who last paid dues at the 1990 conference or at any time up to July 1991 - YOUR MEMBERSHIP HAS EXPIRED. I hope this is inadvertent and not deliberate. We need support to continue to produce the newsletter and Closed

Loop, and to function generally. Those on the net who are not members, please consider joining. $40.00 regular, $5.00 for students.

Mary

Date: Sat Jan 11, 1992 5:36 am PST
Subject: HI MARY

MARY, IT IS GOOD TO HEAR YOU ARE UP AND ABOUT!
WARM REGARDS, WAYNE

Date: Sat Jan 11, 1992 10:41 am PST
Subject: Behaviorism

[From Bill Powers (920110.2100)]

Martin Taylor (920110) --

In replying to Rick and Tom you say

>It seems to me that the argument is at a different level, rather like
>complaining about a description of a clockwork mechanism something like:
>"The spring is coiled tight and as it releases its energy, it forces
>gears to go round, some of which are connected to pointers on the
>clock's outer surface, and others of which are connected momentarily to
>an oscillating mechanism that ensures the gears turn at a constant
>speed." Such a description wouldn't be wrong. It would simply miss the
>point about what a clock IS.

I believe you're saying that while the behaviorists have correctly described the mechanics of behavior, they have missed the point about its higher levels of organization. This is not true. They have NOT correctly described the mechanics of behavior -- they have made and compounded a serious mistake.

The assumption that behaviorists (and many, many others) have made is that once you have explained behavior to the level of motor command signals and muscle tensions, the rest of the process is "just physics." In other words, the muscle tensions create the pushes, pulls, twists, and squeezes that move the limbs that create the external effects that we recognize as patterns of behavior. Cognitive psychologists, stimulus-response psychologists, behaviorists and radical behaviorists, personality psychologists, social psychologists, biologists, medical researchers, artificial-intelligence researchers, modelers of motor control, and all the rest base their reasoning completely on the integrity of this causal chain.

But this is not how the causal chain works, even at the lowest level of description. The way it works is this:

Neural command signals enter muscles where they cause a degree of contraction that depends on immediately prior use of the muscle, the history of exercise, the state of nourishment of the body and of blood circulation, and the current angular velocity and load at the affected joint. The resulting contraction affects the angular acceleration of the limb, which is also affected by varying external forces and by gravity. The net acceleration is integrated to produce angular velocity, and the angular velocity plus variable frictional and viscous forces both internal and external to the body are integrated to produce position, subject to variable external constraints and limits.

Limb forces, velocities, and positions act through variable physical links on objects in the external world and the body's relationship to them, at the same time that other independent and variable influences act on the same objects and relationships. The confluence of all these effects finally creates patterns of posture, movement, and relationship which we recognize as behavior patterns.

We can recognize these behavior patterns because they are regular and repeatable across all kinds of variations in physical and physiological conditions. Clearly, considering all the sources of variation and disturbance that intervene between neural command signals and the repeatable results that we label behavior, the regularities that we see resulting from neural command signals are impossible to explain by any cause-effect reasoning. The conditions required for us to predict the outcome from knowledge of the neural command signals, even total detailed knowledge, are simply not present. In fact, cause-effect reasoning, together with acknowledgement of the many independent and unpredictable influences that enter the causal chain after the command signals have occured, would force us to conclude that there can be no regular relationship of behavior to neural command signals. And in fact there is none. There is, in general, no important correlation between "behavior" and the outputs of the nervous system. Only under very special and unnatural conditions do even moderate correlations appear -- conditions deliberately created to minimize interference (by external disturbances and differences in body condition) with the behavior patterns being studied.

What we observe, in fact, is that the neural command signals vary in precisely the way required to cancel the summed effects of all the myriad disturbances and changes in parameters that occur between the neural signals and the final pattern of behavior. Out at the end of this unreliable and variable causal chain, we find regular patterns of behavior, precise adjustments of positions and relationships that are nearly unaffected by the intervening variations.

This is a fundamental fact of nature that, while known since William James called our attention to it almost a century ago, has been ignored by all branches of science concerned with explaining behavior. In place of these facts, there has been substituted an imaginary causal chain that allows us to reason both forward and backward between neural command signals and the final behavioral results. Every major theory of behavior assumes or tacitly relies upon a regular connection between the outputs of the nervous system and the behavior patterns we see.

Therefore all the sciences of brain and behavior have built their intellectual edifices on a falsity. All of our machines that purport to imitate or take over the behavior of human beings are designed to operate with a reliable output chain of causes and effects in an environment kept free of disturbances, and using components that can repeat their behavior with complete precision given only the command to do so. All of them, that is, but one: the control system.

Only the control system can reproduce the fundamental phenomenon of behavior: the repeated attainment of consistent ends by variable means. That, and that alone, is what is important about control theory. Whether we have a hierarchy of control, how that hierarchy is internally arranged and organized, how that hierarchy develops, what its specific properties are, how it relates to language or personality or learning or pointing, are all completely secondary issues. All such hypotheses are simply attempts to begin anew, exploring behavior from its very foundations upward on the basis of a fundamentally new understanding of what it is and how it works.

Only when one is willing to start anew can the real meaning of control theory be appreciated. There is no way to reconcile control theory with any previous theory of behavior (unless it inadvertently conformed to the principles of control). Everything that has been accepted as known about human or animal behavior is now questionable, because we know exactly what false premise underlies all that knowledge. No existing data about behavior can be taken at face value; no interpretation of it can be accepted any longer. Nothing said about behavior prior to control theory, in fact, can be taken seriously without a complete re-examination. Every science of life, every subdiscipline, has to be dismantled to its foundations and rebuilt -- there is no other way to weed out the universal and pernicious influence of a fundamentally incorrect assumption that has been woven into every observation and deduction since the very beginning.

Best

Bill P.

Date: Sat Jan 11, 1992 3:44 pm PST
Subject: Invoice for Demo Disks

INVOICE

Invoice number:_______________ (use YYMMDD)

Date:_________________________

Remit to: William T. Powers
73 Ridge Place, CR 510
Durango, CO 81301

To: _________________________ (purchaser)

____

____

____

Conditions: This invoice covers the purchase of any of three programs, named
Demo1, Demo1, and Arm, which are in your possession on approval.
Payment for use of these programs by individuals for their own enlightenment is

optional. For institutional or professional use, the charges (taxes included) are as follows:

Demo1: The phenomenon of control
    $35 per class-semester (or course) or other professional use
    $150 for unlimited use in teaching by a single department

Demo2: The theory of control
    $60 per class-semester (or course) or other professional use
    $150 for unlimited use in teaching by a single department.

Arm: A control-system model of pointing behavior, version 1
    $35 per class-semester (or course) or other professional use
    $150 for unlimited use in teaching by a single department.

Copies of these programs may be made freely for student use or for any other noncommercial and nonprofessional use. The programs must be distributed as received with no changes, and may not be sold.

Program	Unit price	# courses	Total remitted
Demo1	$35	______	__________
	$150	(unlimited use)	__________
Demo2	$60	______	__________
	$150	(unlimited use)	__________
Arm	$35	______	__________
	$150	(unlimited use)	__________
		Grand total	__________

Thank you for your order.

Date: Sat Jan 11, 1992 6:34 pm PST
Subject: Foreign Accents

[from Gary Cziko 920110.22.30]

Mary Powers (920110) notes:

>Saw an interesting German on local TV last night --) absolutely
>accent-free but his syntax was dreadful. Not a common
>arrangement, I should think.

I have the same type of problem in both German and Spanish: virtually native-like accents, but only fair command of productive grammar and vocabulary in German and quite dreadful in Spanish (which makes sense since I have never had to speak Spanish, but have listened to lots of it on the radio, mostly shortwave). I suppose Marvin Brown would like the latter parenthetical remark!--Gary

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210 Education Building   N9MJZ

Champaign, Illinois 61820-6990
USA

Date: Sat Jan 11, 1992 7:57 pm PST
Subject: Worth It After All

[from Gary Cziko 920111.2045]

Sometimes managing this network is a real pain in the you know where. If there are six subscribers whose machines are down and there are 10 posts on a given day (lately there have been often more), that means 60 returned messages to me as "list owner" cluttering up my mailbox. Subscribing new netters, unsubscribing the bewildered, and trying to figure out how to make use of Bill Silvert's fileserver for distributing program files all take time from the things I "should" be doing.

But then Bill Powers (920110.2100) comes up with things like...

>There is, in general, no important correlation between "behavior"
>and the outputs of the nervous system. Only under very special and
>unnatural conditions do even moderate correlations appear -- conditions
>deliberately created to minimize interference (by external disturbances
>and differences in body condition) with the behavior patterns being
>studied.

and

>Nothing said about behavior prior to control theory,
>in fact, can be taken seriously without a complete re-examination. Every
>science of life, every subdiscipline, has to be dismantled to its
>foundations and rebuilt -- there is no other way to weed out the
>universal and pernicious influence of a fundamentally incorrect
>assumption that has been woven into every observation and deduction since
>the very beginning.

...and it all seems well worth it!

Just three years ago this would have all seemed absolute nonsense to me. Today it makes so much sense that it is almost frightening. And the major reason that it makes so much sense is because Bill has been patient enough to go over it again and again and again, using different words and different examples but with the same revolutionary message.

I hope that the people on this network who find these radical assertions confusing will have as much patience as Bill has shown in their attempts to make sense out of all this. And thanks again, Bill, for reminding me what a revolutionary insight you have shared with us.--Gary

----
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Date: Sun Jan 12, 1992 8:26 am PST
Subject: Pretty Pictures on Prison Walls

Ken Hacker [910112]

Response to Tom Bourbon who argues a) that cybernetic models as closed systems is an irrelevant issue since PCT accounts for continuous interactions with surroundings, and b) each individual who might be controlled by someone has the ability to "come to that decision..." and enacts what he/she intends as experience.

+ I can see why you argue that PCT is not closed system orientd or at least why it appears at one level not to be. However, as I have observed the discussion on this hotline, it appears to me that social interactions are not, in fact, part of the PCT equation.

+ PCT defensibly argues that each person has unique perceptions and makes individual choices leading to behaviors.

- PCT indefensibly assumes or insinuates that each person has full autonomy over the decision-making process, when in fact, each person has limited ranges of regulation WITHIN which he or she may freely choose.

+ It is true that each person makes decisions. It is also true that humans are teleogical and can form intentions to act.

- There is some confusion here between the ability to make choices and form behavioral intentions and CONTROL over one's life. We can control SOME levels of our actions, but others are SET and REGULATED by others.

If I am the jailer and you are the prisoner, and I throw you in the cell, how free to perceive and act are you? Yes, you are fully free to perceive anything that you wish. You can transcend the walls and have flights of fancy to anywhere you can imagine. Now, how much control do you have over walking to the store, going swimming, or seeing a movie. Answer: none. Who does? I do. I can let you out and send you to the store, swimming, or the movies. Now what about less physical aspects. Can I control your thoughts? NO, again, perception is yours. Can I control the information you process? Yes, I can give you false facts, phony statistics, etc. Will you have full control over your cognitions? Again, yes, but will you have control of the sort that I have not manipulated you. NO, I have used your free choice to bind your decisions. When we assume that PCT or any other explanation of human behavior gives autonomy to humans, who are social creatures phylogenetically and ontogenetically, we are giving the prisoner a paint brush and telling him to paint nice pictures on the walls. Is he free? Sure, in his mind he is free. Is he free in his environment. NO, there are walls which exist as boundary conditions to his free agency.

I like the way that Sartre contrasts psychological freedom from real or material freedom. The former is idealism; the human spirit can do anything and the limits on a human's life, career, etc. are only the results of his or her own doing. This is also a fundamental premise of political conservatism. Society has no responsibility to help individuals do what they should be doing themselves. The material freedom Sartre refers to is the kind of freedom a human has when he or she can truly enact the surroundings and change them.

PCT has great promise, in my view, but only if it accepts the fact that social interaction is part of human BEING, and that perception is personal but that cognition and behavioral intentions are shaped and constrained by by perception-society interconnections. In essence, I argue that the current discussion contains an error which I always that cybernetics avoided (the artificial slicing of internal and external). So rather than this:

```
            perceptions ------->  actions, adjustments,
```

I argue that we should assume this:

```
    actions, adjustments < ----- > perceptions < ------ > actions, adjustments.
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Finally, the challenges being made here and the responses by Bill are a great stimulus to myself and maybe others who have joined the conversations late in the game. I have my own perceptions of what PCT and what human behavior are about, but I am changing my representations of what this all leads to in a gradual and still somewhat unpredictable way. I therefore reserve the right to change my mind about anything at any time!

Cordially, Ken Hacker

Dept. of Communication Studies
New Mexico State University
Las Cruces, NM 88003

Date: Sun Jan 12, 1992 10:58 am PST
Subject: Re: Behaviorism

[Martin Taylor 920111 13:45]
(Bill Powers 920110 21:00)

I still think that the Epstein quote with which Rick took issue is compatible with all you say. I made no reference to higher levels. And even you recognize that the firing of neurons does result eventually in the contraction of muscles, as the Epstein quote says. The point is that, as you always insist, no regularity of the neuron firing corresponds to a regularity of the resulting behaviour, and still less does it correspond to a regularity of the perceptual effects of that behaviour. So, I maintain what I said, that the problem is a matter of the level of abstraction with which people are dealing. When I want to talk about how one gear pushes another, it is unhelpful for you to tell me the clock is running slow, and if you want to tell the time, it is no use for me to tell you that the balance spring tension needs to be changed.

When someone proposes an open-loop description of purposeful behaviour, then there is legitimate cause for complaint. I did as much in my mini-review of Penrose's "Emperor's New Mind" in BBS last year (not from a CSG viewpoint, but the principle was the same, that open-loop algorithms cannot describe behaviour in a real world with unpredictable events that impinge on the behaver). I think CSG people weaken their case when they dispute true statements because the statements carry implications that are false if the reader has a particular mindset.

(Bruce Nevin)
I have prepared a discussion on subjective probability, perceptual likelihood, and error signals, but I can't upload it because our system gurus have done something that causes the upload program to hang whenever I try. I'll try to remember to bring it on disk on Monday, and send it Monday evening.

Martin

Date: Sun Jan 12, 1992 2:08 pm PST
Subject: Hierarchies are not free from social influence

Ken Hacker [920112]

Response to Bill Powers who argues that a) Socially derived goal states can be chosen by each person to achieve a private purpose, and b) "social control can never go further than presenting one with a situation."

+ Goals states are going to have social influence because we are social creatures and as myriads of data indicate, we make are influence by people around us, culture, language, rules, and norms as we make individual choices. I do not disagree at all with the notion of private purpose.

- Private purposes should not be confused with individuated decisions which are idealistically separable from social influence.

+ None us is fully controlled or fully controllable from either internal or external vectors of stimuli, rules, and models.

+ Yes, we are all free to yell fire in the crowded theater. But when you say that we do not, you argue that the decision is your own and not that of society. To some extent, you are right in that we all make the decisions that affect us, even the mindless and self-destructive ones.

- You are not the one who wrote the laws on free speech. Society did and society will punish you if you make the wrong decision. I argue that constraints and encouragement from society are important factors in how we make decisions. Of course we choose to go to jail, but what are we likely to do instead? I think that we consider consequences and add them to our speculations about goals and adjusting our behaviors to accomplish those goals.

+ Only a situation? Situation is critical in my view. "Inner intentions" have two sources: 1) personal thinking and reacting, and 2) social interactions, modeling, learning.

I believe that control from the social circumstances of a person is never a simple matter of total constraint. I hope I never argued it was; I don't think I did. There are measures of control which social interactions and situations can impose on the cybernetically adaptive human being. I believe there are levels of control beginning with influence over perceptions and moving up to persuasion and physical constraints. Still, I concur with the basic assumption that FULL control is not possible. Certainly, Eastern European revolutions and revolts by USSR nations are examples of this.

The type of control which I am describing as the most potent possible and the kind which is ignored, is that which uses freedom as its prime mechanism of contraint. In other words, a person is allowed to choose and to think, but is given false inputs to think with and is given no ability to choose parameters of decisions. Thus, each person feels free and does not resist the level of control because it never identifies itself. Joseph Goebbels was a master of this as are contemporary political communication consultants. Again, the control is never total, but it is effective. For evidence, review the history of civilization and political theory back to 4,000 B.C. Leaders set parameters and keep dissent minimized by keeping choices alive within systems of consensual limitations.

Cybernetically,

Ken Hacker

Date: Sun Jan 12, 1992 2:40 pm PST
Subject: CT on PBS

from Ed Ford (920112.15.40)

For the past two weeks, I have spent all my time in a highly intense effort to produce a one hour (actually 45 minutes, in two segments of 22 1/2 minutes each) program for the local PBS station which they plan to use locally on a regular basis over the next five or more years. It is based on the ideas from my book, Love Guaranteed. The first half is exclusively an explanation, as simply and clearly as I could give, on control theory. This project was initiated two years ago by the station's program manager (who was made executive producer of the project by the station manager) who took a course from me, and it was funded by three corporations. The taping (TV jargon) was done on Saturday, in front of three cameras, 25 technical support people along with lots of cables, lights, plus a make-up person, clothes purchased for the occasion (which I got to keep), a set built for the occasion, and a crowd of 150 people. I used Bill as consultant during the upgrading of the script (I used cue cards to make sure I said what I wanted to say). The credits should mention Bill, his books, and our control systems group. It will air twice in March during the station's regular (4 times a year) campaign for funds. My books will be used as part of the promotion. If it goes well in Phoenix, it could be picked up by other PBS stations throughout the country. I'm not only thrilled my own ideas on relationship building will have the potential for a wider hearing, but also the exposure control theory could be given on a national basis.

I am meeting with Kathy Kolbe tomorrow morning at her office.

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

Date: Sun Jan 12, 1992 6:31 pm PST
Subject: CT on PBS

Congratulations and well done! This is indeed a great opportunity that I know you have nurtured for some time. You have put a lot into it. Knowing what you have done before, I am sure it will be far superior to the regular fare and a most visible boost to PCT.

I can hardly wait to purchase a copy of the video tape from you so I can enjoy it, learn from it and promote it.

You will deserve to sell a lot of books, too.

This is exciting!

oops: This was [from Dag Forssell 920112]
Ed Ford (920112)

Date: Sun Jan 12, 1992 7:03 pm PST
Subject: Social control; behaviorism

[From Bill Powers (920112.1700)]

Ken Hacker (920112) --

>If I am the jailer and you are the prisoner, and I throw you in the
>cell, how free to perceive and act are you?

Completely. I think the point that is dancing just beyond reach here is that NO behavior is externally guided. Behind the idea of social control is that same old idea that stimuli acting on the nervous system cause behavior, which is based on a premise about how behavior works that is easily shown to be false. It makes no difference whether you talk about mechanical stimuli or about "social situations;" in any case, these external things act on you only through your sensory inputs (unless physical force is used).

Let's take an extreme case: suppose someone has a gun pressed to your head and tells you "Give me all your money or I'll shoot you and take it." Now, what effect does this have on your behavior? If you think this through carefully, using the principles of CT and SPECIFICALLY NOT the principles of SR psychology, the answer is easy: none. All that has happened is that you are seeing a physical situation, feeling something against your head, and hearing words. None of these perceptions has the slightest ability to do anything to make your muscles even twitch: you are simply not internally connected in that way.

Before any of these perceptions can lead to behavior, they must first be recognized at the right levels -- as a gun, as a threatening sentence, as a situation carrying a threat to your life and your money. But still this does not imply any action. Action is not driven by perception, but by the difference between what you are perceiving and what you want to be perceiving. So before there can be any action, there must be a comparison of the perceptions with your preferred states of these perceptions. This means that you must already have reference levels relating to these perceptions: there must be variables you are controlling that are disturbed by deviations of perceptions from their specified states. Only when such deviations are detected and converted into changed reference signals for lower-order systems will any attempt to act take place.

It is likely that being held up will lead to an immediate conflict, because although you don't want to be shot, you don't want to give up your money, either. This conflict can be resolved only at a higher level. Something has to run through possible actions in imagination and weigh their outcomes, and turn off one side of the conflict. Some people will simply freeze, not being able to go up a level or not being able to reset the goals quickly enough, and be shot. Others will decide to take a chance with their lives and try to disarm the assailant. Of those, some will be shot (perhaps most). And the rest will choose to give up their money, as much as it hurts, in order to preserve their lives, losing which would hurt more (or so they imagine). Some of those, also, are shot after they have handed over their money. They are, after all, witnesses.

If one can think fast enough and judge the assailant fast enough, the correct solution may sometimes be to fight back because the assailant (not being masked, for example) is more likely than not to shoot you even if you comply. Fighting back need not be physical: it might occur to you to say "Son, are you sure you want to rob Tony Scalesi's brother?" or "Don't let my blood get on you; I have AIDS." Under the circumstances, clever lies are permissible.

My point is that nothing that happens to you causes you to do anything. Your perceptual interpretations at many levels, and your goals for each perception, particularly at the highest levels, determine the meaning of every experience and the action you will take relative to any experience. That is simply how any organism works. The outside world can affect you physically and it can present you with the raw materials for perceptual interpretations. And that is all it can do to you.

It's easy to think of simple examples, but not so easy to think of them in realistic contexts. People who are thrown into jail are not suddenly confronted with a jailor and popped into a cell. They are apprehended, accused, put through a system. If they choose they can give up, or they can fight at every step, right up until the door clangs shut. And if they consider being incarcerated the end of everything, they can quickly contrive to removed themselves from the jailor's control by ending their lives. Most of them consider the situation and decide that there is always a chance for escape, or recognize that their sentence is finite and wait it out (when that's true). In effect, most of them reorder their lower-level goals and consider themselves free, although some goals are temporarily on hold. Very few prisoners surrender their autonomy; they put their time to good use, learning from experts how to avoid being caught in the future (and, apparently, not giving much thought to where the experts are from whom they are learning this).

My point is that what we choose to experience is determined from within us. The highest levels of goals are selected experimentally, on the basis of how they affect our general well-being (the concern of the reorganizing system which considers only internal criteria).

* * * * *

My second point is in reponse to your second post, where you say

> + Goal states are going to have social influence because we are
> social creatures and as myriads of data indicate, we make are

>   influenced by people around us, culture, language, rules, and norms
>   as we make individual choices.

Of course we are "influenced," and that is the correct word. To influence is neither to determine nor to control. When I walk through a door, my motor behavior is influenced by the direction in which the door opens. It's not controlled or determined by that physical fact -- I could break the door down if it refuses to yield to a push, or take it off its hinges, or look for a door that opens the other way, or decide to have lunch instead. The direction in which the door opens is just a fact; it can't tell me how to behave with respect to the door. If I do select a goal that entails opening the door and stick with it, then the way in which the door opens is immaterial because I will find the action that opens it, and proceed to accomplish the goal that required me to be on the other side of it. The door can influence me only at the levels where its properties are relevant.

Each of us lives inside one person surrounded by everyone else. Our goals, insofar as they have anything to do with society, are chosen from our perceptions of what social happenings are possible, knowledge which we accumulate through observing and interacting with other people. We build up models in our heads about the properties of other people, how we can expect them to behave when we act in relationship to them. Once we have such models (and each of us has different ones), we can then use them in pursuit of higher-level goals, such as being thought a good person, finding someone to love, avoiding being thrown in jail, or studying chemistry. Each of us uses the perceived properties of society (established by experimental test) as a means of creating the perceptions we want to experience. We are often quite mistaken about how others will react.

For many people, the most important highest-level experiences have nothing to do with other people. The means of achieving them almost always have something to do with other people.

Of course in seeking high-level goals in a society, we are subject to exactly the same kinds of constraints we would encounter if we wanted to accomplish something that entailed using a screwdriver. We must learn to perceive the environment rather than just imagine how we would wish it to be. We must select as goals perceptions of things that actually can occur; it isn't possible to drive wood screws by turning the screwdriver counterclockwise, even if we find that motion easier than a clockwise one. We may want to behave differently from a crude stupid hostile carpenter whom we see driving screws, but we will end up using the screwdriver just the same way he does if we insist on driving screws. It's the way that works.

In many cases people don't bother to be creative: they simply watch what other people do that gets them jobs, money, friends, mates, or laboratories and carry out the same movements or adopt what seem to them the same attitudes. Of course this doesn't work very well, because reproducing outputs is not the same as reproducing outcomes. But most people don't live very satisfying lives anyway; they think little about higher levels of human nature, and generally they take appearances at face value (the same mistake that led to behaviorism, but not that well organized). They are used to frustration, conflict, and failure and hope for little more than to keep internal error at a tolerable level. Very few people have any conscious participation even in their own principle levels: they learn by rote what is right and wrong, but see nothing systematic in moral admonishments. They learn the difference between right and wrong, but not what the difference is.

Social scientists study people as they appear to be. They do not use any concept of levels of organization within individuals, so they are not concerned with distinguishing between means and ends (and certainly not with hierarchies of means and ends). They tend to assume that "normal" is equivalent to "healthy." It is not the custom in social psychology to see social behavior as emergent from properties of the individuals who compose the society. If anything, the bias is in the other direction: to try to explain individual behavior on the basis of mass measures (with no consideration of why the mass measures come out as they do).

Control theory justifies considering the individual the measure of all things, even societies. It is not that individuals behave as they do because societies exhibit the mass properties they do. It is just the other way around: a society is as it is because individuals are as they are. Of course the individuals influence each other; the environment of each individual contains all the others, or at least all the others with whom an individual comes into contact. At some level, individuals behave in certain ways because others respond to their behavior in certain ways. But at a higher level of organization, the "because" is conditioned on what the individual wants to experience: what moral codes, what system concepts both social and private. But there is no such thing as an abstract society that somehow influences ALL the individuals in it from some superior (and etherial) position. There is, in fact, no such thing as "a" society. Each person belongs to many societies, each with different customs and agreed-upon rules, each understood more or less wrongly and more or less in conflict with the others. When you get in your car and drive 100 miles, you end up in a society with different properties, although you are most likely to assume that it has the properties you are used to.

Finally, I'm not impressed by what "myriads of data indicate." Most of the data in the social sciences are, to put it kindly, lousy. If they weren't lousy you wouldn't need myriads of them. Correlations of only 0.6 are considered gifts from heaven, and those as low as 0.2 are still quite publishable. And anyway, data don't indicate anything: SOMEONE'S THEORIES ABOUT the data do the indicating. You will have a hard time finding any social theory that is not at bottom a stimulus-response theory, founded firmly on the mistaken impression that if the environment does the same thing twice to the nervous system, the organism will respond twice in the same way.

I don't consider any correlation of less than 0.95 to be of scientific interest, and for correlations that low, a lot of added work is implied to reduce the span of the error bars. At the very least, raising the standards this much should result in one person being able to keep track of all the important research that's going on. With the lax standards we have now, most of what is published (and cited) as fact is garbage, and will be forgotten immediately except by people who need to cite one set of garbage to support another.

I have decided not to run for president of any of the social science societies.

>You say "I believe there are levels of control beginning with influence
>over perceptions and moving up to persuasion and physical constraints.

If you will change "control" to "influence," I will agree.

Martin Taylor (920112) --

>I made no reference to higher levels. And even you recognize that the
>firing of neurons does result eventually in the contraction of muscles,

>as the Epstein quote says.

The firing of sensory neurons may result in contraction of muscles on one occasion, and the same firings of the same neurons may result in relaxation or no effect on another occasion, with no difference in the organization of the nervous system. And on all these occasions, the observed behavior can be the same. I am sure that Epstein would have to disagree with this.

If we continue to seek a mutually-acceptable generalization, we will end up saying that behavior is accompanied by changes in the firing of neurons, a statement that is both true and trivial. The KIND of relationship that is assumed makes all the difference.

If you weren't referring to higher-level interpretations of the function of a clock, such as its being a timekeeper, what did you mean by saying that Epstein's statement was not false? If all that Epstein meant was that influences from the environment affect the nervous system, and that behavior is produced by neural signals exciting muscles, then he said nothing. I think he meant (and that most behaviorists mean) that there is some regular connection from the incoming influences to the final patterns we refer to as behavior, mediated by the nervous system. That is wrong. It is wrong because the final stage of this process, connecting motor signals to those final patterns, is subject to arbitrary and unpredictable disturbances -- yet the outcomes remain regular. Are you saying that behaviorists no longer assume that in the final analysis, the environment causes behavior? Are you saying that they no longer assume that behaviors are related by regular causal links to the motor outputs of the nervous system? Are you saying that they no longer assume behaviors to be indications of the output activities in the nervous system? If you said "yes" to any of these questions, I would be greatly surprised, to the extent that I would like to see some examples.

Best to all, and congratulations to Ed Ford.

Bill P.

Date: Sun Jan 12, 1992 8:34 pm PST
Subject: Re: Social control; behaviorism

From Ken Hacker [920112]

In response to Bill's hot reply to my post: Bill, I find myself agreeing from an introspective and deductive set of reference levels to much of what you argue. And as a social scientist, I painfully admit that much of what social scientists do is not as good as it should and could be. I especially concur with your criticisms of social science determinism and the avoidance of key issues raised by CT that can lead to better research on human behavior. On the other hand, I find some points of divergence in our views which I would like to still toss around. I will send a set of them Monday. For now, let me just say that I am convinced that total control over anyone is not possible, but that strong influence to the extent that their own means of self-control are nudged in particular directions is indeed possible. Secondly, when you say that our own internal signals and references levels DETERMINE our actions, isn't that a kind of mental s->r? I would like to believe (notice the perceptual set up) that we are totally controlled from neither internal nor external forces or signals, but that the hierarchies of processing which exist within us, somehow regulated by what we decide about differences between the desire and the encountered, have direct linkages to the higher and ethical levels of our being, even if that being is socially influenced and even if the differences between the desired and the encountered are learned in social interactions.

Thank you for some good feedback. I am gaining quite a few interesting lessons from these conversations -- far more than I ever get from any other hotline I subscribe to!   KEN

Date: Mon Jan 13, 1992 2:26 am PST
Subject: reality

Re Rick Marken (Fri Jan 10 07:52:30 1992):

>... The fact that these models often
>must be changed to account for newly discovered perceptual experiences
>suggests why is probably not a great idea to treat any current model as
>"reality".

The lowest-level model changes fast (quarks, strings, ...), but at the higher levels that are relevant to biology, ethology, basic lab technique, etc., things are pretty stable. The `ecological variables' I'm talking about are common-sense matters such as proximity to food and water, distance from predators, etc. If one denies existence to this sort of thing, I doubt that any of science would survive (for one thing, this is the level at which the setup of experimental apparatus is described).

>I think PCT emphsizes the fact that it is perception, not reality
>(or the current model thereof) that is controlled because there is no reason
>to expect that any two organisms will map "reality" into perception in the
>same way.

So what? different aspects of reality are relevant for the survival of different organisms, furthermore they make different compromises w.r.t. the costs and benefits of `accurate' perception (e.g., attaining perceptual variables that correlate well with the ecologically relevant ones).  So frogs can get by with a `small moving dot' detector that picks up nearby flies and distant birds (since the cost of sticking out your tongue at a faroff bird is negligable), while peregrine falcons (if they're the birds I'm thinking of) have to be a bit more discriminating at what they choose to dive-bomb at 200 mph.

>Thus, the test for the controlled variable is an attempt to map
>the perceptual variable(s) controlled by an organism into the perceptual
>variables of the researcher.

I'd say that one is identifying the ecological variable that one can deduce that the organism has a perceptual system that will `pick up', e.g. produce a corresponding perceptual variable. And of course, the ecological variables that we think we can identify are restricted to those for which we can construct perceptual variables for ourselves, which makes things tricky

when we study ourselves (and probably hopeless if we tried to study
beings smarter than ourselves).

>I think you were also saying that there are variables in "reality" -- past
>the organism's perceptions, so to speak -- called "ecological variables" --
>that influence the organism's ability to control and survive. I think these

Sort of. The ecological variables determine *whether* the organism survives-
whether you live or die depends on what's out there + what you
actually do, not what you perceive. Survival depends on `controlling' these
variables in a sense that covers the ordinary language usage of the word
`control', but not the PCT usage, whence my attempted `p-control' versus
`e-control' terminology. It is perhaps a bad idea to call them variables,
but, after all, one is x many meters away from the nearest awake, hungry
leopard, y many km from the nearest drinkable water that one knows
how to get to, etc. Successful cybernetic artifacts likewise have to
e-control *our* perceptual variables in order to stay off the
scrap-heap, although the way they do it is by p-controlling theirs.

This `ecological' viewpoint may be a side issue for people whose
primary concern is to figure out how particular psychological mechanisms
work, but it looms quite large on various other agendas, such as
explaining why these systems are set up the way they are. I don't think that
there is any fundamental conflict with the basic ideas of PCT here, and
neither do I think that anyone in PCT is actually confused about the issues
(Bill Powers certainly isn't, at any rate). But I think the terminological
setup and orientation of much of the discussion would tend to make people
interested in the `ecological viewpoint' think that there was a
conflict.

Avery Andrews

Date: Mon Jan 13, 1992 3:10 am PST
Subject: Baaaaaaah!

From Greg Williams

I still await comments on my "Sheep in Wolves' Clothing" post.

The point was that, from the standpoint of a CT type of analysis, some
social situations involve someone's controlling what someone else wants
to perceive.

The implications are twofold; both, I think, contrary to the common CT
metatheory concerning personal autonomy:

1. CT is an effective tool to aid control of what others want by deception.
2. Contra behaviorism, the inanimate (and animate, up to a point -- when
   deception is possible; mammals?) does not control living control systems,
   but contra CT metatheory, people (and chimps and some other "higher"
   animals?) CAN control what other living control systems want. This
   resolves the paradox in behaviorism: Skinner CAN have what he wants
   controlled by others IN CERTAIN SITUATIONS. It also speaks to why it
   is easier to control what "lower" animals and retarded humans want than
   what your peers want (unless your model of their hierarchy is quite good).

So the bottom line is that I claim a CT analysis supports some of Ken Hacker's claims.

Best wishes,

Greg

Date: Mon Jan 13, 1992 4:09 am PST
Subject: recursion

[From: Bruce Nevin (920111 12:20)]

(Avery Andrews 920108 1644, 920109 1119) --

Avery, you said that the brain does use recursion, pace Bill (920106 1518). You gave examples of a sort commonly claimed to be of recursion in English.

These represent recursion in a phrase-structure grammar of English, or in a grammar that uses phrase-structure rules (rewrite rules). They do not represent recursion in an operator grammar.

In rewrite rules for English you might find the following:

```
  a. S    --> NP VP
  b. NP   --> that S
  c. NP   --> N
  d. VP   --> V NP
```

Because S appears in the output of rule (b) as well as the input of rule (a), and because NP appears in the input of (b) as well as the output of (a) (also in the output of (d), whose input, VP appears with NP in the output of (a)) this set of rules is recursive. It generates an infinite set of strings like:

1. the N V that the N V that . . . that the N V

Phrase-structure grammar uses the mechanism of recursion to account for sentences like (2):

2. The dog hoped that the cat saw that the boy forgot that the fish fell.

These are instances of recursion because a given element (e.g. S) in the output of rules is still available to satisfy the input requirement of the same or other rules.

In operator grammar, the input requirement for the entry of an operator word in the construction of a sentence is the prior presence of other words. (Or we may speak in terms of word classes, words classified by their entry requirement.) Crucially: once a word has satisfied the entry requirement for an operator, it is not available for the input requirement of another operator (for the given construal of the sentence). Thus there can be no recursion in operator grammar.

Operator grammar accounts for (2) as follows (ignoring tense and "the"):

fall is asserted of fish
forget is asserted of boy and fall
see is asserted of cat and forget
hope is asserted of dog and see

Similarly for the examples Avery gave involving relative clauses. In PSG (and in systems that use PSG rewrite rules in their base), relative clauses are output by rules that have on the left-hand or input side symbols that occur on their right-hand or output side, and hence involve recursion as above. But in operator grammar, relative clauses are reductions from a sentence that is the second argument of an Ooo operator (conjunction) of subordinated assertion. In writing, this is represented by semicolon if it follows, or by dashes if it interrupts the first sentence, to which the second is conjoined, and in which the second will become a relative clause. Thus (reverting to Avery's numbers for examples):

1a. The dog chased the cat that nibbled the steak.
<-- The dog chased the cat; said cat nibbled the steak.

a. The person (whom) John gave a book to gave a picture to him.
<-- The person--said person John gave a book to--gave a picture to him.

Which words may be reduced to relative pronouns, wh- plus -ich, -o, -at, etc.? Tellingly, it is precisely those argument words that may appear at the front of a sentence conjoined (under the semicolon/dash operator) immediately after the head word. The head of the relative clause is a repetition of that word in front position, and remains as the word "modified by" the relative clause when the second occurrence is reduced to relative pronoun.

Thus, these examples provide no evidence that the brain uses recursion. However, no version of Generative grammar that I have encountered can reach this conclusion, because of the presumption of PSG rewrite rules in the base.

You said also (920106.1355) that my "foo" recognizer etc.
>seems . . . more like the genesis of a PS grammar than of the kind of
>dependency grammar you prefer.
For a PS grammar, you need to have a sequence recognizer that takes input from a class of other recognizers that are also on the sequence level of the hierarchy, to an indeterminate depth of recursion from the sequence level into itself. That is not at all what I am describing.

Bruce Nevin
bn@bbn.com

Date: Mon Jan 13, 1992 4:38 am PST
Subject: associative meanings as interpretation

[From: Bruce Nevin (920110 1242)]

(Martin Taylor 920104 20:50)

>   The linguistic information, as I understand
>Bruce to mean it, is in the Operator-argument structure and the reductions,
>in other words it is the carrier structure, that contains nothing of the
>content. The meanings inhere in the words chosen to fill that structure,
>the relations among them which are certified by that structure, and their
>relations to the pragmatic situation of the talker and listener. I do not
>see how meaning can in any way be "an interpretation of that linguistic
>information" if the first sentence and the last two sentences are also to
>be believed (as I do).

This paragraph contains several phrases that differ from the way I would write (have written) in ways that suggest to me that you misunderstand me. You say that the meanings inhere in three aspects of an utterance: the words, the relations among them, and their relations to the "situation" of the language users involved.

>The meanings inhere in . . . the relations among [the words
>chosen to fill the operator-argument structure] which are
>certified by that structure

>The linguistic information . . . is the carrier structure, that
>contains nothing of the content.

The relations among the words in a sentence are not something apart from the operator-argument structure of the sentence (which may be superficially obscured but not destroyed by reductions). Those inter-word relations ARE the operator-argument structure, and that structure IS those relations. By attributing meaning to relations among the words in the sentence, you are in fact attributing meaning or content to the operator-argument structure of the sentence. Thus, by your own reckoning it is not true that the operator-argument structure "is the carrier structure, that contains nothing of the content." The operator-argument structure is precisely the relations among the words that pertain to the meaning of the utterance--that is, not all their possible relations but just those that correspond (with some caveats) to the notion of "predication" familiar in philosophy.

It is important to realize that the operator-argument relation is a relation among the words in the utterance. It is not a relation among abstractions that refer to the words. (This is important for more than just the discussion of phrase-structure grammar.)

>The linguistic information . . . is in the Operator-argument
>structure and the reductions

This suggests a misunderstanding in two directions. First, the reductions do not contribute to the intended linguistic information, they merely affect the form in which it is expressed. The reductions change the behavioral outputs, often obscuring but never destroying access to the intended words and word-relations. One must undo the reductions in order to make the operator-argument structure explicit, so that the words having those operator-argument relations all have non-zero forms. The undoing is possible because every reduction leaves in the utterance a trace of its operation. (Harris's use of the term "trace" long antedates that in GB theory, Avery.) The presence of a zero form of a word is betrayed by an irregularity, an apparent violation of the argument requirement of some nearby word or words. Since you have been reading _A Grammar of English on Mathematical Principles_ this will be familiar to you, no doubt.

Secondly, the linguistic information is not only in the operator-argument structure of the particular words, but also in the discourse structure based (loosely speaking) upon word repetition across the sentences of a discourse.) Discourse structure is one important basis for zeroing when one originates an utterance, and conversely, when one is understanding an utterance, for construing contextual sentences that have been zeroed, for example as common knowledge too obvious to be given overt utterance.

>The meanings inhere in the words . . . the relations among them . . .
>and their relations to the pragmatic situation

These words denote three sorts of perceptions and an association:

1. Word-perceptions.

2. Perceptions of relations among the word-perceptions of (1).

3. Perceptions of "the pragmatic situation."

4. A relation of associative memory holding between word-perceptions (1) and situation-perceptions (3).

To this I would add:

5. Association of additional remembered and imagined nonverbal perceptions with the situation-perceptions of (3).

6. Association of additional remembered and imagined nonverbal perceptions with the word-perceptions of (1).

7. Correspondence of word-relations ("under" asserted of the word-pair "dig" and "tower") with relations among nonverbal perceptions.

8. Association of word-perceptions with the perceptions of (5-7).

9. Repetition of (1-8) for the word perceptions of (8).

This obviously sets up an associative reverberation between four poles of perception: nonverbal perceptions of the real-time situation, remembered and imagined nonverbal perceptions, perceptions of real-time discourse (words and their relations), and perceptions of remembered and imagined discourse. (The Buddhists go so far as to say that the reverberation almost immediately overwhelms real-time sensory input, supplanting it with constructions--samskara--out of memory and imagination, but we'll ignore the epistemological questions here.)

You are claiming that meanings "inhere" in (1) word-perceptions and (2) perceptions of word-relations. If you intend this "inherent meaning" of words and word-relations to be something other than associative meanings such as those outlined here, please say explicitly what you do mean.

I assume that the associative meanings of (4) and (6) (augmented by (5), (8), and (9)) are an interpretation of (1) the words in the utterance, and that the correspondence of (7) is an interpretation of the word-relations of (2). Note that these associations and correspondences are not *perceptions* of association and *perceptions* of correspondences, they are immanent in (inferred) structural characteristics of the perceptual hierarchy. As such, they almost certainly differ markedly in detail from one individual to another. Your particular perceptual associations with the phrase "old stone tower" are idiosyncratic and private, and they remain so even as we come to agreements based on transmission of those words between us. For me, the image includes rough dark gray stone, probably some kind of granite or basalt, with moss and lichen on it, and the tower is round with crenelations on it. For someone else these details of perception may differ, and likely both of us have perceptions different from those of the writer, without prejudice to communication. Any exemplar will do for purposes of the communication. Whatever is "inherent" in the words must be of the same level of generality and abstraction as that proposed for category perception. But the associative meanings evoked on reading the words are much more particular and detailed than the category level by itself warrents. Why? The associative reverberation outlined above pulls in facts known to me about the geological environment in New England and in Rhode Island in particular. These associative perceptions are not inherent in the word "stone". They are pulled in through memory and imagination by way of the prior occurrence of the words "Rhode Island." If the text were set in Florida, I would have associated very different perceptions with the word "stone".

If the meanings were inherent rather than merely associative, then one would expect the relation between meaning and word to be reflexive. The text evoked for Bill the image of some men standing around a tower with shovels. Does this mean that the image of some men standing around a tower with shovels evokes the word "scientist" in Bill's mind? No, the phrase "they dug under it" (in discourse context) evoked that image for Bill. And a rather different image for me. I imagined a back-hoe for initial excavation, and people of both genders, a professor and students, with stakes, string, and trowels. Neither Bill's particular images nor mine are "in" the words or the sentence "The scientists dug under the tower" (undoing the reductions to "they" and "it"). I suspect that the image it evoked for you (if you remember it now) was different yet again. The words evoked the image, but the image does not evoke the specific words. To see this, suppose that image popped up in a new person's mind-- telepathically, let's say, so we know it's the same image. Is there any reason to suppose that it would evoke the word "scientists" for that person? No, these images are part of the meanings we each idiosyncratically associate with the words. They are in no sense "inherent in" the words. Whatever it may be that satisfies the input requirements for the category "scientist," those perceptions are not obviously present in the image of a group of people with shovels next to a stone tower on a sunshiny summer day. (Hm--what were the weather and season like in your image, Bill?) Could as easily be digging worms for a fishing trip. In my case, I construed "scientist" as a classifier word and replaced it with "archaeologist," which makes the association with the image easier--brought in the stakes, strings, and trowels in addition to the hole and the digging implements--but in no way obviates the ambiguity of the nonverbal perceptions. A variety of words other than "scientist" or "archaeologist" could still apply, like "gardener." Well, make the hole too deep for gardening. Must be archaeologists. But that is a circumstantial inference, not an "inherent" association of that sunshiny-day image with "archaeologist." And the inference is by way of words: archaeologists dig for artifacts, laying out a site in zones marked by stakes and string, digging carefully and slowly and sifting as they go. Each of the more concrete-reference words like "string," "stake," "sift" has some more specific imagery associated with it, so that we come closer to a relation of "inherent meaning" between the word and the evoked nonverbal perception.

How can the associative meanings "inhere" in the words? In my view, they are associated with them, and are an interpretation of them. They are nonverbal perceptions. Many of them are remembered and imagined, some of them arise in real time from "the pragmatic situation of the talker and listener."

But some kinds of meaning are "in" the utterance. The linguistic information (the operator-argument structure and discourse structure) is not just a carrier for the associative meanings, devoid of content. The correlation of linguistic information (the objects and relations of language) with particular nonverbal perceptions is an interpretation of the linguistic information for the individual language user making the associations. The linguistic information (words and word-relations) is much more stable and constant across individuals than the particular nonlinguistic associations are or ever can be. The former can easily be isolated in a way that different observers agree about, and then studied; the latter are difficult even to determine, and when determined turn out to be typified by disagreement.

Particular substructures of words are probably associated with single category perceptions, even while (concurrently, in parallel) the words may be associated separately with other category perceptions. I gave examples of idioms and fixed expressions. Another example I have given is "the beating of the heart" as a single element of the category "symptom" in a sublanguage of pharmacology, though it is five elements (more with undoing of all reductions) in other usages, and in particular in the sublanguage of physiology from which it is borrowed for pharmacology. This is the sense also of (Bill):

>phrases like "I could
>care less" aren't really phrases. They're words with spaces in them.

[A digression: the basis of reinterpretation in those particular N.Am. dialects that have this--mine doesn't--seems to be the sense that "less" already carries the semantics of negation ("I deny" in the source) and so the "not" may be zeroed, or perhaps even is "wrong" as the "error" of double negation. Avery's

   I could give a(n) X

is similar: X must be something of negligable value, e.g. "I don't give [even so much as] a hoot" or more grimly "I don't give [even so much as] two shakes of a dead lamb's tail." As for "an," Bill, I have heard "I don't give two shakes of an aspen leaf," but probably most good pejoratives in English begin with consonants, so you can spit them out.

And of course the phrase begins with "two" here. End of digression.]

Familiar quotations like "now is the time for all good men to come to the aid of their party" introduce some gratuitous confusion because there is a recognition of the sentence as a whole. Does that mean there is a single recognizer for it? Or is that a special case of associative memory?

We don't become aware of the category perceptions, but only of particular exemplars of the category. (I wondered a while back if the category perception could be in the form of an idealized exemplar plus capacity to analogize over missing or changed features, but this is different: *particular* exemplars.) Perhaps the category-perceptions are part of the linguistic information and hence more "inherent" than the associated meanings. (I had proposed this some time ago.) But the particular images and other perceptions of which we become aware while musing over the utterance are associative meanings.

Note especially that a perception of likelihood may arise from error signals in the associative reverberation process sketched above, but neither statistical nor subjective probabilities are required for carrying it out.

Bill, I think you will find this picture accords well with your view expressed (920104.0900) to Avery "I think of the process of sentence construction as one of _assembly_" and so on. One way to model it, seems to me, is to simulate the inputs from other parts of the perceptual hierarchy. One way to do this simulating is to represent those inputs as discourses on the same subject matter (see (8) in the process). Hence, proposals that I and Stephen Johnson have independently made for knowledge representation can serve as the start of PCT modelling, provided only that the supplementary contextual material adduced as out of memory and imagination could plausibly have got there by the process (1D9). Efforts to work with language in an encapsulated way may enable us to model higher levels in a manageable way without having to wait for all the lower-level modelling to be well in hand. Such a programme (Lakatos-type programme vs computer program) does *not* imply that the whole perceptual hierarchy devolves to language.

Martin, at the end of your (920104.20:50) you renew your claim that "fast" associative processes are primary, and processing of syntax comes into play only when the two disagree. I submit that this is a difference that makes no difference, since both kinds of processing are going on concurrently in parallel, and the check for disagreement can come only after the reconstruction of words and word dependencies (the linguistic information).

Your distinction between similarity processing (immediate) and distinctiveness processing (cutting in later) seems to me to follow pretty directly from the hierarchical organization of perceptual control. Recognizers on level n wake up immediately on appropriate input. They send signals to potential recognizers on level n+1. One of these responds with an error signal "wait a minute, on the basis of this input X feature I expect a Y feature, so this must not be a phusarp after all." The error signal from level n+1 leads to exploration and perception of distinction rather than similarity on level n. Voici a distinction-recognition process cutting in after several similarity-recognitions.

Bruce Nevin
bn@bbn.com

Date: Mon Jan 13, 1992 5:29 am PST
Subject: subjective probability as 1/e

[From: Bruce Nevin (920113 0817)]

(Martin Taylor 920110 20:30) --

>Hey, I thought you were going to listen silently for a while!

I was pretty silent all week, intending to steal some early AM time on the weekend before family get up. Did that, but gave in to temptation Friday too. Still haven't responded to Bill, hopefully next weekend.

>Your question about subjective probability, likelihood, and the error
>signal comes right out of left field for me, because I could never have
>thought of confusing likelihood with an error signal.

The question is, what do YOU mean by "subjective probability"? All the rest is my attempt to make sense of the term. I see no occasion for an ECS to use probabilities within the control hierarchy. When you said a control system acts to maximize probability, that seems to identify probability as a signal that an ECS seeks to maximize. My understanding is that an ECS seeks to minimize the error signal. Hence, it appeared to me that you might be using "probability" here as the inverse of error. What did you mean, when you said a control system acts to maximize probability?

Got to run,

Bruce Nevin
bn@bbn.com

Date: Mon Jan 13, 1992 7:31 am PST
Subject: Social Control

<<<<<[FROM CHUCK TUCKER ON THE SPUR OF THE MOMENT 011392.0958]>>>>>

Dear CSG'ers,

I can't believe that we are beginning a discussion of social control AGAIN since I have not yet cought up with the discussion on this topic last year (I know - that is my problem). But I would suggest that the difficulty may be reduced or even eliminated by the specification of what is meant by CONTROL. If one limits the definition of this term to what Powers calls for then there is no way that there can be anything like what is regularly thought of or specified as SOCIAL control; CONTROL is a single living system or such a system devised by a single living system based on the principles of negative feedback control. If you agree with that specification or definition then another word should be used for 'social control'; I would suggest 'social influence' or simply 'influence' but not as a "weasel" word, as is often done, but with the specification of 'control'. Of course, if one's purpose is to confuse and present a vague "text" then let the terms be defined as they currently are in the literature. I believe this is ONE reason that some who come on the NET have problems understanding the discussion; they don't get the very special way that 'control' is specified within CT, PCT or HCT as put forth by Powers (by the way, the Richardson book on feedback seems to recognize the difference in Powers's formulation and thus places him sort of in between the cybernetic and servomechanism threads [this is my reading of do have] is with this very particular definition of 'control' that is put forth in Powers's formulation).

IF ANYONE (YES, ANYONE) FINDS A PROBLEM WITH WHAT I HAVE JUST PROPOSED AS A SOLUTION TO THE 'SOCIAL CONTROL' QUESTION, PLEASE LET ME KNOW SINCE THIS IS WHAT I AM WORKING ON IN MY THINKING AT THE MOMENT AND WOULD LIKE TO FIND ANY ERRORS IN IT.

Best regards,   Chuck
Date: Mon Jan 13, 1992 8:27 am PST
Subject: Chuck: social control vs. influence

[From: Bruce Nevin (920113 1049)]

(CHUCK TUCKER ON THE SPUR OF THE MOMENT 011392.0958) --

Chuck, Martin has said he believes there is such a thing as a transpersonal control hierarchy. I have asked him where the comparator resides, where the perceptual input and reference signal come from, and where the error signal goes, and how these signals are communicated to elementary control systems (ECSs) in personal control systems.

This set of questions seems to me to bear centrally on the distinction you raised.

Martin, have I characterized your view adequately? Have you answers to these questions about transpersonal or social control? Or is it only mutual influence, with coming to apparent agreement as a goal in each party?

Bruce
bn@bbn.com

Date: Mon Jan 13, 1992 9:50 am PST
Subject: Re: Social Control

[Martin Taylor 920113 11:45]

Last year, I was in Ken Hacker's position with respect to social control. Now, I think I understand Bill's position, and have (perhaps) a briefer answer to Ken.

Consider the basic structure of an ECS

```
                          | reference
                          V
                    ------------------
                    |                |
        ----------->|  comp--->trans ----------|
        |   |   |   |                |         |
        |   |   |   ------------------         V
      perceptual                             output
        inputs                          (reference for lower levels)
```

"Control" is exercised by changing the reference signal, NOT by changing the percept. Changing the percept occurs because the output provides reference signals for lower level ECSs, which eventually results in behaviour, which, when passed through the environment with all its constraints, MAY result in changing perceptual input. MAY is where your example of the jailer comes in. You say that the jailer exerts control, whereas what the jailer does is to prevent the prisoner from effecting certain types of control, in that the percept does not change when the ECS changes its output.

In this context, as Bill tried unsuccessfully to explain to me last year, the jailer can affect ONLY the perceptual inputs, and not the outputs of the control system. Nevertheless, in a more free context, if person A wishes to "control" person B, A can do this if A (correctly) believes that B has a fixed or predictably changing reference signal. In this case, A can create an environmental disturbance that produces a percept that creates an output desired by A from the ECS. This output then controls the lower-level ECSs, and can simulate the control of B by A.

I think this covers Greg's Sheep/Wolf situation, too.

The myriad of constraints in a social environment cannot be construed as social control, but must be seen as social barriers that impede control. They constrain what perceptions the individual person can bring to a desired state. External control would be possible only if there were some link from the external perceptual world to a top-level reference signal that did not go through a control system (and it would be a paradox if it did, because then the affected reference signal would not be top level). There is a bit of a theoretical hole here, because noone has satisfactorily described where those top-level references do come from, but I don't think that hole affects the main point, that what we see as social control is mislabelled.

Have I got it right yet, Bill?

Martin

Date: Mon Jan 13, 1992 10:03 am PST
Subject: Re: Chuck: social control vs. influence

[Martin Taylor 920113 12:30]
(Bruce Nevin 920113 10:49)
>
>
>Chuck, Martin has said he believes there is such a thing as a
>transpersonal control hierarchy. I have asked him where the comparator
>resides, where the perceptual input and reference signal come from, and
>where the error signal goes, and how these signals are communicated to
>elementary control systems (ECSs) in personal control systems.
>

I think there is a minor miscommunication here, or else I have changed my views and forget what they were (anything is possible). My view of the communication problem is much like that of Tom Bourbon with several people controlling one thing, such as four people carrying a heavy object and keeping it level. If they all are intending to cooperate, it works.

In the communication system, there is what I would call "transpersonal control" in that each protocol layer is intending to perceive that the other has a compatible understanding of some message. A transmitting protocol intends to perceive that the responses generated by a corresponding receiving protocol have desired characteristics, and a receiving protocol intends to perceive that the corresponding transmitting protocol sends messages that indicate it perceives what it wants to perceive.

There isn't any concept of an ECS that transcends the two people, or that either is actually controlling the other in any technical sense. If one of them isn't being cooperative, the communication will probably fail, just as it will if one of the people trying to carry and keep level the heavy object decides to sit on it instead. I'd call the cooperative situation "transpersonal control" since it involves a control loop that incorporates the control systems of more than one person, even though from the viewpoint of each, the other is only an environmental disturbance.

Martin

Date: Mon Jan 13, 1992 10:13 am PST
Subject: Spur-of-the moment problems

From Greg Williams

[Chuck Tucker on the spur of the moment 011392.0958]

>But I would suggest that the difficulty may be reduced or even eliminated by
>the specification of what is meant by CONTROL. If one limits the definition
>of this term to what Powers calls for then there is no way that there can be
>anything like what is regularly thought of or specified as SOCIAL control;
>CONTROL is a single living system or such a system devised by a single living
>system based on the principles of negative feedback control.

Despite some infelicities of expression, I believe you are right about what Powers' definition of CONTROL is. But I claim that it does NOT follow from accepting that definition that there cannot be "true" (Powersian) control which is a part of what is "regularly thought of or specified as SOCIAL control." This was the point of my "sheep in wolves' clothes" post. Did you miss it? (The post, not the point!)

>If you agree with that specification or definition then another word should be
>used for 'social control'; I would suggest 'social influence' or simply
>'influence' but not as a "weasel" word, as is often done, but with the
>specification of 'control'.

Sometimes, there is indeed only "social influence" -- when the influencer is not CONTROLLING FOR maintenance of others' perceptions.

>Of course, if one's purpose is to confuse and present a vague "text" then let
>the terms be defined as they currently are in the literature.

I am trying to clarify and distinguish terms in accordance with CT. I like the theory, but think some parts of its meta-theory as expressed by various control theorists previously on the Net go too far when claims are made that I can't control (in some circumscribed ways) the way others' hierarchies behave. For example, Gary Cziko has dismissed the notion of social control because we can't reset others' reference signals directly. True social control doesn't need to reset others' references -- it can use the ones already there; the social controller CONTROLS for others' perceiving some things they (and the controller) want, and for the others' NOT perceiving other things they might want (and the controller DOESN'T want). Bill Powers has dismissed social control (in part) with his "competing for different reference levels" example. But my example of true social control (in the "sheep" post) doesn't involve different reference levels -- both parties seek the SAME perception (and the controller CONTROLS for that perception so that the other won't decide to seek a different perception).

>IF ANYONE (YES, ANYONE) FINDS A PROBLEM WITH WHAT I HAVE JUST PROPOSED AS A
>SOLUTION TO THE 'SOCIAL CONTROL' QUESTION, PLEASE LET ME KNOW SINCE THIS IS
>WHAT I AM WORKING ON IN MY THINKING AT THE MOMENT AND WOULD LIKE TO FIND
>ANY ERRORS IN IT.

As you can see, I am having BIG problems with it.

Greg

Date: Mon Jan 13, 1992 10:27 am PST
Subject: Social Control (sic)

<<<<<<<<FROM CHUCK TUCKER AGAIN ON THE RUN 011392:12:54>>>>>>>>

To: Martin 011392 ?????

Re: Transpersonal Control

If you mean that 2 or more persons are controlling their own conduct to accomplish a goal (like moving a table across the room, carrying the Christmas tree out of the room; pushing the car down the street; pushing the car over on its top - and such "cooperative acts") with each other then it is fine with me. I like "transpersonal" rather than "interpersonal" just like I prefer "transaction" rather than "interaction" but I am usually not strict about those differences. But I do think we should reserve the word 'control' by itself for the individual living system just to make our talk clearer and better understood (which is an act of transpersonal control, isn't it).

Regards,   Chuck
Date: Mon Jan 13, 1992 10:42 am PST
Subject: reality,control

[From Rick Marken (920113)]

Avery Andrews (920112?) says:

>   But I think the terminological
>setup and orientation of much of the discussion would tend to make people
>interested in the `ecological viewpoint' think that there was a
>conflict.

What is the 'ecological viewpoint'? If it's related to Gibson's theory of perception and Turvey's theory of action then I think that there are serious areas of conflict with PCT. If it's just a viewpoint that says inability to detect predators can be hazardous to your health, then there is no conflict (at that level, anyway). If you want to call some controlled variables (like distance between you and a lion) ecological, then OK, but I don't see what it gets you. Control theory recognizes that living control systems MUST control certain variables (Bill called them INTRINSIC variables) or die. Those variables are not things like "distance from lion" but things that are influenced by attacks by lions, among other thing -- these are variables like blood sugar level, blood pressure, pH level, O2 levels in the cells, etc. The model says that these variables are controlled indirectly -- by controlling perceptual variables that influence them. Thus, the organism learns to control certain sensations, movements, relationships, etc that constitute "eating food", "running from lion", etc. When and what you eat, run from, etc is determined, to some extent, by "intrinsic error" the degree to the level of an intrinsic variable differs from its reference level. But there are many different ways to build a perceptual hierarchy that will control intrinsic variables -- thus we get people who actually consider lima beans food and others who place their hears in lion's mouth's.

Since, in theory, the entire perceptual control hierarchy exists as part of the means for controlling intrinsic error, then all variables controlled by the perceptual hierarchy are "ecological" . All variables, from intensities to system concepts, are controlled at their specified levels, because that is what keeps the intrinsic variables at their references. This is the model, anyway. So I think that, from the point of view of the PCT model, the concept of "ecological variables" -- external variables that have special significance for the survival of the organism-- does not seem particularly useful.

Chuck Tucker (920113) says:

>   But I would suggest that the difficulty may
>be reduced or even eliminated by the specification of what is meant by
>CONTROL.

I agree that it's important to define "control" but I doubt that this will eliminate the problem of the possibility of social control -- it will just clarify the issue.

I disagree with your proposed definition of control:

>   CONTROL is a single living system or
>such a system devised by a single living system based on the principles of
>negative feedback control.

Control is a phenomenon; it is stability in the face of instability. Control is seen in situations where a disturbance, d, is added to a variable, v, that is also influenced by the output of some system, o, so that v = d + o and these variables vary continuously over time. There is control when

var(v) << var(d)+var(o)
[var() is the variance of the variable in parentheses]

and

o = -d.

Variable v is controlled because system outputs,o, cancel the disturbance (o = -d) and v remains stable (the variance of v is less than expected given knowledge of the variances of the only two influences on it).

"Social control" is, then, control of a variable that is considered "social". Social control occurs when v is, say, "regularity of church attendance" or "number of speeches advocating atheism" or "productivity ". To the extent that I can keep v at the intended value, protected from disturbances, then I am doing social control.

There is no way (or need) to define away social control; it is a real possibility and people try it (with varying degrees of success) all the time. In order to understand why social control doesn't work (in the same way that, say, control of the air/gas mixture in a car works) you must understand how a hierarchical control system DOES work. This, I presume, is what your class will be about.

Re: Greg Willaims' Sheep in Wolves' Clothing post:

I think Greg's point is that PCTers should not underestimate the potential "misuse" of PCT.

>   It is a dangerous myth that no one but you controls your
>perceptions. And it is a dangerous myth to believe that control theory (and,
>especially, the test for the controlled variable) are not prime tools for the
>kits of chronic deceivers.

With or without PCT there will be people who want to control others (ok, Bill, I mean control variable consequences of the actions of other people) and who are willing to use any means to do it. I don't think PCT will help much in their efforts -- the best methods of control are already known (just ask a mob member).

All PCT can do is show, through scientific modeling, why interpersonal control can't work in the long run, why it creates more problems than it is intended to solve and why people are so reluctant to abandon it anyway (they are control systems). PCT offers no special cures for the world's problems (none that have not already been suggested verbally by many wise people in many different ages) and it offers no special threat. It does help understand why "social control" is both unfeasible and bound to create more problems than it aims to solve.

Hasta Luego

Rick

Date: Mon Jan 13, 1992 11:00 am PST
Subject: Re: subjective information

[Martin Taylor 920113 13:30]
(Bruce Nevin 920110 0734)

Bruce asked:
>
>Martin, what do you mean by subjective probability?
>
>Your response (910203 17:15) seems to agree when I equate it with
>perceptions of likelihood (however determined) of other perceptions:
>
>>>As I understand it, we PCT folk might think of subjective probability the
>>>perception of likelihoods associated with other perceptions. The

>>>evidence on which these perceptions of likelihood (or reasonableness,
>>>etc.) is based may include frequentist probabilities, but need not.
>
>>I think it might be better to think in terms of the "imagination" loop,
>>to distinguish the perception of likelihoods of possible "other perceptions"
>>from some externally controllable perception, though, of course, one aspect
>>of control is to bring to a maximum the likelihood of a desired perception.
>
>The last clause confuses "likelihood" with the error signal. ...

I promised a response, and here it is. I hope it clarifies things to some extent, but I am not naive enough to hop that it brings complete understanding.
==========

Bruce asks about the relations among subjective probabilities, the likelihoods of different percepts and the error signal of a control system. His question threw me for a (closed?) loop, but I think I may have a handle on an approach to his difficulty.

First off, look at the control hierarchy with everything eliminated except the perceptual transforms (i.e. no comparators, no reference signals, no error signals). What do you have left? A multi-layer perceptron, if you use the simple model in Bill's 1979 Byte articles (yes, Bill, I found them and copied them). What will be present at the outputs of the various nodes of this perceptron (the perceptual inputs to the comparators of the full control hierarchy)? The answer depends on the disturbances of the environment, since the system as a whole is paralyzed. The output values of the perceptual nodes will have some distribution, different for each node. In principle it would be possible for a device looking at the output of any node to estimate the relative likelihoods over time of different outputs, and to estimate those likelihoods given recent history, or given values at other nodes, or given any other information it had access to.

In a Newtonian view, at any moment the output of a perceptual node has a "value" that could, in principle, be represented by a number that had infinite precision. But in an Einsteinian world, that is not a sensible way of looking at it. One has to consider what really limits the observation. It's rather like saying that a waveform has a specific frequency at a precisely defined moment. We do say such things, but they have only a sloppy relation to what we should say; there is a tradeoff between how precise we can be about what we measure and over what time period we measure it. If we know infinitely precisely what the value is, we don't know when it had that value, whether we are dealing with a frequency or any other observation.

Quite apart from that limitation of principle (but maybe, at a deep level, because of it), any measuring device has a limited resolution, and that includes whatever comparator might be in the ECS that incorporates the perceptual node we are considering. Accordingly, the "value" used by the ECS actually represents a subjective probability distribution over the possible underlying "true" values of the perceptual input.

So, stage one: before looking at what the input value is, the node can have a subjective probability distribution for what it may be. After observing the value, the node has a different subjective probability distribution for what the value was. The difference between the uncertainties represented by these probability distributions represents the information gained from the observation. In practice, that information gain occurs over time, since very often the observation at time t is much better predicted by the observation at time (t - epsilon) than by observations deeper into history. The node output is usually redundant over a time scale commensurate with the observation rate. That's where the notion of "equivalent rectangular bandwidth" or Nyquist sampling rate comes in.

The Nyquist rate, under rather special conditions, is the fastest sampling rate at which each observation provides information independent of that given by other observations. It has nothing to do with the actual rate at which observations are taken, and in any case the special conditions are rarely applicable under the kind of circumstances with which we are dealing. (They require the signal to have uniform energy over a specified bandwidth and zero energy outside the band, and to have no internal redundancy). Nevertheless, Nyquist rate, or equivalent rectangular bandwidth, is a good concept to use for comparing information rates and the like, so we stick with it in many discussions.

OK, now let's put back the rest of the control system hierarchy, so that each node output is continually being driven toward a value known to the ECS of which it is a part (the reference value for that ECS). Putting the perceptual node into the context of a behaving control system introduces several additional sources of uncertainty, even though its expected value distribution is sharper than when the behaving system is paralyzed. Firstly, the environment may not be accomodating, as in the situation I asked about a month or so ago in the discussion about whether multidimensional control systems are sometimes needed; there may, for example, be a barrier that prevents any particular ECS from reducing its error, and a higher level ECS may achieve its own error reduction in a way that increases the error for the blocked lower-level one. Secondly, as we discussed in the middle of last year, the environment, as well as the higher-level control system set, has many more degrees of freedom than the low level control systems (Bill pointed this out in his Byte articles in 1979, and maybe in BCP). This ensures that most of the control systems cannot at any time bring their errors to zero, so even if the system could come to a steady state, that steady state would have some unknown distribution of percepts at a particular ECS. Thirdly, the system as a whole never is in a steady state. Things move, disturbances happen. So, the future percept at any ECS again has some distribution of likelihoods.

In the context of the fully operating system, the distribution of likelihoods for future percepts is, of course, affected by the current reference. If that reference were not to change, the error would be more likely to reduce than to increase over time, but even that would not be guaranteed, for the reasons mentioned above. But the reference is unlikely to remain unchanged, as things change at higher levels, as well. So there is a subjective distribution of reference levels to be considered when the ECS evaluates the likelihoods of future percepts.

If, as one presumes, the ECS has no information other than its history of its own inputs and outputs, then all these probabilities must be derived from some function of that history. Maybe the information is totally discarded, and the ECS remains "hardwired". The subjective probability in such a case has been determined by evolution, not by experience. But maybe it learns, and such items as output gain or weights on the inputs to the perceptual transform function change are altered, to improve the ability of the ECS to reduce its error signal in the face of a variable environment that includes conflict as well as external blocking and disturbances. At higher levels such as the program level, these changes may be more complex, perhaps taking probabilities into account even in an analytic way.

I hope this helps.

Martin

Date: Mon Jan 13, 1992 11:01 am PST
Subject: Time and PCT theory

Having belatedly realized that the perceptual structure of the "classic" perceptual control hierarchy (that of Bill's 1979 Byte article) is that of a multilayer perceptron (MLP), I have some questions relating to time and sequence.

There is a lot of ongoing work in the neural network community trying to accommodate the recognition of sequence, most particularly sequence in which the relative durations of the component events change on different occurences of the "same" sequence. This is particularly important for speech recognition, and is a problem that is far from solved by people working with neural nets. Sometimes even the "same" sequence involve temporal inversion of events, and this is an even harder problem to resolve. The classic (non-neural net) technique for dealing with temporally variable sequences, Dynamic Time Warping, will not deal with it. Two ways for sequence recognition that have been tried in speech recognition (and that work to some extent) are to include recursion in the net structure, or to include shift registers at each layer, of which several time delays are simultaneously fed to higher layers. Various people have tried different tricks, but they all involve some kind of special structure to deal with time as such, and task-specific modularity in the network itself.

Control systems inherently incorporate time, but unless some external control of time delay is incorporated into the structure of an ECS, the time scale is that of the loop transport delay. No such control exists in the classic PCT structure. There is also an implicit time scale associated with the bandwidth of stable control, but this is related to the transport delay, and seems to be intrinsic to the structure within which the ECS finds itself.

The MLP formed by the perceptual functions of the hierarchy is embedded in a control network. This fact seems to promise some advantage in the ability of the MLP to handle sequence recognition, but I cannot see how this happens. For one thing, the timings require the external world part of the control loop, and much of what we are perceiving is passive (e.g. word recognition from acoustic waveforms).

If the MLP of the control hierarchy incorporates shift registers, it risks the stability of the control systems whose perceptual inputs contain those shift registers at their own or any lower level. If it does not, then it presumably incorporates recursion, which means that there is no strict hierarchy of levels, but a heterarchy. I see no problem with a heterarchy, but all the discussion seems to revolve around the idea that ECSs at level N get their reference from ECSs of level N+1 and provide reference signals to ECSs of level N-1. To incoporate recursion, ECSs of level N would have to be able to provide reference signals at least to other ECSs of the same level, and perhaps to those of a higher level. This seems to complicate the control system as a whole, and to provide a great opportunity for all kinds of instability. The whole thing seems a can of worms, but maybe the solution is a simple one I don't see.

How are event and sequence levels provided with their percepts (not to mention higher levels)? Most particularly, how are the problems of variable duration sequences handled, with or without shift registers? Does the incorporation of the MLP into a control hierarchy ease or exacerbate the problem? Is there a "Little Man" type of demo that demonstrates sequence control?

Martin

Date: Mon Jan 13, 1992 11:37 am PST
Subject: Re: associative meanings as interpretation

[Martin Taylor 920113 13:40]
(Bruce Nevin --date and time scrolled off my display 920113?)
>
>
>Martin, at the end of your (920104.20:50) you renew your claim that
>"fast" associative processes are primary, and processing of syntax comes
>into play only when the two disagree. I submit that this is a
>difference that makes no difference, since both kinds of processing are
>going on concurrently in parallel, and the check for disagreement can
>come only after the reconstruction of words and word dependencies (the
>linguistic information).
>
>Your distinction between similarity processing (immediate) and
>distinctiveness processing (cutting in later) seems to me to follow
>pretty directly from the hierarchical organization of perceptual
>control. Recognizers on level n wake up immediately on appropriate
>input. They send signals to potential recognizers on level n+1. One of
>these responds with an error signal "wait a minute, on the basis of this
>input X feature I expect a Y feature, so this must not be a phusarp
>after all." The error signal from level n+1 leads to exploration and
>perception of distinction rather than similarity on level n. Voici a
>distinction-recognition process cutting in after several
>similarity-recognitions.

We do seem to be mired in misunderstandings, don't we? My claim is about the processes *at every level of abstraction*, in contradistinction to the frequently made suggestion (which you repeat) that the distinction-recognition process occurs at a different and higher level of the ladder of abstraction. The process you describe, in my view, happens in BOTH distinction-process track and similarity-process track. In the one, the answer is "this is distinct from a phusarp" and in the other "phusarp is not among the things this is similar to." Check Chapter 10 in "The Psychology of Reading" by I. Taylor and me, Academic Press, 1983, or Chapter 17 in "The Alphabet and the Brain" (De Kerkhove and Lumsden, Eds.) Springer-Verlag, 1988, for more detailed explanations and experimental supporting references.

The claim is also that the distinctiveness process at a level is usually preempted because the similarity process at a higher level finds a
satisfactorily
unique solution. If it does not, that may be because it has found no solution or because it has found too many solutions. In the former case, the distinctiveness process takes on its other guise, as a rule-based pattern matcher, in an attempt to find a solution (indeed, in the case of no similarity solutions, it has never been aborted in this attempt). In the latter case, it weighs the similarity solutions, as goals in its pattern matching process. It always starts with a goal of "any match", and depending on whether the similarity process produces one or more results, changes

its goal to "check correctness of this pattern" or "select among alternatives."

If a higher-level process (not necessarily in control-theory terms) is happy with one of the similarity results, not only is there no need for the distinctiveness process to keep working, but also it can be positively detrimental, as it could cause denial of the validity of a malformed "instnace". This is what happen in lexical decision experiments, in which the subject is asked whether a particular letter string is a word or not. If that string were to be found in a text, it would be taken to be a word, and the question would be about the writer/speaker's intent in using it, malformed or not. If the malformation leads to ambiguity of interpretation, then other aspects of the input ("syntax" at that level of abstraction) come into play.

Martin

Date: Mon Jan 13, 1992 11:38 am PST
Subject: thanks re transpersonal hierarchy

[From: Bruce Nevin (920113 1353)]

Martin, the reference that concerned me was in your galumphry post of (920103 12:00), quoted together with my questions about it in my response of (920103 1448) as follows:

>My own opinion is that meaning inheres in the hierarchic
>mutual control systems that connect the participants in a conversation,
>and that it is intimately related to galumphry.

Thank you for clarifying what you meant.

I plan to look at your post on subjective information this evening on the train. Don't know when I will be able to respond, perhaps the end of the week.

Bruce
bn@bbn.com

Date: Mon Jan 13, 1992 12:15 pm PST
Subject: Re: Martin & Rick on social control

From Greg Williams

[Martin Taylor 920113 11:45]

>"Control" is exercised by changing the reference signal, NOT by changing
>the percept. Changing the percept occurs because the output provides
>reference signals for lower level ECSs, which eventually results in
>behaviour, which, when passed through the environment with all its
>constraints, MAY result in changing perceptual input.

In CT, control is exercised by maintaining a perceptual signal close to its reference signal. Via deception, and depending on my model of your hierarchy, I can control MY own perceptual signal of the form "I perceive you will controlling for THIS ONE (out of many possible) of your reference signals." By providing you with a perception which, if you took it to be true, would CHANGE which reference signal you would control for (i.e., getting up and going into another room, instead of lounging), I maintain that I am controlling for perceiving you continue to control for a particular reference signal OF YOURS, and that I am controlling for perceiving you continue to NOT control for certain other reference signals OF YOURS. I will defend that control in the face of disturbances (making up more lies if circumstances threaten to give me away). I am exercising control by defending AGAINST your changing to a different reference signal. And you might never "know" that.

>In a more free context, if person A wishes to "control" person B, A can do
>this if A (correctly) believes that B has a fixed or predictably changing
>reference signal. In this case, A can create an environmental disturbance
>that produces a percept that creates an output desired by A from the ECS.
>This output then controls the lower-level ECSs, and can simulate the control
>of B by A.

>I think this covers Greg's Sheep/Wolf situation, too.

>The myriad of constraints in a social environment cannot be construed as
>social control, but must be seen as social barriers that impede control.
>They constrain what perceptions the individual person can bring to a
>desired state. External control would be possible only if there were
>some link from the external perceptual world to a top-level reference
>signal that did not go through a control system (and it would be a
>paradox if it did, because then the affected reference signal would
>not be top level).

Our only disagreement is that I think my control which impedes your control, as outlined above, is genuine control. Nevertheless, your bringing in control at the top raises interesting issues. If the highest level is CONSTRUCTED in development, investigating its determinism would be very interesting. You see, Gary has (had?) the idea that what saves CT from determinism is going up. But if the top is historically determined (please don't bring in chaos on me -- I can argue for robustly (i.e., redundantly constrained) limit cycles which obviate chaos, and nobody has any data on any side of that sort of argument!), then so are its results. Shades of "genetics + history + discriminative stimuli (all right, call them disturbances) = current behavior"! And the putative stochastic -- not entirely random! -- mechanism of reorganization doesn't save it; it still goes into "history."

[From Rick Marken (920113)]

There is no way (or need) to define away social control; it is a real possibility and people try it (with varying degrees of success) all the time. In order to understand why social control doesn't work (in the same way that, say, control of the air/gas mixture in a car works) you must understand how a hierarchical control system DOES work.

Amen! (Though that might be addressed differently than yours! Mine is return receipt requested.)

Re: Greg Willaims' Sheep in Wolves' Clothing post:

I think Greg's point is that PCTers should not underestimate the potential "misuse" of PCT.

>   It is a dangerous myth that no one but you controls your
>perceptions. And it is a dangerous myth to believe that control theory (and,
>especially, the test for the controlled variable) are not prime tools for the
>kits of chronic deceivers.

>With or without PCT there will be people who want to control others (ok, Bill,
>I mean control variable consequences of the actions of other people) and who
>are willing to use any means to do it. I don't think PCT will help much in
>their efforts -- the best methods of control are already known (just ask a
>mob member).

I disagree that the mob has the best methods (I assume you mean threats and applications of overwhelming physical force). CT shows why these methods don't work well (the "marks" know damn well that they'd better run and hide -- and a good place to hide is with the Feds!), AND why more subtle and devious methods often work better. A spy would tell you how useful the concept of the Test would be for aiding his/her dirty work. But so far, I've only matched your opinion, Rick, with my own. It would be more interesting to gather some data. You're the psychologist who claims that CT wouldn't help "much" in controlling "others" (I'm not being precise here, but neither were you; I tried to be precise in my "Sheeps" post). So lay the CT rap on them (not the whole thing -- the hypothesis about there being a hierarchy of controlled variables with reference levels WHICH YOU CAN IDENTIFY VIA THE TEST should do nicely) and see whether it aids their control. I bet it will. Familiarity with another's hierarchy (not all the details -- a little goes a long ways!) is the Royal Road to manipulation, should you be so inclined.

>All PCT can do is show, through scientific modeling, [is] why interpersonal
>control can't work in the long run, why it creates more problems than it
>is intended to solve and why people are so reluctant to abandon it anyway
>(they are control systems).

I disagree with most of this premise. The kind of control I outlined in "Sheep..." neeedn't necessarily become impossible over "the long run" and needn't "create more problems than it is intended to solve." By and large the "controllees" (again, speaking loosely) "never knew what hit them" -- in fact, NEVER KNEW ANYTHING HIT THEM. However, I do think CT helps to show why successful controllers don't want to give up the technique I outlined. It works -- they successfully control for the perceptions they want to control for.

>PCT offers no special cures for the world's problems (none that have not
>already been suggested verbally by many wise people in many different ages)
>and it offers no special threat.

CT IS A TOOL. Certain tools in the hands of certain people pose threats.

Greg

P.S. Just noticed the lack of ">"s before Rick's first paragraph. That is, the paragraph before I said "Amen." is by Rick, not by me. Sorry, Rick.

Date: Mon Jan 13, 1992 1:13 pm PST
Subject: Kathy Kolbe

from Ed Ford (920113.13.40)

I met with Kathy Kolbe (along with her husband, Will, and one other associate) this morning for an hour and 15 minutes. It was a most enjoyable meeting. I found her very bright, articulate, well-read, and eager to understand control theory and what I had to say.

Unfortunately, not being a theorist nor acquainted with much of what she had read (except CSG stuff), I wasn't really the "right" person for the meeting. I felt the best way to help you understand her thinking was for me to say nothing about what she had to say, but rather to let her speak for herself. I suggested that very approach to her and she had already begun to look in to how you join the CSGnet. The bottom line is that a) she joined the CSG and paid her dues, which I promptly sent to Mary, and b) she promised to join the CSGnet (I gave her our address). She also expressed interest in coming to our conference this summer.

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

Date: Mon Jan 13, 1992 2:26 pm PST
Subject: More re: Martin on social control

Martin, if REAL control of someone by another requires altering their highest reference signals, how come at least some control theorists use the line "no control without overwhelming physical force"? I don't see overwhelming physical force as necessarily doing ANYTHING to the highest reference signals of the "controlled." And yet its application apparently can allow control of someone (to which the force is applied) by another (the applier).

Greg

Date: Mon Jan 13, 1992 2:32 pm PST
Subject: Misc. messages

[From Kent McClelland 920113]

I'm posting to the net to answer some messages that have been accumulating, though I almost hate to add to the overwhelming volume of material coming across the wires these days.

Tom Bourbon (920107 -- 1:57)

Thanks for your kind words on the ms. I will be happy to send you revisions to scan, assuming I eventually find the time to write some revisions.

Bill Powers (direct)

Thanks for sending me the invoice for the Demo programs. I've sent it on through channels and you should get some money eventually. I appreciate your playing along with our redtape mindset here.

Gary Cziko (direct)

For a "standard" brand sociology paper to contrast with the "PCT and Soc Theory" ms., you could try

Ridgeway, Cecilia, and Cathryn Johnson. 1990. "What Is the Relationship between Socioemotional Behavior and Status in Task Groups?" _American Journal of Sociology_ 95(5): 1189-1212.

This article may have more of a social-psych flavor than you want, but it appeared in a mainstream soc journal. The paper is theoretical in scope, and though it doesn't present any complicated data, it does focus on predicting "behavior" and casts its argument in statistical terms. The authors come to the conclusion that "the status hierarchy, through performance expectations and legitimacy norms, controls negative but not positive socioemotional behavior" (p. 1208), which has a nicely non-PCT ring to it. Tell me if this is the kind of thing you are looking for.

Greg Williams (920104, 920110)

I notice you've been looking for responses to your posts on "Lies" and "Wolves" and I wanted to say that personally I've found them quite interesting, though I haven't had the leisure to work out a substantive response. In fact, the whole recent thread on social control by Ken Hacker, Bill Powers and others has been fascinating from my point of view, and I haven't even had time to consider the new flood of material today in response to Chuck Tucker. I guess I hope that you and others will keep it coming (though maybe not too much of it!).

Avery Andrews (920109)

Your discussion of "p-control" and "e-control" raises an question that I too have had some difficulty in sorting out, as some of those on the net who looked at diagrams from an early version of my PCT paper may remember. I believe Rick Marken responded to your post, but I was hoping someone would deal a little more directly with issues you raise about _where_ controlled variables are located. (Or is this one of those "if a tree falls in a deserted forest, does it make any sound?" conundrums?)

Best wishes to all,

Kent

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Grinnell, IA 50112-0810

Date: Mon Jan 13, 1992 3:49 pm PST
Subject: Re: More re: Martin on social control

[Martin Taylor 920113 18:00]
(Greg 920113 16:05)
>

>
>Martin, if REAL control of someone by another requires altering their
>highest reference signals, how come at least some control theorists use
>the line "no control without overwhelming physical force"? I don't see
>overwhelming physical force as necessarily doing ANYTHING to the highest
>reference signals of the "controlled." And yet its application apparently
>can allow control of someone (to which the force is applied) by another (the
>applier).
>

I think that the idea of "social control" implies something different from the physical control of the body of another. I could indeed control the behaviour of someone else by getting them into a complex bodysuit that had a motor to correspond with every joint, and of which I held the motor controls. Social control seems to me to imply that the "controlled" person is induced to do what the "controller" wants, without the actual physical manipulation of the controlled person's body or (equivalently) the intervention of the controller in the muscle control apparatus of the controlled person. Social control may involve the apprehension by the controlled person that the "controller" may use force if the action desired by the controller is not performed, but this effect is based on what I proposed in my response to Ken: The "controller" presumes that the "controllee" has a fixed reference that would generate an error signal if he felt pain. By generating data that the controllee is likely to perceive as the precursor of pain, the controlee is induced to control (adjust the reference signals of) lower level control systems.

One can "control" the motion of a stone (more properly, one's perception of the motion of a stone) in the same way as one can "control" another person's body. But more common is the Pentagon's approach to getting the Vietnamese to support the US side: Grab them by the short and curlies, and their hearts and minds will follow. It's control of a sort, but not, I think, technically control.

Martin

Date: Mon Jan 13, 1992 5:13 pm PST
Subject: Bad data

[From Bill Powers (920113.1200)]

Still worrying the same bone -- what's wrong with statistical facts about individuals. I'm not bashing statistical studies of populations -- only the attempt to apply population statistics to individuals. I should mention in this context the modern classic on this subject by a CSG member, Philip J. Runkel: _Casting nets and testing specimens_; New York: Praeger (1990). A must-read for anyone who uses statistics in connection with human behavior.

My objection isn't esthetic or moral: it's that the predictions of individual behavior that come out of mass measurements are very poor, much worse than they need to be, mostly from lack of trying to meet higher standards for acceptance of facts. Today's offering concerns what predictions from bad data look like.

I wrote a little program that plots the function y = 2x + [a random variable]. The random variable is just the "random()" function from the C library, so it doesn't conform to Gaussian statistics, but the results are at least suggestive. What we're pretending here is that a dependent variable y has been postulated to be proportional to an independent variable x, and that this hypothesis is used to explain a collection of data points obtained by varying x and observing y. If there were a perfect linear relationship in the data, the points would plot as a straight line. After generating an array of 24 pairs of data points, we calculate the correlation coefficient between x and y. The question then is, how well does the regression equation, y = 2*x, predict the value of y given the value of x?

In the plots below, x runs from top to bottom and y runs from left to right.

Here is the plot of y vs x when there is no random noise added to the measure of y:

```
---------------------------------------------------------------------
          *
            *
              *
                *
                  *
                    *
                      *
                        *
                          *
                            *
                              *
                                *
                                  *
                                    *
                                      *
                                        *
                                          *
                    correlation = 1.000
```

Obviously, given x you can predict y exactly. There is no scatter.
Here is what the data look like when enough noise is added to bring the correlation down to the level we get in easy tracking experiments:

```
---------------------------------------------------------------------
       *
            *
              *
              *
                *
                    *
                      *
                     *
                          *
                            *
                             *
                                  *
                                   *
                                  *
                                        *
                                     *
                                        *
                                            *
                                             *
                                              *
                                                   *
                                                      *
                                                     *
                                                       *
Correlation = 0.995
---------------------------------------------------------------------
When handle sensitivity gets too high or disturbances get large, the
correlation drops to the low 90s, something like this:
---------------------------------------------------------------------
              *
      *
           *
              *
                  *
                            *
                        *
                              *
                         *
                    *
                                    *
                               *
                                      *
                              *
                                        *
                                *
                                   *
                                                 *
                                      *
                                             *
                                                     *
                                              *
                                                              *
                                                   *

Correlation = 0.928
-------------------------------------------------------------------
```

In most statistical studies of relationships between dependent and independent variables, a correlation of 0.8 would be considered very high. Here is what the data would look like:

```
            *
                         *
*
      *
                     *
                    *
                 *
(8)    *
                                         *
          *
               *
(12)                                             *
                                     *
                                             *
                                             *
                    *
                       *
                              *
                                                            *
                                                            *
                                                            *
                                                                      *
                                                              *
                                                  *
Correlation = 0.798
-------------------------------------------------------------------
Even a correlation of 0.6 is considered rather good:
-------------------------------------------------------------------
*
                                     *
                             *
     *
*
*
                                       *
                                                     *
             *
                                    *
                                                           *
                *
                      *
                 *
                   *
                                                              *
                                                    *
                         *
                                      *
                                                                    *
*
                                                 *
                                                 *
                                                  *
Correlation = 0.620
----------------------------------------------------------------------
```

As Gary Cziko has reported, there have been published studies in which relationships with correlations of 0.2 have appeared. Here is that degree of correlation:

```
*
    *
*
                                                   *
                                                          *
*
                                             *
             *
           *
*
*
                                                           *
*
                                                       *
                                                       *
                                     *
                                              *
                                                       *
     *
                           *
        *
                           *
              *
                    *
```

Correlation = 0.201 (the points on the left actually went somewhat to the left of zero)

An interesting fact came up while I was generating these plots. When the argument of the random function is set to produce a correlation of 0.6, and the plot is generated over and over, the result can be any correlation between 0.3 and 0.8 on repeated trials, as different sets of 24 random numbers are generated. The implication is that with only 24 subjects, one can't say what the meaning of a given correlation is without re-doing the study many times. The first correlation obtained is very unlikely to be at the center of the spread of correlations. How many times do typical researchers replicate their studies, to find where the center of the range is? I suspect that the mean number of replications is close to 0.

Suppose that a person is exposed to 12 units of the independent variable x (top to bottom, halfway down). You want to use this score to predict that person's score on a test of the dependent variable y (left to right). Looking at the above plots, at what level of correlation would you begin to take the prediction of y seriously for that person? I would say that at r = 0.8, the prediction is too bad to use: clearly, the error in prediction would be something like 50% of the y-score. I wouldn't be much interested unless unless the correlations were up into the 0.90s.

Suppose that you were comparing two people, one with an x-score of 8 and the other with an x-score of 12. This would be like using one questionnaire to determine the independent variable, and using some other measure of the dependent variable. That's a difference of 4 points around the average of 10, or a 40% change in x-score. I've labeled the 8th and 12th lines in the plot for a correlation of 0.8. Clearly you would get the right comparison and then some. But suppose you move them both up one notch, or two, or three. Your prediction could differ from the actual difference in y scores by a large amount -- it could easily be backward.

Again, I don't think that any correlation lower then the 0.90s would be scientifically usable. And you don't get results that you could call *measurements* until you're up around 0.95 or better.

When you look at the plot for a correlation of 0.6, it's easy to see the trend. Clearly there's something going on here that you can see with the naked eye, despite the huge scatter. An effect! It's easy to overlook the fact that in order to see this "effect," you have to look at ALL the data points. You don't get this impression from looking at just a few of the points (put your hands over the plot so you can just see the center part). This "trend" you see is a property of the whole plot. The individual measurements don't "trend." Each point is where it is. The trend line, y = 2x, is far above many points and far below most of the rest. The distance from the trend line for each point shows you how badly the trend line misrepresents each point.

When you use the trend line to predict differences between people, the picture gets even worse. By drawing a line between various pairs of points, you can get slopes ranging from highly positive to highly negative. But the trend line predicts that the slopes should all be the same as the slope of the trend line. You have to get high into the 0.90s before comparisons mean anything at all.

There's another way to look at this. Somewhere around the 0.80s, the scatter becomes small enough that you could divide the y scores into a high group and a low group. You could then say that if the x score is less than, say, 6 or greater than, say, 18, it will predict that an individual point is in the low group or the high group. What has happened here is that the resolution of the "theory" y = 2x has become just great enough to treat the measurements as binary data: 0 or 1. We can pretty well tell the difference among 0,0 0,1 1,0 and 1,1. As the correlation rises above 0.8, the coarseness of the meaningful numerical measures falls: we begin to make out details. And when the correlation is in the upper 90s, we begin to get something resembling a continuous measurement scale.

When the resolution is too low, most of the data points are useless; it takes an extreme of the independent variable to predict that the dependent variable will be in the high group or the low group. In this case, the useful N is not the total number of subjects or points. It is a much smaller number, only the points indicating extremes of both x and y. Below correlations of 0.8, most of the points near the middle are useless. Even at 0.8, all we have is a crude measure that could easily be confounded by any slight effect from a common cause.

A true science needs continuous measurement scales so that theories about the forms of relationships can be tested. This means that correlations have to be somewhere in the high nineties. True measurements, with normal measurement errors, require correlations of 0.99 upward. If this were universally understood among scientists, two things would happen. The first is that most statistical studies would end up in the wastebasket. The second is that the good studies would be done again and again, with successive refinements to reduce the scatter, until something of actual importance and usefulness was found.

Best, Bill P.

Date: Mon Jan 13, 1992 6:41 pm PST
Subject: Re: Social Control

Ken Hacker [920113]

Chuck Tucker --

I agree with your comments that CONTROL is creating semantic chaos for our mini-debate here. I think it should. For all sides of the issue, we need to differentiate terms or we shall point fingers at ghosts all year. My argument boils down to this: Humans are self-regulating and regulate their own behaviors with hierarchical systems of signals and perceptions. However, the comparisons made between desired states and experienced states do not arise independent of social INFLUENCE. Moreover, that influence is nontrivial. Additionally, the core of the matter has less to do with understanding Bill than with ageeing with him on the rough edges. PCT is interesting; it is not yet perfected. The potential is wonderful and Bill is a source of wisdom and fascinating ideas. But let's not confuse what is good with what needs more work.   KEN

Date: Mon Jan 13, 1992 6:59 pm PST
Subject: Re: Social Control

Ken Hacker [920113]

Martin (920113) --

Thank you for a lucid description of Bill's position (and your own). I will respond tomorrow after I reflect on it and consider where my own comparator is having some trouble with some of the claims I have read. For now, substitute "social influence" for "social control" in my argument and you will be closer to my main points. Perceptual comparisons originate from somewhere; we do not operate on electro-chemical charges alone. I tend to believe that we have mental representations which are complex and which we use as bases for comparing inputs with desire states. I also believe that we do not simply react on the basis of comparisons, but make complex evaluations of behavioral path consequences.

You may be more right than me on these issues; I really don't know. I am using these conversations to induct, not to deduce anything or prove anything. What I personally am looking for is a connection between control theory and communication theory. When communication studies was beginning in the 1950s, we relied on Shannon and Weaver's model of signal tranmission to model how humans interact. We were all wrong and the model today is a footnote on a footnote for communication theory. One of our scholars has suggested that we should have used cybernetic descriptions of behavior instead of electrical signalling models. I am not sure what would have changed, but I tend to belive that both have a place in our discipline. Also, when we rejected behaviorism, we went strongly toward social constructivism with little focus on the individual as an adapting entity alone. I think this was also a bad turn.   Thanks for the ideas and perturbation.   KEN

Date: Mon Jan 13, 1992 7:00 pm PST
Subject: MOTIVATION AND EMOTION papers

I got a big surprise at the local college library tonight. Two papers on control theory from out of left field (well, one was mentioned in the A.P. Bandura reply to Bill Powers' reply to Bandura's article -- A.P. means AMERICAN PSYCHOLOGIST, not Bandura's initials).

The issue is volume 13, number 1, March 1991 [nominally, it appears they are running late, as the library got this on November 4, 1991]. Edwin A. Locke, "Goal Theory vs. Control Theory: Contrasting Approaches to Understanding Work Motivation," pp. 9-28. Howard J. Klein, "Control Theory and Understanding Motivated Behavior: A Different Conclusion," pp. 29-44. (The "Different" in the second title refers to the first article's UTTER AND TOTAL PAN of CT. Or so the authors (both of them) think! Suffice it to say that the quality of Locke's critique, in my humble opinion, is laughable.

What a nice opportunity to teach GENUINE CT to a brand-new audience, Bill P. I'll send copies of both articles to you tomorrow. The rest of you Netters should be able to get them through your institutions, I guess. A little light reading....

Greg

Date: Mon Jan 13, 1992 7:08 pm PST
Subject: Determinism & Evolution

[from Gary Cziko 920113.2010]

Greg Williams (920113):

>Gary has (had?) the idea that what saves CT from determinism is going up. But
>if the top is historically determined (please don't bring in chaos on me -- I
>can argue for robustly (i.e., redundantly constrained) limit cycles which
>obviate chaos, and nobody has any data on any side of that sort of argument!),
>then so are its results. Shades of "genetics + history + discriminative
>stimuli (all right, call them disturbances) = current behavior"! And the
>putative stochastic -- not entirely random! -- mechanism of reorganization
>doesn't save it; it still goes into "history."

I find this a very surprising statement, especially coming from someone who knows more about modern physics than I ever will. I had thought that all the Laplacean demons were long gone; perhaps they are just hiding out in the hills of Kentucky!

I suppose that I don't need to bring up chaos (although I don't see how your "redundantly constrained limit cycles" obviates chaos), but it would help. Stephen J. Gould in his book _Wonderful Life_ makes a strong argument for the indeterminism of evolution; evolution is _contingent_ on history, but history doesn't _determine_ the course of evolution. He never mentions chaos, but he should.

I don't see how evolutionary processes depending on blind variation and retentive selection (from biological evolution to PCT reorganization) can be considered deterministic. Tiny initial differences can make huge differences later on. If the first chordates had all been wiped out by parasites or a meteor hit, we wouldn't be here. If my Dad had not been introduced to my Mom by the only friend they had in common who just happened to me in town for the weekend, I would not be here, CSGnet might not be here (yet), and we would not be having this discussion. While I think that Gould's "lottery" model of evolution goes a bit too far (survival of the luckiest), his point is important.

But that does not mean that all evolutionary processes must necessarily diverge. Wings have evolved several times independently (insects, pteradactyls, birds, bats, flying fish). Just about all children in a speech community learn the language of the community, but they all get there through a slightly different path.

Of course, when we look back at a process of evolution (either biological or psychological) when will be able to trace the path by which A lead to B lead to C, etc., and all this may seem in retrospect quite deterministic. But this is definitely not the viewpoint that we have at A. If the higher references levels are a function of evolution and reorganization, then I can see no way in which they can be considered DETERMINED in any useful sense of the word.

--Gary

P.S. Here are two observations of mine which I find intriguing. (a) Biological evolution has no purpose, no grand design; and yet it has resulted in entities (organisms) which have purposes and grand designs. (b) Evolution is also a chaotic process in that tiny differences at one point in time can make a big difference later in time; and yet evolution has resulted in entities which can achieve repeatable ends in spite of quite large differences in both initial and accompanying conditions (i.e., disturbances). Nonpurpose begets purpose; chaos begets control.

Gary A. Cziko

Date: Mon Jan 13, 1992 7:56 pm PST
Subject: Social vs. physical something-or-other.

From Greg Williams

>[Martin Taylor 920113 18:00]

>I think that the idea of "social control" implies something different from
>the physical control of the body of another.

Fair enough. I now suppose that the unpacking of "you can't control somebody else except by direct physical force" is "you can't force somebody else to behave as they DON'T want to behave except by direct physical force." This, of course, leaves open the possibility of other kinds of social control (or, as some would say, influence, or pseudo-control).

Greg

Date: Mon Jan 13, 1992 8:05 pm PST
Subject: e- & p- control

Re Rick Marken (Tue Jan 14 05:24:15 1992)

>What is the 'ecological viewpoint'? If it's related to Gibson's theory of
>perception and Turvey's theory of action then I think that there are
>serious areas of conflict with PCT. If it's just a viewpoint that says
>What is the 'ecological viewpoint'? If it's related to Gibson's theory of

>perception and Turvey's theory of action then I think that there are
>serious areas of conflict with PCT.

It's my term for what I take to be the viewpoint of people studying how creatures get along in their environments, and also a lot of people working in natural language semantics these days. Gibson has been a significant influence (especially in the `situation semantics' of Barwise & Perry), but there are also some independent sources in linguistics, such as David Dowty's story in his book on Word Meaning in Montague Semantics to the effect that what model theoretic semantics is trying to do is explain what it is for language use to be successfull.

I personally have found Gibson appealing in certain respects, quite obscure in others, and what little Turvey have looked at, incomprehensible. I am not attempting to claim that what these people say is overall consistent with PCT, but rather suggesting that recognizing environmental variables would make PCT more accessible to people who have been brought up on this kind of stuff (e.g. those of my colleagues are into model-theoretic semantics).

>If you want to call some controlled
>variables (like distance between you and a lion) ecological, then OK, but I
>don't see what it gets you.

I think you're missing the point here. The `ecological variables' are not certain of the perceptually controlled variables (which are internal to the organism, manufactured by its perceptual system), but are out there in the environment (I hope this answers Kent's question about the location of variables). Of the myriads of external variables that one might choose to recognize, a few are actually relavant to the viability of the organism. These are the ecological variables (e.g. the currently popular style of T-shirt in Arnhem land is an environmental variable for most participants in CSGNet, but not an ecolotical one). Some of the ecological variables have perceptual systems that are `attuned' to them, so they get e-controlled (read `e' as `external' if you don't like `ecological') via p-controlled.

Notice that when one tests for control, what is actually observed is e-control by the organism: p-control is then inferred as the explanation, since that's the only way we know about or can even semi-plausibly imagine that the effect of p-control can actually be produced.

>Control theory recognizes that living control
>systems MUST control certain variables (Bill called them INTRINSIC variables)
>or die. Those variables are not things like "distance from lion" but things
>that are influenced by attacks by lions, among other thing -- these are
>variables like blood sugar level, blood pressure, pH level, O2 levels
>in the cells, etc. The model says that these variables are controlled
>indirectly -- by controlling perceptual variables that influence them.

Your `indirect control' concept here looks like my `e-control'. Of course some of these internal variables, such as blood pressure, are subject to relatively direct control via sensors for them, etc., but these systems can only work within narrow limits, and if various special conditions are met: the blood-pressure control system can't work if there's a hole in the aorta. But to maintain the essential internal conditions for life, one also has to maintain various external conditions (I'd be happy to replace e-control by `maintain', except that the latter word as an inappropriate static component to its meaning). I would claim that `control' over these external conditions is indirect in the same way as you suggest that control of the intrinsic variables is.

But I also think that `indirect' vs. `direct' control is a more confusing terminology than what I'm suggesting, partly because the absence of a bit of exotic linguistic material (`p-' vs, `e-') makes it less likely that people will wake up and notice that the words are not being used quite in their ordinary senses.

Avery Andrews

Date: Mon Jan 13, 1992 8:28 pm PST
Subject: Premises to stand on

Ken Hacker [920113]

In response to Bill Powers:

1. Bill, I am disappointed that you single out social sciences for bad research. C'mon, let's be candid about research in general. Don't make me name names of physical scientists who have fabricated data or others who have published nonsense. This is not a level of debate I wish to engage in. So, yes, social science has problems, like ALL science has problems, and of course, needs more rigor, maturity, etc. etc. (obvious).

2. You say that "NO behavior is externally guided." I argue that all ALL behavior is externally guided, but that none is fully guided or controlled (in all senses of the term).
   Guidance is not to be confused with control, nor should influence and persuasion be confused with control.

3. I agree that social situations act on us only through sensory inputs. That is basic physiology. What is more interesting is how those sensory inputs and mechanisms are affected by thinking and feeling which has multiple origins, including social interactions.

4. I agree fully with your claim that "action is not driven by perception, but by the difference between what you are perceiving and what you want to be perceiving." Given that, we need to specify how I ever come about making one comparison conclusion over other possible ones.

5. "Reference levels relating to these perceptions" are not black boxes. We can identify what they are and how they are developed.

6. You are correct in stating that nothing CAUSES us to act. However, there are many things which IMPEDE us from acting. Ex. I want to be a basketball player and I am 4 ft. tall. Ex2. You wish to be President and you are a Communist.

7. You argue that the "outside world can affect you physically and can present you with the raw materials for perceptual interpretations." PRECISELY!! The social interactions we have, in particular, provide not only with feeback, but with models for making perceptual comparisons.

8. Your claim that "what we choose to experience is determined is determined by us.." is tautological. What we actually experience as we move about is often random, never chosen by us. Ex. We are driving down the street and another car hits ours. This was not our decision! How we interpret and react to certain are. The point about social influence is that how we interpret our experiences is not purely a self-reflexive process.

9. You say that "Our goals, insofar as they have anything to do with society, are chosen from our perceptions of what social happenings are possible, knowledge which we accumulate through observing and interacting with other people." YES! We choose among perceptions, always having an array of possible ones, yet those perceptions are related to our interactions with sociey and with others. And how much of our goals having NOTHING to do with society?

10. Archimedes argued, "Give me a premise and I will deduce the world!" I hope that you are arguing something much different and that the contruction of control and perception control theory will begin to include propositions which account for the social nature of human being and social adaptation.

I could go on, but I do need to write some lecture notes about more mundane matters. Best, KEN

Date: Mon Jan 13, 1992 9:36 pm PST
Subject: recursion

Operator grammar still looks recursive to me: to integrate a word and its dependents into a structure, you have to find the dependents of the dependents. In the case of central recursion, you have to in effect put the search for the dependents of one word on hold while pursuing the search for those of another. `Edge-recursion' can be processed without the use of stacks, etc. in either dependency or PS grammar, central recursion seems to require them.

On reductions: what's the source of something like:

everybody/nobody who saw the play liked it.

Avery Andrews

Date: Mon Jan 13, 1992 10:05 pm PST
Subject: social control

A point I'd like to make on social control is that it does not seem very likely to me that the setting of high-level goals in humans has much to do with with the satisfaction of basic intrinsic ones (if it did, how could tribes get warriors to die for them?). Rather, I think that people are predisposed to pick up the high-level goals of the people around them in an extremely unselective and unreflective way. Some people, but by no means all, develop to the point where they can sift through this junk and make appropriate selections, but it is originally just sucked up from the local social environment (perhaps by a system whose goal is `find the prevailing high-level goals').

This mechanism worked pretty well in close-knit traditional societies with a stable technological base, but produces a lot of wreckage under contemporary conditions, where virtually none of the currently popular high-level goals have been adequately tested, and things change to fast for things to settle down in a reasonable way.

Often, the failure of the mindlessly assimilated goals to lead to satisfaction of the intrinsic reference levels leads to some degree of reorganization, but it seems to take several decades for this to happen, when it does at all (do Japanese middle-managers have midlife crises? I get the impression that they tend to drop dead first). In the interim, I would say that there is quite a high degree of the sort of social control/constraint of the kind that I take Ken Hacker to be talking about, though this is not PCT control (there aren't sinister controllers out there ruthlessly fulfilling their aims through us, but just a soup higher-level goals spreading from mind to mind).

Avery Andrews

Date: Tue Jan 14, 1992 2:53 am PST
Subject: Social control (an annual provocation)

Observations:
1) There seem to be some collections of chemicals in certain configurations. They behave in such a way that can be interpreted as sending signals, affecting each other, and trying to fullfill the goals of an abstract entity called a 'cell'.
2) Some collections of the so-called cells (with specilaized types, etc.) seem to be behaving as if to maintain the goals of a higher-level organizational entity called an "animal" or a "person". Many theories try to explain how the low-level behavior of those "cells" (and especially "neurons") realizes the "goals" of the more abstract entity. Btw, theories are construts of the peculiar "consciousness" phenomenon which seems to be realized at the top of these so-called persons.
3) Some spatio-temporal collections of those "persons" form even more abstract entities such as "the Roman empire", "the jewish people", "the academic world" "ideas", and "ideologies". A high level observer could identify goals, signals, and control in the behavior of such entities, as realized by the behavior of those "persons". Unfortunately, our observers are looking only downwards, and find the idea that they, with all their complexity, and autonomy, are realizing a higher-level system, in which they are no more than cells are to the body or molecules to the cell.
Of course the control of such a high-level system cannot be explained in termes of a simple servo loop, but neither can any non-trivial level in the intra-personal hierarchy.

--Oded

Date: Tue Jan 14, 1992 6:22 am PST
Subject: subjective probability

[From: Bruce Nevin (920113 1752)]

(Martin Taylor 920104 20:50)

>Bruce asks about the relations among subjective probabilities,
>the likelihoods of different percepts and the error signal of a
>control system.

Actually, I asked "what do you mean by `subjective probability.'" Looking back, I see in your (920103 1715) "all probability must be subjective" because there is no viewpoint apart from that of the observer, and "it is indeed hard to separate the notion from that of degree of belief." What I am asking for is the derivation, status, and application of "subjective probability" within a control-theoretic model. Is it a perceptual input p into some elementary control system (ECS) that controls for belief in some other perceptions? In CT terms, what is it?

Now let me see if I understand what you said in yesterday's post, (920113.1330).

The precision with which the comparator in an ECS can "measure" the input perceptual signal p against its input reference signal r is limited. A range of input values p<i>-p<n> may be accepted as equal to the reference signal. I take this from paragraph 4 after your mark
==========

This degree of imprecision you call the "subjective probability distribution over the possible underlying `true' values of the perceptual input." (Same paragraph.)

Before "looking at" an input signal p1, that imprecision is the range p<i>-p<n>. Then after input of p1, the memory of p1 is more precise within the range p<i>-p<n>. It is a smaller range p<j>-p<k>, more precise, but still with that non-Newtonian indeterminacy. I get this from:

>After observing the value, the node has a different subjective
>probability distribution for what the value was.

I say "memory" because you say "was." (You are talking here about a node in a multi-level perceptron. If there is some essential difference between the perceptron and a control system such that I am missing your point, then you must clarify the analogy you have made between a control system and a system that lacks comparators, reference signals, and error signals.)

You talk about how the reference signal r is also indeterminate, and you talk about increased indeterminacy in the relations of ECSs at multiple levels. I agree. I will limit myself to one

ECS here. You say:

> there is a subjective distribution of reference levels to be
>considered when the ECS evaluates the likelihoods of future
>percepts.

Here, you seem to be saying that the ECS perceives the likelihood of a percept--that the perceptual signal p of a particular value and the likelihood of its having that value are both inputs to the ECS. But the percept and the likelihood of the percept (or the indeterminacy of it, which is I guess what you mean) are things of different logical type. If anything "evaluates" the likelihood or the indeterminacy of the signal p input to our ECS it could only be the comparator of some other ECS, for which that indeterminacy itself was made a perceptual signal p' --somehow. This is the only sense I can make of (second paragraph after your mark):

>In principle it would be possible for a device looking at the
>output of any node to estimate the relative likelihoods over
>time of different outputs, and to estimate those likelihoods
>given recent history, or given values at other nodes, or given
>any other information it had access to.

There are only three signals in an ECS: p, r, and e. What signal in an ECS corresponds to the likelihood of a particular value of p? If there is some device in the hierarchy that notices the range of indeterminacy for the sensory input signal p for our ECS, how can it determine that some particular value is more likely than some other?

This device would have to stand outside the ECS (or outside the perceptron node). So it seems that some other meta-ECS must be comparing the present and prior indeterminacy of p input to our ECS. The difference, the output e, is not an error signal in this case, but a measure of the amount of information received. "The input p1 into the ECS I have been monitoring has enlightened it (reduced its uncertainty) by this amount."

I will stop here for a check of comprehension before going on. Am I still "mired in misunderstnadings," Martin?

Bruce
bn@bbn.com

Date: Tue Jan 14, 1992 6:33 am PST
Subject: Transpers; Stats; Social control

[From Bill Powers (920114.0800)]

Martin Taylor (920113) --

This is taking over our lives, isn't it? I keep thinking that I have to get on with projects that are suspended, and every day I get this jolt of intellectual electricity than sends my mind racing again. Evidently I'm not the only victim, though. I don't know how much longer I can go on at this pace. I never thought that when I entered retirement I'd start looking forward to a vacation from it.

I am vastly relieved at your post on transpersonal control systems. I should have known better than to misunderstand you.

>I'd call the cooperative situation "transpersonal control" since it
>involves a control loop that incorporates the control systems of more
>than one person, even though from the viewpoint of each, the other is
>only an environmental disturbance.

That's it. "Transpersonal" control is control that is carried out partly through another person or persons. A new name for sociology? "Transpersonal controlology." Well, I guess not.

Greg Williams (920113 and previous) --

I think the hangup here begins with your statement that one person can arrange to control another's perception so that BOTH people are controlling the SAME perception. I want to perceive you in this room, and you want to perceive you in this room. Since we use the same term for both perceptions, "perceive you in this room," they must be the same perceptions. Well, not quite.

In the first place, the perceptions are in different heads. In the second place, they are derived by private and possibly unique means from the raw sensory inputs available at the locations of the two people. And in the third place, they are in different hierarchies with different goals at the same and at higher levels. I may be satisfied that I have caused you to control a perception just like mine relative to the same reference level I am using. But that's my conceit, not a fact. However, I'll play the game.

The cut-finger ploy is a little artificial, but how about the ploy in which the aged momma keeps demanding help and attention and threatens to have a heart attack when the dutiful daughter, who has given up independence and marriage for 40 years, tries to do something for her own benefit? Aged momma is playing on the daughter's sense of love and inferiority and guilt, and it works. It can work for the daughter's whole life if she finally collapses and dies in harness. So isn't AM controlling her daughter's perceptions, making her daughter seek the kinds of perceptions that momma wants at the reference levels that please momma? Indeed she is; her control is working.

This sort of story makes us very irritated with momma and her selfishness. But doesn't it suggest something less than optimum about the daughter, too? It takes two to play this game. If momma were a drunk or a junkie, the daughter's problem would be called co-dependency. Nowadays a friend or a doctor would strongly suggest joining a support group which would bluntly point out to faithful daughter that she's doing it to herself. If FD gets the message, she will realize that by controlling for a placated momma, she is helping to keep momma frail and in bed (or in a stupor) and that no human being owes it to another to play the role of slave. It's not doing a favor to either party.

Just turn on your TV if you want to see examples of people trying to control other people by manipulating their perceptions. I refer either to the campaign spots or the commercials. Elect me and I'll put America (you) back to work. Buy our cars or drink our beer, fella, and you'll have a front seat full of T&A. This works (although not as wonderfully as ad agencies like to persuade their clients that it does -- another example). Despite the fact that most candidates do not get elected and despite the fact that most nerds drive around or get drunk mainly alone, there is a certain degree of success with this method of control. But just as with AM and FD, it takes two to play the game.

Why do you go to the aid of your friend with the practically amputated finger? Partly because you like him. Partly because you're concerned for the rug in the other room. Partly because you feel guilty about a lot of things, probably about not really liking this person as much as you are convinced you ought to. Partly out of principle: you don't turn your back when someone is really in trouble. So far so good.

But what about the second time, the third time, and so on? After a few false alarms, if you keep on falling for these little deceits, you have to face a problem: this person is obviously trying to control you, and you're playing along with it. Why? Could it be, just possibly in some remotely conceivable way, that you're getting something out of it? Could you be feeling a bit superior to this transparent liar, this pitiful creep, this silly manipulator? Could all this be giving you a feeling of (shhhh!) control?

Do you really think that a viewer hearing effulgent promises from a candidate swallows every word whole? That Joe Nerd really believes that a car or a bottle of beer will fulfill his wistful fantasies? No, the viewer or Joe Nerd understands at some level that he or she is being lied to. But he or she also understands that it would be very, very nice if what he or she is hearing were true. So the viewer and Joe Nerd both do the same thing: they imagine that what they are hearing is true, and they experience the joy of knowing that their problems are about to end. Because that's what they want to experience.

Whose reponsibility is it to check out information that arrives by the senses: the sender, or the receiver? Who is it who is in a position to tell the difference between actual satisfaction and imagined satisfaction? Only the person who seems subject to the control of another can do these things. Nothing much can be done to prevent people from trying to deceive you or fool you. But everything can be done if you simply admit that you can be deceived and fooled, and turn your attention to the inner conflicts that make you play the part of the victim and refuse to admit you're fooling yourself.

If you were in the presence of a person who knew the difference between perception and imagination and who didn't accept important statements without some verification, you could still lie to this person and falsify perceptions and gain some control. But it would be a very tough task in comparison to the way it would be if the other person cooperated. You would have to be a perfect liar.

A perfect liar, it is said, has to have a perfect memory. But it's worse than that. When you fool around with someone else's perceptions, you have the problem of maintaining consistency. You've created an apparent environment, a little bit of one, in the middle of a huge set of perceptions that the other person is getting independently of you. If you say, "Yeah, I washed the car," but you haven't, you then have to think up lie after lie to keep this person from going to the window from which the car can be seen. One thing leads to another; the world of perception is very large and very interconnected and above all, people demand consistency among their perceptions (or at least those who know the difference between perceiving and imagining do). Keeping control of one of these tough cases in this way quickly becomes a full-time job, and it's basically impossible.

But I admit that for a time, in the short run (by which I mean years) it's possible to maintain control of a sort by manipulating other people's perceptions, particularly when they cooperate. In the long run, however, it will all fall apart.

I'm not satisfied by waiting for the long run, though. What I hope will come out of an education in control theory is that everybody will become tough cases. Being controllable (by means other than overwhelming physical force) is something the victim, not the perpetrator, does. This state comes from insufficient grasp of one's own nature, one's own inner organization. It comes from being stuck at too low a level of awareness in the hierarchy. It comes from desperation and grief, from extreme poverty and pain. It comes from wanting to blame others when things go wrong (you were the one who said I should bet those Kings). These are all things we can do something about. We can do something about the external situation, and about the internal competence. So if control theory has the effects I think it is going to have, the short run may become shorter.

I think that when you imagine how one person can control another, you're making the controllee much too passive. Passivity isn't the natural state of a human being, or any animal. Even a protozoan -- when you see it become passive, you know it's dead.

Get a haircut.

Chuck Tucker (920113) --

>Re: Transpersonal Control
>
>If you mean that 2 or more persons are controlling their own conduct
>to accomplish a goal (like moving a table across the room ...

There's more to it than that. The doctor holds out his hand and says "scalpel" and the nurse slaps it into his grip: that's transpersonal control, too. The doctor, who can't take his eyes off the work he's doing, uses his voice to get a scalpel in his hand. The nurse, a willing participant in this transpersonal control process, already has his hand on the right scalpel and prides himself on knowing when the command will be given. There can be great beauty in transpersonal control processes, as people move wordlessly through their tasks, using other people and being used in turn by them with only a sense of competence and enjoyment -- never a shred of resentment. This transcendant relationship between people is often imitated (team spirit) but not so often achieved. The memory of moments of perfect intermeshing and trading of control is what keeps grown men playing games with leather balls.

Martin Taylor (920113) --

Re: subjective probability.

A brilliant exposition. I understand now why you have said that your work is complementary to control theory; it surely is. I think you're really involved in the nitty-gritty of modeling, preparing the way to go one level deeper into the explanation of how the system works. Some of your statements about probability belong to a metadiscourse -- to that little group of modelers standing off to one side examining this here hierarchy *as modelers*. This is different from imagining that you are the system, and trying to see what that system would be experiencing, my major approach. Sometimes, I believe, you tend to blur the boundaries between the two modes of analysis -- but who doesn't? This is difficult work and you're bringing a lot of order into it.

I especially loved your strategy of eliminating the comparison and output sides, and identifying what was left as a multilayered perceptron. I don't know of anyone working with perceptrons who has considered that *each layer* may have significance of its own, rather than being merely part of the machinery producing the ultimate output. Of course when you bring the control loops back in, the utility and experiential significance of the intermediate layers becomes obvious. Everything you say about the complications and difficulties that beset the hierarchy rings a bell; we never do stop trying to resolve these problems, and I doubt that we ever fully succeed. This is not some perfect clockwork mechanism, but a living system seeking solutions for problems at the very edge of possibility.

One suggestion: the concept of likelihoods doesn't seem to translate as neatly into neural functions as the concept of subjective probabilities. I think that likelihoods, which suggest rather sophisticated extrapolations into the future, belong to the metadiscourse more than the analysis of perceptual functions. But if you can find some exposition as elegant as the one in which you show the tradeoff between sampling interval and precision, I'm ready to be convinced.

Re: recognition of sequence.

If you look in BCP, p. 144, you'll see one design for a neural sequence-detector that has some of the properties you mention as desirable. It's aperiodic -- the timing of the occurrance of the elements doesn't matter. it can be thought of as outputing a signal when the final element of the sequence is complete, or by summing intermediate signals, as providing a signal that increases as the sequence progresses toward completion. It's also possible to reset the entire detector at any time just by inhibiting the initial neuron. Although I didn't say this in the book, this design also lends itself to compact recognition of related sequences, in that the signals at any stage can branch off to become parts of other sequence-detectors with the same initial elements. A "heterarchy" -- but all at the same level.

I'm not very impressed by the "similarity and distinctness" stuff. Match and mismatch, it seems to me, are adequately handled by the idea of an error signal and the actions it implies (or does not imply, when zero). Your concept strikes me as proliferation of entities.

Greg Williams (920113) --

>If the highest level is CONSTRUCTED in development, investigating its
>determinism would be very interesting. You see, Gary has (had?) the idea
>that what saves CT from determinism is going up. But if the top is
>historically determined ...

Determinism is an interesting question only if it promises to reveal observable determinants. To say that the highest levels are "historically" determined is probably acceptable, in that the intrinsic reference signals that guide reorganization must be inherited, and thus the product of a long history spanning billions of years. But what good does that do us? We might as well say that behavior is caused by the Big Bang, so it's deterministic.

In fact there has to be a large nondeterministic component in behavior, else reorganization wouldn't be as powerful as it is. The whole point of a reorganizing system with a _random_ output is that it can find a solution for a control problem, if one exists, without any preconceptions about the nature of the local environment. You can say that this randomness generator is a product of evolution, and I will agree. I will go further; I think it has been active since the beginning of life, operating on much the same principle. We call it "mutation" as if it's something that happens to an organism. But I think it's the essence of life, when coupled to a control system.

But to say that a nondeterministic system is a product of a history of development doesn't make the nondeterminism deterministic. I suppose that like any pseudorandom sequence generator, reorganization isn't truly, at the atomic level, random -- except that if you carry the analysis far enough, and believe the quantum physicists for the time being, the nondeterministic system is only roughly, statistically, deterministic.

Anyhow, knowing that in principle there's determinism behind human development may warm the hearts of those who want determinism, but it doesn't settle any questions scientifically. We can claim to understand only those processes whose workings are open to observation. "History" is not open to observation; only to conjecture. If anyone comes down firmly on the side of determinism (or against it), you can be sure that doing so accomplishes some purpose other than scientific enlightenment. The status of determinism is of a such a nature that we can't determine it.

>I don't see overwhelming physical force as necessarily doing ANYTHING to
>the highest reference signals of the "controlled."

Have you ever felt overwhelming physical force? Coercion that goes implacably beyond the limits of your most extreme attempts to prevail and resist? I think it is the most profoundly disturbing thing that can happen to a human being, and possibly to an animal, at every level. Those who have been through it are changed by the pain applied to them and created by their attempts to preserve themselves, by the rage and the fear and the shame. They reorganize; nothing else works. Everything changes, until finally they submit, or die, or win.

Overwhelming physical force is a nice abstraction. Experiencing it is something else.

Date: Tue Jan 14, 1992 8:24 am PST
Subject: recursion and stacks

[From: Bruce Nevin (920114 1019)]

Avery Andrews (14 Jan 1992 16:28:08) --

>Operator grammar still looks recursive to me: to integrate a word
>and its dependents into a structure, you have to find the dependents of
>the dependents. In the case of central recursion, you have to
>in effect put the search for the dependents of one word on hold while
>pursuing the search for those of another. `Edge-recursion' can be
>processed without the use of stacks, etc. in either dependency or
>PS grammar, central recursion seems to require them.

In the pandaemonium model we have been considering, a word recognizer requires input not only from recognizers for phonological elements, but also inputs from recognizers for those classes of words that must be available (perhaps in modified or zeroed form) as arguments of the word. The continued activation of a word recognizer that has its phonological inputs satisfied but not all its dependency inputs constitutes a kind of stack, I suppose. But the stack is only one element deep. It simply ignores what doesn't fit, and if unsatisfied long enough the reactivation from memory (memory of the inputs that were satisfied) dies out, and that's just a bit of what was heard that remains not understood. Happens all the time.

A PSG treatment requires recursion many levels deep.

>On reductions: what's the source of something like:
>
> everybody/nobody who saw the play liked it.

Something like the following. See _GEMP_ for details. Could send you my paper on unbounded dependencies for other examples. Donkey sentences are especially fun.

For the definite article:

   someone saw something which is the same thing as mentioned
   someone saw that; that is a play
   someone saw that which is a play
   someone saw the play

For the relative pronoun and "it":

   someone--said one saw the play--liked a thing; said thing same as mentioned
   someone who saw the play liked it

For the quantification (given somebody is an alternant of someone):

   somebody who saw the play liked it and somebody who saw the play
   liked it and somebody who saw the play liked it, mounting to
   everybody

   somebody and somebody and somebody mounting to everybody who saw the
   play liked it

   everybody who saw the play liked it

All of this is supported with much greater detail in _GEMP_.

BTW, I can't send you Stephen's dissertation (or my paper) to Australia on BBN's nickel, and I am nickelless. How badly do you want it? I could split the postscript file for my paper up into mailable chunks and send it, and you could reassemble it and print it on any printer that can take MicroSoft Word's postscript files.

Bruce
bn@bbn.com

Date: Tue Jan 14, 1992 8:49 am PST
Subject: everything

[Joel Judd]

The server is down one day, and I'm greeted by no less than *32* posts this morning.

Ed,
Congratulations also--I'll be looking forward to seeing the program.

Dag,
Could you zip me your e-mail address? I deleted your message before copying it.

Martin,
Received the manuscript.

Joel,
Yours is in the mail.

Avery, Bill, Ken,

Basketball has returned! It must be that time of year. Last year it was employed to discuss learning and reorganization. Anyway, Ken's mention of things IMPEDING control sound like Bickhard's "selection pressures." Interestingly, there seems to have been some modifications to b-ball to allow for someone 4' 3" to have a chance--the 3-point line! Now if you can't work inside and slam dunk, there's still a place for you.

I don't think there's any doubt that the environments we have to choose within lead to our [individual] perceptions. The problem is, as discussions on statistics have pointed out, that many feel they see too much similarity among members of a delimited society, or look at similarities where they are either trivial or useless.

After reading Bill's comment on "having the same perception," I don't feel so uncertain for having turned in a chapter where I say that even though most people learn multiplication tables or to ride a bike or other things, they've all learned it individually. Hugh Petrie has also mentioned that the nature of the task constrains the learning outcomes (what's observed). The more objective the outcomes, the easier to test (typing speed, for example). But it is still the case that the individual had to develop the perceptions involved in the behavior observed.

Re: the hold-up scenario--there's plenty of anecdotes which can provide an alternate outcome: What happens if the thief does this to a 2-year old (or would he even bother)? What if the person is deaf and blind, or mentally retarded, and the situation has no significance? How many times have we seen in a movie someone feign some sort of mental deficiency to get out of a similar situation? Or what if this happens with someone from--I don't know--the highlands of Papua New Guinea, and the person doesn't even know what a 'gun' and 'holdup' are?

There really needs to be some epistemological propositions laid out which would help to clarify control of what by whom and why. Elucidation of development of the control hierarchy clears up some of the problems of immediately dealing with mature ones. Children seem to resist yet utilize the "selection pressures" of home and family and school. How often have we heard the following anecdote (or some version thereof):

Teen A: Hey, I gotta go--it's almost eleven.
Teen B: You mean your parents make you be home before midnight?
A: Yeah--don't yours?
B: No way! I can stay out as long as I want!
A: You can stay out as long as you want? Man, I wish my parents gave me that kind of freedom!
B: Naah, no you don't. Your folks really must care about you to give you rules...
etc.
What kinds of higher-level perceptions are being developed here?

Date: Tue Jan 14, 1992 9:25 am PST
Subject: social behaviorism

[from Joel Judd]

Ken, Bill, Ed, David,

Right after signing off, this front page article caught my eye. It deals with disturbing issues, most notably, the "vicious circle" so often referred to in reference to welfare families.

Illinois is currently in deep trouble financially (who isn't), an the Governer is insisting on cutting $350 million--no new taxes, no more borrowing. Yesterday the general public was allowed to testify, and the following woman is mentioned:

"Linda Haley, a member of the Chicago Coalition for the Homeless who was protesting cuts to welfare programs, testified she was without shelter for 41/2 years and was forced to live a life of shoplifting and prostitution to survive. 'That's what's going to happen to these people if these cuts go through,' she said, fighting back tears."

There you have it--environmental control. She was FORCED to become a prostitute.

Right?

Date: Tue Jan 14, 1992 10:28 am PST
Subject: I can't believe it but I will

<<<<<< FROM CHUCK TUCKER 911014 >>>>>>

Dear CSG'ers,

Did anyone notice my date formatting error; I used MMDDYY instead of YYMMDD in my two previous posts - sometimes there is a delay in noticing an error.

I have not caught up yet but it seems that I am in a position of arguing against the "social" of control and for "influence"; what a strange position to be in. I will write more later on this point.

I wish to remind all of you that what you write may appear in a paper to be presented at the SSS meeting this Spring since the autor of the paper on "The Myth of Social Control" is this NET. Of course, each of you will be ample opportunity to edit any statment you might have made here.

I agree with anyone (especially Ken Hacker) that the model needs development and serious testing and that much of what is written on this NET about the model is speculation and possibilities but I will repeat my statement of on sociocybernetics: Society, social structure, social class, culture, group pressure, personality, socialization , social background, biological agents, chemical agents, technology, social norms, rules, values, beliefs, customs, traditions, laws or social sanctions DO NOT MAKE PEOPLE DO ANYTHING rather they are constructed by people to make social life possible.

Sorry to be so brief but I must go and tell some students about this view.

Best to all,
                         Chuck

Date: Tue Jan 14, 1992 11:23 am PST
Subject: Social Control, Ecology

[From Rick Marken (920114)]

Ken Hacker [920113] says:

>comparisons made between desired states and experienced states do not arise
>independent of social INFLUENCE. Moreover, that influence is nontrivial.

Is this an assertion about how the control model works? If so, it is clearly false. Is it an assertion about how the control model should be changed? If so, why? What data requires such a change? In terms of the model, what is social influence? A signal, a variable?

>   PCT is interesting; it is not
>yet perfected. The potential is wonderful and Bill is a source of wisdom and
>fascinating ideas. But let's not confuse what is good with what needs more
>work.

What is "good" about PCT is the basic concept of behavior as the control of perceptual experience. Of course it needs more work; but all the work in the world won't change that fundemental insight. Your statement above stikes me as something someone might have said to Copernicus -- it's good (the sun centered concept) but it needs work (it did, Copernicus didn't get the eliptical orbits, for example). But, like PCT, the problem for the helio-centric model was not a result missing details. The establishment objected to the central (literally) notion -- that the sun rather than the earth was at the center of the "universe". The problem for PCT is also not with the details: establishment life scientists just don't want to get the basic concept -- that the behavior of living systems is organized around the control of perception. The anti-copernicans didn't want "man" displaced from a central role in the universe. The anti-PCTers don't want to abandon the idea that behavior is caused output; they don't want to because it would mean abandoning the foundation of their discipline and their methodology. Quite an unpleasant change -- almost as bad as finding out that god put you in an inconspicuous corner of the galaxy (maybe he has someone else he likes better?).

Ken Hacker [920113] says, in response to Bill Powers:

    >Bill, I am disappointed that you single out social sciences for bad
    >research. C'mon, let's be candid about research in general. Don't
    >make me name names of physical scientists who have fabricated data
    >or others who have published nonsense. This is not a level of debate
    >I wish to engage in.

You missed the point. Social science research is not "bad" because the researchers are cheating. It's bad because it is based on the wrong premises. Acceptance of these premises leads to the conclusion that "significance tests", correlations as low as .8 and conclusions about individual process based on aggregate data are a reasonable approach to studying behavior. The control model (and Phil Runkel's book) explain why this approach to research produces "garbage" with the utmost honesty.

Avery Andrews says:

>I think you're missing the point here. The `ecological variables' are
>not certain of the perceptually controlled variables (which are internal
>to the organism, manufactured by its perceptual system), but are out
>there in the environment (I hope this answers Kent's question about the
>location of variables). Of the myriads of external variables that one
>might choose to recognize,

Hmmm. How do we recognize these variables without perceiving them?????

>   a few are actually relavant to the viability of
>the organism.

>the ecological variables have perceptual systems that are `attuned' to
>them, so they get e-controlled (read `e' as `external' if you don't
>like `ecological') via p-controlled.

Explain how e-control works without p control. You will have to design a control loop that controls an environmental variable WITHOUT PERCEIVING that variable (after all, you want to control just that e variable out there. I look forward to seeing the equations of this novel control system.

>Your `indirect control' concept here looks like my `e-control'.

Nope. My (well, Bill's) indirect control system (the system that changes the structure of the hierarchy of controlled pereptions in order to control intrinsic variables) controls perceptions (like all control systems) which, in-cidently, are very likely to be of variables that you might not want to say are "really" out there -- like non-linear combinations of chemical concentrations -- derived measures of whatever you want to imagine are "real" (e) variables. In my indirect, intrinsic variable control system, what is controlled is defined by the functions that determine the perceptions of what we call intrinsic variables.

>A point I'd like to make on social control is that it does not
>seem very likely to me that the setting of high-level goals
>in humans has much to do with with the satisfaction of basic
>intrinsic ones (if it did, how could tribes get warriors to die
>for them?).

I think that intrinsic variables are not all "vegetative". PCT people have done little research in this area. Right now, the concept of intrinsic controlled variable is "ultimate goal" -- and "staying alive" is obviously not one of them. I think we stay alive as a result of the fact that all the intrinsic variables are kept at their reference values. But there is obviously not an intrinsic variable that prevents you from taking life threatening actions (if there were there would be no suicide or, as you note, WAR)..

Still, without the concept of intrinsic variable in the model I don't see how you can account for the fact that people seem to settle for some behavioral organizations at some time and different ones at others -- There may be no obvious relationship between a higher order variable (like being catholic) and some intrinsic variable (like CO2 level in the blood). But we don't know what all (or even some) of the intrinsic variables might be. The clinicians talk about intrinsic variables like "belonging". It is easy to imagine a system that computes a function of physiological and external variables that can be called "belonging" . It is also easy to imagine a genetically determined intrinsic reference for such a variable (evolution might have liked people who were social but not TOO social). I can imagine how selection of certain system concepts could have an influence on that variable -- a different influence depending on the social context into which you happen to be born. I can't model it right now (well, I can in the abstract) but I sure can imagine it.

You say:

            Rather, I think that people are predisposed to
>pick up the high-level goals of the people around them in an
>extremely unselective and unreflective way.

I just can't buy it -- this is the ultimate S-R notion. It says that you end up being who you are because you tend to respond by imitating who you are with. I agree that people imitate. But they also have criteria (the intrinsic references) for deciding if a particular way of behaving (of controlling certain variables) is OK. Imitation is a nice way to keep people from having to invent original ways of "being" every time they experience some intrinsic error. The imitations are likely to be successful to the extent that people are able to control their intrinsic variables in the same way; thus, imitating eating is a good strategy for kids. But imitations are likely to be unsuccessful to the extent that control of intrinsic variables requires actions unique to the particular individual. So if "sex" is an intrinsic variable, controlling it by imitating a sucessful "sex getter" who shows a "lot of leg" might work if you have great legs. Another strategy might be needed for others less well endowed.

It looks to me like people do "go along with the crowd" to a large extent;
but I also see very pronounded individual styles amongst people in the
same crowd.

Hasta Luego

Rick (Legs) Marken

Date: Tue Jan 14, 1992 11:30 am PST
Subject: Re: Determinism & Evolution

[Martin Taylor 920114 At home in a snowstorm that does wonders for the line!]
(Gary Cziko 920113.2010)

>
>P.S. Here are two observations of mine which I find intriguing. (a)
>Biological evolution has no purpose, no grand design; and yet it has
>resulted in entities (organisms) which have purposes and grand designs. (b)
>Evolution is also a chaotic process in that tiny differences at one point
>in time can make a big difference later in time; and yet evolution has
>resulted in entities which can achieve repeatable ends in spite of quite
>large differences in both initial and accompanying conditions (i.e.,
>disturbances). Nonpurpose begets purpose; chaos begets control.

I tried to make this point a few months ago. Self-organizing systems inevitably
develop in a string enough energy flow, in that structures that increase
the likelihood of similar structures being created will be the structures
that will be seen after some time has passed. Some structures, by fluke, are
more stable than others, and of these, those that emulate a control system
will be yet more stable. If and when a true control system emerges, it
will be even more stable against the buffeting of the variable environment.

So, control systems must almost inevitably emerge from an environment in
which a high enough energy flow is maintained for a long enough time, but
in which the environmental buffeting is not so large as to disrupt the
structures that implement the control. There is a certain delicacy, which
seems to limit the possibility of control systems using chemistry to a
relatively narrow range of energy flow. You couldn't have a chemical
control system in the solar atmosphere, but some other type of control
system might possibly emerge there. Who knows?

Martin

Date: Tue Jan 14, 1992 12:41 pm PST
Subject: Re: Transpers; Stats; Social control

[Martin Taylor 920114 14:50]
(Bill Powers 920114.0800)

>
>I'm not very impressed by the "similarity and distinctness" stuff. Match
>and mismatch, it seems to me, are adequately handled by the idea of an
>error signal and the actions it implies (or does not imply, when zero).
>Your concept strikes me as proliferation of entities.
>
Actually, it's not just my idea, and it comes from a variety of different

kinds of experiments with normal and brain-damaged people. There may be more to it than I have said, but I don't think that the essence will be much changed by more studies. Of course, in a complex subject, anything can happen, and new concepts can revolutionize the way we see old data. But for now, I'm convinced that there is a difference between similarity mechanisms and difference mechanisms, that they are done at least some of the time in different parts of the brain for the same comparison, and that as a rule similarity is quicker than distinctiveness in its operation. Also, more tentatively, similarity is less likely to have a conscious impact than is distinctiveness, which is why conscious detection of an ambiguity reduces or removes the effects of the multiple possibilities on subsequent perception.

Yes, it is taking over my life. I've got deadlines I am missing for papers on unrelated topics, and I am afraid I am going to have to cut back on contributions (and probably reading, too).

Martin

Date: Tue Jan 14, 1992 12:51 pm PST
Subject: Quantity of mail

I have been keeping all the CSG postings since about last March. Typically, the quantity has been about 600-700K per month. In the first 14 days of 1992, I have 731K, more than doubling the rate. I realize I have been responsible for much of this, either directly or indirectly, but really, it is becoming a bit too much. The topic is important and interesting, but we might be able to make our points more succinctly without losing their force, mightn't we?

Martin

Date: Tue Jan 14, 1992 1:11 pm PST
Subject: Re: subjective probability

[Martin Taylor 920114 15:15]
(Bruce Nevin 920113 1752)

\>
\>I will stop here for a check of comprehension before going on. Am I
\>still "mired in misunderstnadings," Martin?

To some degree, yes. Basically in assuming that there is something external to the ECS that evaluates probability for it. That evaluation becomes apparent in such things as the weighting and transformation functions of the perceptual input matrix, the dead zone (if any) of the comparator or output transform, and the like. The higher-level ECSs may incorporate much more sophisticated mechanisms, especially if inputs to the perceptual matrix are from above the category level, wherin logic is an acceptable way of manipulating them. As I said (to paraphrase) in the posting about which you are asking, the probability estimates at very low levels could even be built in by evolution, and might be totally insensitive to experience.

A more picky element of misunderstanding:

> A range of input values p<i>-p<n> may be accepted
>as equal to the reference signal.

Not so, if by that you imply that if the value reported as the signal p is between p(i) and p(n) then the comparator will say that the difference is zero. the p signal would have an effect indistinguishably different from that when the Newtonian God would have said it was 3.12566734239317... and it might even, because of the intrinsic uncertainty, have a greater effect even though it was "actually" smaller. We are talking here about distributions of probability density, not rectangular ranges with thresholds.

>
>There are only three signals in an ECS: p, r, and e. What signal in an
>ECS corresponds to the likelihood of a particular value of p? If there
>is some device in the hierarchy that notices the range of indeterminacy
>for the sensory input signal p for our ECS, how can it determine that
>some particular value is more likely than some other?
>

Another misunderstanding, or perhaps the same. There is, and can be, no such device. If there were, its subjective probability would not be that of the ECS, but would be its own.

Martin

Date: Tue Jan 14, 1992 4:30 pm PST
Subject: Re: Social Control, Ecology

Ken Hacker [920114]

Rick Marken (920114) --

Any assertions that human beings compare desired states with encountered states with no form of social influence are sheer poppycock. You need to to start thinking in terms of HUMAN processing of signals, which by the nature of our constitution, complexity, and interactions, has CONTENT other than gateway impulses. We are not capacitors; we are are cognizing and creative beings. KEN

Date: Tue Jan 14, 1992 4:44 pm PST
Subject: Quantity of mail

OK, everybody, I've got enough stuff on social control for the next CLOSED LOOP. You can stop now.

Just kidding

Greg

Date: Tue Jan 14, 1992 7:35 pm PST
Subject: squirrels running around elephants

Ken Hacker [920115]

Reflecting on the current stalemate over control vs. influence, I think three areas of control need distinction:

1. Motor and sensory control -- this is 100% PCT-as-stated.

2. Control over personal intentions and actions. This is not simple electrical circuits and gateways. This involves ANIMATE regulatory centers. We are much more than thermostats. And of course, our setpoints are not created by other people, HOWEVER, in our interactions with others, we can SET them to what conforms to what we have been convinced is the only or the most appropriate option.

3. Control in social systems. As the individual is a self-organizing system, so is an organization and a society. All three forms of systems have essential variables, regulators, comparators, disturbances, and hierarchies of control. Still, organizations cannot totally control individuals; instead they can influence choice consequences (Do this or get fired). And society cannot force or totally control its citizens although it can socialize us into believing half-truths and even lies which we then perceive and act upon as mediated by our personal control and choice-making.

Part of the social connection, i.e. communication, to control theory may be in the Law of Requisite Variety. As Ashby notes, the law states that the capacity of a regulator cannot exceed its capacity as a channel of communication. I argue that the success of humans in their naturally social ecology is based on their abilities to manage their communication and to identify what learning is producing what changes in their methods of regulation.

Date: Tue Jan 14, 1992 7:48 pm PST
Subject: e- & p- control; imitation

> Hmmm. How do we recognize these variables without perceiving them?????

We don't. But perception does not always succeed in discerning the variable that is the ecologically relevant one. The fly-detector of the frog is one famous example, here's a non-famous one. Consider a robot speargun with eyes mounted either size of the barrel. The ecologically relevant variable for this system, for short-range shooting, is whether the barrel is aimed at the target. On land, this can be achieved by satisfying the p-goal `target centered in binocular visual field'. But if the system goes hunting for fish, shooting them from above the water, it will make mistakes (fail to satisfy the e-goal) until it learns to correct the p-goal for refraction.

Here is yet another example. As a subgoal for staying alive, people often attempt to perceive themselves eating edible plants. But in an unfamiliar environment it can be quite easy to be mistaken about this, and poison yourself: the attained p-goal, `perceive oneself eating an edible plant', does not coincide with the e-goal of actually doing this.

To reiterate something, talk about e-goals only makes sense when one is analysing how an organism fits into its environment, especially when it doesn't do so very well (e.g. why do whales tend to beach themselves). If clairvoyance existed, e-control would equal p-control, so it doesn't, so it doesn't.

>Explain how e-control works without p control. ..

Like I have said several times, it doesn't. Actually, this is a slight exaggeration. Sometimes e-goals do appear to be maintained in part by simple S-R systems. Bird imprinting is one example: There are e-goals of `staying near parents' and `feeding young'. Imprinting seems to be an S-R device whereby the first thing the bird sees under certain circumstances is categorized as `parents' or `young', with no provisions for correcting a misidentification. Then, presumably, actual control systems take over to maintain the thus-derived p-goal, but they will fail to satisfy the e-goal if the imprinting has gone wrong.

The male erection is another plausible example of an S-R system, whose e-goal is presumably to insure that the capability is there when the opportunity is. But S-R systems are very limited in the extent to which they can achieve e-control, and where complex and delicate adjustments to constantly varying circumstances are required, p-control is the only known way to do the job.

>>Your `indirect control' concept here looks like my `e-control'.
>
>Nope.

Right.

>which, in-
>cidently, are very likely to be of variables that you might not want to say
>are "really" out there -- like non-linear combinations of chemical concentra-
>tions -- derived measures of whatever you want to imagine are "real" (e)
>variables.

No. You're confusing `being real' with `being a primitive ultimate constituent of reality'. If the chemical concentrations are real, so are the mathematical constructs based on them.

More generally, I don't know how useful this viewpoint might be for psychology in general or CT in particular. But I do believe that getting some story along these lines together would help in dealing with the sorts of philosophers and linguists I mix with.

On imitation: perhaps I overstate the lack of selectivity to some extent, but given that the connection between particular actions (high to intermediate level goals) and intrinsic reference levels is so intricate and obscure, I continue to prefer to emphasize the importance of luck w.r.t. who you happen to be thrown in with. Maybe this would be different if we lived a lot longer and had more time to figure out what was actually going on.

Avery Andrews

Date: Tue Jan 14, 1992 8:19 pm PST
Subject: Terminology

[From Bill Powers(920114.1000)]

Ken Hacker (920113) --

(to Chuck Tucker)
>I agree with your comments that CONTROL is creating semantic chaos for
>our mini-debate here.

So is "behavior." I think we have to pause and make sure we're all using these words to designate the same phenomena, otherwise we'll end up saying things like "control is just influence which control systems control, and anyway we don't control our behavior, but only our behavior." Semantic chaos indeed.

Control is the central phenomenon that CT addresses. CT explains fully and quantitatively a fact of nature that has been unrecognized in existing mainstream sciences of life. This is the fact that behavior -- the patterns we see organisms producing, such as moving about, doing things to the environment, and creating new relationships with and in the environment -- does not correlate with the outputs of the nervous system. Referring to those outputs as "actions," we can see that the actions of an organism join with independently variable influences and constraints to produce outcomes that we term "behavior." Neither the organism's actions nor influences in the environment *determine* those outcomes. Instead, they only contribute to the outcomes, the behavior patterns. If a crosswind contributes enough to the behavior of steering a car, the driver does not need to turn the wheel in order to steer around a curve.

Independent forces from the physical environment affect outcomes blindly, unknowingly. This applies as well to unintended side-effects of the actions of other organisms. Neither the physical environment nor an unintended side-effect modifies its effect on the outcome according to the outcome. Hence they do not control the outcome, but only influence it. The organism, on the other hand, does modify its actions according to what it senses of the outcome. Hence, the organism can control the outcome. When the sensed state of the outcome deviates from the state the organism prefers, the action changes as a function of the deviation. The inner organization of the organism is such that the change in action always opposes the deviation, with the result that the deviation is not permitted to become larger than what is necessary for detecting it. Note that the organism does not have to take into account the sources of these deviations, the independent causes of influences on the outcome. It simply monitors the outcome, compares it with an internal specification for the outcome, and acts directly on the outcome as required to oppose deviations. Therefore independent disturbances are prevented from having any effect on the outcome: their influences are cancelled by influences from the changing actions of the organism.

The name I suggest for controlled outcomes is "behavior." The means of influencing outcomes, I suggest, should be "action." The word that I suggest we use for the process of monitoring outcomes and adjusting actions to make outcomes match an inner specification is "control." This is precisely its meaning in engineering control theory.

The words we use for effects of one variable on another that are not modified according to their outcomes, I suggest, should be terms like "influence," "affect," and "disturb." If one variable influences another, the second variable could also be subject to many other influences; the state of the influenced variable can't be determined without knowing *all* sources of influence acting on it, including the influence of actions by an organism.

The term "determine", I suggest, should be reserved for cases in which the state of one variable is deducible from the state of another variable (or enumerable set of variables). If A determines B, then other variables influencing B can have no effect on it: only A need be known in order to know the state of B. Thus the location of the tracks determines the path of a railroad engine; influences such as lateral forces resulting from wind have no effect on the path.

Finally, a "constraint," I propose, should mean a property of the environment that establishes a relationship between two variables, or between the state of a variable at one time and its state at a later time. Conservation of momentum and energy is a constraint on motion. The length of one's arms and the limits of angular bending at a joint are mechanical constraints on control of positions. A schedule of reinforcement is a constraint on the relationship between bar-pressing behavior and the rate of reinforcement. Laws of nature are constraints. Properties of matter and energy are constraints. Constraints do not prescribe action; they simply account for the effects of whatever actions do occur.

"Actions" and "behaviors" have to be defined relative to the level of organization under discussion. The "behavior" of adding numbers using a hand calculator results from the "action" of pressing keys on the calculator. But at a lower level, the "behavior" of pressing keys results from the "action" of adjusting the positions of the hand and fingers. And at yet a lower level, the "behavior" of exerting a force on a key results from the "action" of tensing a muscle.

The behavior of adding numbers is influenced by the action of pressing keys as well as by the states of the keyboard contacts and of the integrated circuits inside the calculator. The behavior of pressing keys is influenced by the action of moving the hand, and also influenced by the position of the keyboard and the initial position of the hand. The behavior of pressing a key is influenced by the tension in muscles, and also by the spring resistance and the length of travel of the keys. Actions do not determine behaviors; they, along with other independent variables, only influence behaviors. When influences other than the actions vary, the actions must also vary -- oppositely -- in order that the same behavior occur. Only a control system can produce the necessary kinds of variations of action that maintain behavior in a constant state (or bring it to that state initially).

More terminology:

>I tend to believe that we have mental representations which are complex
>and which we use as bases for comparing inputs with desire states.

"Mental representations," in HCT, are identically neural signals arising from sensory receptors. Each level of signals enters a higher level of perceptual functions (neural computers), many functions acting in parallel, which re-represent subsets of the incoming signals as a new level of mental representations. There are 11 such levels in my model, covering (as far as I could) all phenomena of perception, all aspects of the experienced world, inner and outer, concrete and abstract. I refer to the mental-representation signals at all levels as "perceptions," rather than using different terms for low-level and high-level representations.

"Comparison" implies two things to be compared. In the HCT model, one of them is a mental representation, a perceptual signal, indicating the current actual state of the perceived world, or one aspect of it. The second is also a mental representation, a signal, but it represents the state of the same aspect of the perceived world _as it is intended to be perceived_. This is the reference signal. A comparator is simply a device that receives these two signals and emits an "error" signal indicating the difference between the two inputs to the comparator. A less pejorative term is "deviation." An error signal does not indicate a mistake. It simply indicates by how much and in what direction the current perception deviates from the current setting of the reference signal. That indication drives the corrective actions of the control system.

Your statement implies that mental representations are one thing, and inputs are another. In HCT there is no difference. All that the organism can know about the environment exists in the form of mental representations, perceptual signals. The organism can't know the actual states of its physical inputs (although an intelligent enough organism can certainly make models of the external environment and thus provide itself with a highly plausible story about what they are). When I say "an organism" I mean me and you and every other human being, as well as our coevals of other species. The environment that is directly experienced by a human organism is confined to the set of all perceptual signals (although they are not all consciously experienced at once). Wayne Hershberger disagrees with me. But I agree with me.

>I also believe that we do not simply react on the basis of comparisons,
>but make complex evaluations of behavioral path consequences.

Evaluation of behavioral path consequences can be done through the imagination connection. A system of higher level normally acts by sending reference signals to lower-level systems. Those reference signals specify the states to which individual lower-level systems are to bring the kind of perception that each controls. Copies of the resulting perceptual signals become inputs to the perceptual function in the controlling higher-level system. When lower-level control succeeds, as it usually does, the result is that each lower-level system sends upward a perceptual signal that matches the reference signal it is receiving from the output of the higher system.

Exactly the same effect can be achieved if the higher system sends its output not to the comparator of the lower system, but back into its own perceptual function. It is just as though the lower system had succeeded perfectly and instantly. This is what I call the imagination connection. With this connection in effect, the higher system can quickly go through possible outputs (I assume a level where complex logical processes are occurring) and judge their effects on the controlled variable. Thus selection of lower-level actions (and their perceptual consequences) can be done without actually producing any actions.

This process of mental planning is undoubtedly more complex than I make it here. Modeling must be involved, in the imagination path, because the properties of the outside world (which includes all lower-level control systems) must be taken into account. But the basic picture of how imagination works seems to explain the broad outlines of planning of all kinds -- not just behavior path planning.

Behavior always follows some path. The question is whether the paths are in fact always planned, or whether they are simply the result of the way a control system gets from a state of error to a state of no error. Planning of behavior paths is not necessary in all cases -- in fact, it is necessary in very few cases. To see whether a path is planned, one can introduce disturbances and see if their effects on the path are corrected or if the organism simply accepts the deviated path and reaches the goal anyway. The latter is probably the more likely outcome. Paths would be planned in advance only when they make a difference to the organism. Control systems do not have to precalulate behavior paths. Despite what you may have been hearing from motor-control or robotics types.

Skipping to your second post of today:

>Bill, I am disappointed that you single out social sciences for bad
>research. C'mon, let's be candid about research in general. Don't
>make me name names of physical scientists who have fabricated data or
>others who have published nonsense.

I'm not talking about dishonesty or incompetence. I think that social scientists have all the basic equipment that any other scientist has, and that most of them, like other scientists, are perfectly honest. I am singling out bad data and bad methodology, neither of which was invented by any of the social scientists now alive. Social scientists have been asked to build a science with the tools handed down to them. It's not their fault that the hammers are made of rubber and the nails bend at the first tentative tap. This is not an ad hominum argument.

>You say that "NO behavior is externally guided." I argue that all ALL
>behavior is externally guided, but that none is fully guided or
>controlled (in all senses of the term).

I think that "guidance" and "control" are essentially synonymous. To guide someone you must monitor what that someone is doing, compare what you observe with what you want to observe, and take corrective action based on the difference, if any. If you leave out any of the three -- observation, comparison with a goal, or action -- you have neither guidance nor control.

See the above distinctions between controlling and influencing. A society does not guide or control an individual -- that is, the whole society has no goal regarding the behavior of any one person, does not monitor that person's behavior, does not take corrective action if there is a deviation. Individuals within that society may do all these things, because individuals are control systems. All that a society as a whole can to is serve as a constraint and influence the perceptions that individuals are controlling. Individuals may oppose those influences or not, as they choose.

>I agree that social situations act on us only through sensory inputs.
>That is basic physiology. What is more interesting is how those

>sensory inputs and mechanisms are affected by thinking and feeling which
>has multiple origins, including social interactions.

How do those "origins" get inside us to become part of thinking and feeling? Through our sensory inputs. There's no other way. Thinking and feeling are part of what the control hierarchy does.

>Given that, we need to specify how I ever come about making one
>comparison conclusion over other possible ones.

But a "comparison conclusion" is simply a measure of the deviation of a perception from a reference signal. This process takes place in each control system, independently of the others. The only conclusions possible are "not enough," "too much", and "just right."

If you refer to comparison in the non-modeling sense, then you're talking about perceptions. You can say that an apple is redder than an orange, or cheaper, or better-tasting. But the control process is separate from that comparison, which is really a judgement of relationship. Given the perceptual comparison, you must still specify what the goal is: are you going to paint the orange to make it as red as the apple, or is the difference in redness OK with you? Are you going to raise the price on apples, or inject something in the orange to make it taste better? The goal has to be stated if control is to be involved. And then the comparator -- an element of the model -- must take the perceived relationship between apple and orange, compare it against the desired relationship, and judge it as being not sufficient, just right, or overdone -- relative to the preferred state.

>"Reference levels relating to these perceptions" are not black boxes.
>We can identify what they are and how they are developed.

Yes. In the HCT model, they are proposed to originate as memories of past states of perceptions, which are then selected by the outputs of higher-level systems to become active reference signals: "make your perception match this one."

>You are correct in stating that nothing CAUSES us to act. However,
>there are many things which IMPEDE us from acting. Ex. I want to be
>a basketball player and I am 4 ft. tall.

This leaves too much implicit, and is a bit loose. In fact, nothing external can impede us from ACTING, because we act with our muscles and our muscles are run by our nervous systems, not by the external world. The external world can impede us from SUCCEEDING -- that is, render a given action ineffective in controlling something. But it can't prevent us from TRYING.

The two statements, "I want to be a basketball player" and "I am 4 ft tall" do not impede me in going out for basketball. I might, in imagination, put the perceptions to which these terms refer together, and predict that I will not make the team, but I can STILL go out for basketball. If my judgment that my tryout will be a failure is not in conflict with any controlled variable -- if I don't mind trying something and failing at it -- I will go ahead and try. Who knows, maybe the coach will suddenly see some possibilities in the underground approach. Remember Bill Veeck and his LESS than four-foot baseball player. In fact, of course, my height will probably keep me off the team, but that's something I can't control; that's someone else's decision. I will control what I can control -- trying out for the team -- and see if the result is what I want at a higher level. If it isn't, I'll be disappointed but I'll reorganize and find something else that would be fun to try.

>The social interactions we have, in particular, provide not only
>with feedback, but with models for making perceptual comparisons.

Terminology again. Feedback is the effect of a variable on itself via a closed causal loop. One provides himself with feedback. Others can't just come up and "give you feedback." If they could give you any input they chose, independently of your actions, there wouldn't be any feedback loop. Feedback is the effect on your inputs of your own actions, which are derived in part from the same inputs. The loop may pass through other people, but their actions (that affect your perceptions) are based on your actions: they aren't just "given."

The mere fact of social interactions can't provide us with models: we have to make our own models, based on what we perceive to happen when we act or when parts of the perceived world act. Social interactions provide us with DATA. Our brains interpret it and create models of the organization underlying the data. Those models, however, can't simply be passed from one brain to another.

>Your claim that "what we choose to experience is
>determined by us.." is tautological. What we actually experience as we
>move about is often random, never chosen by us.

My mistake. I didn't see the ambiguity in my statement. I didn't mean that we can choose to experience anything but what we are experiencing, given the way we have organized our perceptual systems (imagination aside). I meant that we can set a goal defining what we want to experience, _and then act on the world so as to modify what is going on there and bring it into a match with the experience we want_. We determine what that goal will be; the environment doesn't. The environment may provide us with a selection of experiences from which to pick feasible goals, but it doesn't do the picking. The environment DOES determine what we must do in order to have the desired effect on experience. If we can't do what it requires, or if the desired result is impossible, then we fail to control.

>We choose among perceptions, always having an array of possible ones,
>yet those perceptions are related to our interactions with society and
>with others. And how much of our goals having NOTHING to do with
>society?

Don't forget that our perceptions are also related to being hungry or cold or happy or puzzled, and that they encompass not only society, but physics and chemistry and astronomy and geology and paleontology and biology and so on through the whole list of human interests. Because we live among other people which whom we cooperate and compete, most of our actions in some way must take other people and their desires and actions into account. Most of us are very involved with a few other people -- families, friends, co-workers, bosses. Most of us occasionally come up against "the system" in the form of people behind counters and desks, who demand that we do silly things and threaten us if we don't want to. And some of us -- but only some -- consider human interactions the only interesting game going, and spend their entire lives specifically and deeply involved with the control of social variables -- like getting elected again, or teaching sociology for a living.

Neurologists tell us that human beings are basically a set of neural connections. Biochemists tell us that behavior is controlled by interactions among molecules. Sociobiologists tell us that it is genetic fitness to reproduce that determine how we shall act. Physicists tell us that thermodynamics and quantum uncertainty are the key. Radical behaviorists tell us that schedules of reinforcement are what do the trick. Personality psychologists tell us that traits and attitudes and feelings and aspirations account for behavior. Sociologists tell us that the individual is simply an expression of the society. Existentialists tell us that individual being is at the core of it all.

Doesn't this strike you as a bit suspicious? All these answers, and they all show that the particular interests of the explainer just happen to contain the correct solution to it all. But when you ask any of these explainers how their explanations work, you run into a blank stare. The explanations ARE how it works. They don't ask what lies beneath the explanation. They don't try to link their own field of study to the fields of study of others. It's all extremely provincial and, aside from the specialized expertise involved, superficial.

Control theory crosses all these boundaries because it is concerned with the how of behavior more than the what. It has nothing specifically to do with society, or even with any particular individual behavior. All examples of behavior, all aspects of behavior in any discipline, are grist for its mill. The world it addresses is larger than that of any existing discipline. You ask how much of our goals have NOTHING to do with society. My answer would be: most of them. The thousands of goals that specify how our biochemistry will be controlled, how muscles will act, what sensations will be controlled, what shapes and movements and events, have nothing at all to do with society, because society is not perceived at these levels. The very concept of a society belongs in the upper levels of the hierarchy, and even there, only some of the control systems are concerned with relationships with other people or institutions. For the most part we are no more concerned with society than is a dog or a cat. At the upper levels, we are concerned with our conceptions of society and with many other things as well.

>Archimedes argued, "Give me a premise and I will deduce the world!" I
>hope that you are arguing something much different and that the
>contruction of control and perception control theory will begin to
>include propositions which account for the social nature of human
>being and social adaptation.

It already contains these things, but work toward making them specific and testable is only beginning. There is no guarantee, however, that the outcome will support that idea that there is such a thing as the "social nature of human beings." That notion is peculiar to the social sciences (funny thing). It is not likely that control theory will vindicate any specialist's point of view; it is more likely to transform them.

Gary Cziko (920113) --

Mary is responding, with my permission, to your comments on evolu --
OUCH!

Greg Williams (920113) --

I am introducing CT right where I want to be introducing it, in a Master Class. Why don't some of you psychologists reply to the articles on CT? You know how to do it.

This insane outpouring has got to cease. My command line says this is page 7. We are headed toward a disaster. Avery Andrews, I leave you to my friends.

Best to all

Bill P.

Date: Tue Jan 14, 1992 9:00 pm PST
Subject: Evolution

[from Mary Powers]

(Gary Cziko 920113.2010)

>Biological evolution has no purpose, no grand design; and yet it
>has resulted in entities (organisms) which have purposes and
>grand designs

Not quite, I think. Purpose is not the result of evolution, it is the reason evolution began in the first place. The original purpose, and the one that drives evolution to this day, is to resist disturbances. The original and continuing grand design is to survive. Certainly circumstances (luck and tiny differences) have a role to play and variation is blind, but selective retention (or retentive selection (?)) is the phenomenon that accounts for evolution, not its result. Nonpurpose begot purpose -- once, two billion years ago. One complex molecule accidentally configured just a little differently, so that it resisted dissolution -- one of billions that came and went, maintaining their integrity for shorter and longer periods of time -- but just one permanently successful one, that is not our ancestor, but is alive and well in the form of all living things today although bits and pieces die off all the time.

(Ken Hacker 920113)

I don't think you need to be so defensive about the social sciences. Bill's remarks are pretty clearly directed at (but it's such a mouthful every time) the social, behavioral, and life sciences (as in his 920110.2100). Nobody's singled out. And if there's sloppy physics going on, bad cess to that too. Usually he says what he really believes, but sometimes he's trying on a point of view to see what comes of it, and sometimes he's just being provocative (I can't always tell the difference -- after 35 years).

Date: Wed Jan 15, 1992 5:13 am PST
Subject: probability estimates

[From: Bruce Nevin (920115 0709)]

(Martin Taylor 920114 15:15) --

So you as an external observer could in principle determine the probabilities of a given value for the perceptual signal p input to an ECS (if you could meter its inputs with some external measuring device), but the ECS itself cannot, nor does any other ECS.

However, you are saying that "probability estimates" exist in each ECS:

>That evaluation [of probabilities] becomes
>apparent in such things as the weighting and transformation functions
>of the perceptual input matrix, the dead zone (if any) of the comparator
>or output transform, and the like. The higher-level ECSs may incorporate
>much more sophisticated mechanisms, especially if inputs to the perceptual
>matrix are from above the category level, where in logic is an acceptable
>way of manipulating them. As I said (to paraphrase) in the posting about
>which you are asking, the probability estimates at very low levels could
>even be built in by evolution, and might be totally insensitive to experience.

In other words, for example, there could be no recognizer for bananas, or for a phonemic segment /f/, or for the word "fish" if the perceptual input requirements of each such ECS were unlikely to be satisfied.

I see nothing in this that requires the ECS (or its input function, or its comparator, or its output function) to measure probability, derive it (probability) as a quantum, and use that quantum (the probability estimate) in either its function or its development. Probabilities may be estimated by an outside observer, and that observer may hold that the probabilities inhere in the things observed, without those observed things themselves controlling for probabilities in any way. The strengthening of the memory of a perception with repetition correlates with probability of further repetitions, and conversely for attenuation of memory. That does not mean that the mechanisms for associative memory (whatever they may be) estimate probabilities.

Tom Bourbon (911226 - 13:43) had asked for indications how you might use information-theoretic concepts

>to either duplicate, or improve on, the results
>published for the IT-free versions of the PCT model
>. . . picking any published example of quantitative
>modeling with PCT

It appears that there is no neural current in the model that corresponds to a measurement or assessment of the probability of p. I take this as prima facie evidence that the duplication or improvement that Tom asked for will not and can not be forthcoming.

Bruce
bn@bbn.com

Date: Wed Jan 15, 1992 6:31 am PST
Subject: Re: Social Control, Ecology

[From Chris Malcolm]

Rick Marken writes:

>Ken Hacker [920113] says:

>   Rather, I think that people are predisposed to
>>pick up the high-level goals of the people around them in an
>>extremely unselective and unreflective way.

>I just can't buy it -- this is the ultimate S-R notion.

>It looks to me like people do "go along with the crowd" to a large extent;
>but I also see very pronounded individual styles amongst people in the
>same crowd.

The development of handwriting might be a good domain in which to study this kind of thing. We learn it by imitating a reference model, but it is understood that there is a lot of latitude. People deviate from the reference model (and choose colours of ink, size of nibs, pen technology, etc.) with which they can produce writing that -- for some personal reasons -- feels and looks good to them as they write it, and looks good to them in the mass after a good amount has been produced. Some people are especially pleased with tiny neat writing, others by big writing with large bold flourishes, etc.. Some people are upset if their writing is not easily legible to others, some consider the important legibility criterion to be only that they personally should be able to have a good guess as to what it means.

Date: Wed Jan 15, 1992 6:42 am PST
Subject: Re: MOTIVATION AND EMOTION papers

[from Gary Cziko 920114.2230]

Greg Williams (920114)

Greg Williams mentioned the following two articles and implied that they were in the _American Psychologist_. They are not.

>Edwin A. Locke,
>"Goal Theory vs. Control Theory: Contrasting Approaches to Understanding
>Work Motivation," pp. 9-28.

Howard J. Klein, "Control Theory and Understanding
>Motivated Behavior: A Different Conclusion," pp. 29-44.

Joel Judd discovered for me (and now for you, too) that they are both in _Motivation and Emotion_, 1991, _15_(1).

--Gary

Date: Wed Jan 15, 1992 7:12 am PST
Subject: determinism not for us; benign social control

From Greg Williams (92015)

[Gary Cziko 920113.2010]

>I had thought that all the Laplacean demons were long gone; perhaps they are
>just hiding out in the hills of Kentucky!

CSGNet probably isn't the place to discuss this issue, for two reasons: the volume of more CT-relevant stuff is already huge and Netters almost certainly aren't going to come up with the answers, as I suggested in my post which you were answering (the data aren't available -- history is not subject to our experiments). Nevertheless, I could discuss privately with Gary such interesting issues as superdeterminism in the foundations of quantum mechanics, (deterministic!) chaos, limit cycles and negative feedback to obviate the effects of chaos-like "noise" on system function (that is, to discretize possible system states near, but not necessarily at, certain points of functional importance, so that the system carries out the same function -- stays in the relevant "basin" in state space -- despite physical indeterminism), Eccles' wonderfully quaint notion of free-will residing in miracles happening at the level of synaptic vesicles, the frequency (over infinite time) model for "probability" and the ignorance model for "randomness" generation, etc., etc. Anybody else who wanted to could participate.

[Bill Powers (920114.0800)]

>I think the hangup here begins with your statement that one person can
>arrange to control another's perception so that BOTH people are
>controlling the SAME perception. I want to perceive you in this room, and
>you want to perceive you in this room. Since we use the same term for
>both perceptions, "perceive you in this room," they must be the same
>perceptions. Well, not quite.

>In the first place, the perceptions are in different heads. In the second
>place, they are derived by private and possibly unique means from the raw
>sensory inputs available at the locations of the two people. And in the
>third place, they are in different hierarchies with different goals at
>the same and at higher levels. I may be satisfied that I have caused you
>to control a perception just like mine relative to the same reference
>level I am using. But that's my conceit, not a fact. However, I'll play
>the game.

I tried to make it clear that the "conceit" of the controller in my example is part of a MODEL (or are all models conceits? -- I think not -- "conceit" implies no basis for one's claim except wishful thinking, and many models, including the one alluded to here, has other bases) which could be wrong in various ways and could be tested in various ways.

>So isn't AM controlling her daughter's perceptions, making her daughter seek
>the kinds of perceptions that momma wants at the reference levels that please
>momma? Indeed she is; her control is working.

Note that you have agreed with me to call this sort of manipulation "control."

>This sort of story makes us very irritated with momma and her

>selfishness. But doesn't it suggest something less than optimum about the
>daughter, too? It takes two to play this game.

Exactly my point. I claim that A is controlling for A's perception that B is controlling for a certain perception (of B), and not other possible perceptions (of B). That certain perception of B, when controlled for by B, entails the satisfaction of control for a perception of A. A has a model of B's hierarchy, and on the basis of that model, controls for perceiving B's hierarchy controlling for what A wants to perceive it controlling for. B's hierarchy "goes along" with this because it takes B's perception of the situation (as "set up" by A) as important. A didn't (just now) do anything to alter the existing hierarchy in B to make the set-up be taken by B AS important. That "importance" has a history, involving the interactions of the friends A and B. A is simply USING what A can guess about B's hierarchy to manipulate B. It is still (completely) B's hierarchy. It does take two hierarchies to play this particular game.

>But what about the second time, the third time, and so on? After a few
>false alarms, if you keep on falling for these little deceits, you have
>to face a problem: this person is obviously trying to control you, and
>you're playing along with it.

The "contrived" deceitful situation was just to get your ears pricked up. Now that (I think) you're paying attention, I claim that analogous non-deceitful situations involving the kind of control discussed above are commonplace. People "play along with" experts and teachers and advisors of all kinds. Medical examples come first to mind. The physician performs a few tests on you and solemnly announces that you have "x-disease," which is "quite serious," but if you take 3 Hifaluten(TM) pills every 4 hours for the next 60 days, you'll probably be OK. The physician (because of his controlling for adherence to the Hippocratic Oath, etc.) believes that his diagnosis is correct, ditto his prescription. He isn't deliberately being deceitful. Still, he could be wrong about the wonderfulness of Hifaluten pills for YOU. At any rate, you buy into the doc's non-deceitful set-up of your perceptions. The pills cost an arm and a leg, but you take them, as prescribed. YOU want to see yourself doing it and the DOCTOR wants to see you doing it. (But you surely wouldn't have wanted to if the doctor hadn't convinced you that it was "best," given YOUR OWN HIERARCHY'S DEFINITION OF BEST (something like: not dying soon). So maybe the pills don't work. If your estate sues the doctor for malpractice and he can show that he followed accepted procedures based on the "right" tests, and so forth, your estate will lose and he'll be back in the office prescribing Hifaluten to somebody else next week. I hope you begin to see the potential complexities of such "benign" control by someone of the way others' hierarchies produce behaviors which both they and you want. The key to all of this, of course, is that pervasive social lubricant, trust. Yes, it takes two to play the trust game. And we all are playing it incessantly, all involved in the sort of social control as outlined above.

I think the most important upshot for CT is the notion that such social control is aided by better models of others' hierarchies. The Test is the way to make better models. Skinner would be turning over in his grave if he could realize how he handicapped himself so by not making use of CT.

>Get a haircut.

This is a superb example of what I mean by "the Royal Road to Manipulation lies through CT, for those so inclined." The only time in many years when I have seriously considered getting a haircut was when I was planning to visit

Bob Clark (hi, Bob!) to learn more about the early history of CT. Knowing that it was at least possible that my appearance might "turn him off," I asked your advice on whether I should cut my hair. You suggested not, on the basis of your personal knowledge of Bob. And I didn't.

The way to get me to get a haircut now would be to convince me that it is important (whether so or not -- you might be deceitful or not) to ME to get a haircut: that upon the haircut probably hangs the chance of, say, locating a CT paper previously thought long lost. If it all worked out in the end, and PROBABLY EVEN IF IT DIDN'T, I'd welcome more of that sort of control from you.

Best wishes,

Greg

Date: Wed Jan 15, 1992 9:29 am PST
Subject: Testing

[From Bill Powers (920115)]

The tone on this network is getting testy, probably a symptom of mental exhaustion unless I'm just projecting. If that is right, perhaps I have an antidote: get more test-y. How about some testing?

A general question to all the theoreticians who have been proposing explanations of language, social control, cybernetic laws, and so on. How do you know you're right? I don't mean how do you know absolutely, for certain, and forever that your suggestions reflect ontological Nature, but only this: what tests have been done, or could be done, to support or deny your explanation or your claim?

When someone expresses doubt about a proposition, the general reaction here (which includes mine) seems to be to amplify and exemplify the theory, somewhat like the British colonialist shouting louder when the natives refuse to understand plain English. The proposition is not defended; it is merely given some additional shoring-up by further details of the structure of which it is a part. If I express doubt that the sentences we actually hear and produce are created in some particular way, I would expect some attempt to demonstrate that this process actually does take place in real people -- not just added reason to believe that if it did, sentences would be produced. If a statement is made characterizing a neural process, I would look for some evidence that this characterization is defensible, not just a proof that it makes logical sense.

I don't mean that I demand the smoking gun -- only that some attempt be made to think of assertions in terms of testing them against observations. Anyway, I can't "demand" anything. I've been an offender, too. I just think that far fewer words would be generated if we turned our attention to challenging our own theories by thinking up ways of testing for the implications of our propositions: "If this is true, then such-and-such ought to be observed. Is it?"

And by this point, isn't everyone in favor of producing fewer words, especially fewer words that lead to more substance?

Models are supposed to imitate the behavior of a real system. But more is demanded of them than that (in my view). Not only must the whole model produce the right outcomes, all by itself, but it must achieve its results in the same way that the real system does. That is, if the model's behavior depends on some process, then we have to show that this process occurs, or is convincingly plausible.

Every theoretician knows better than anyone else what the critical assumptions are in his or her own model. Those are the assumptions without which the whole structure of the model would collapse. Once you have carried the model to the stage where you know what those assumptions are, there's no point in going farther without finding out whether the assumptions are true. If you just trust the assumptions without test, you can waste years of effort following out their implications and showing that if, IF, they were true, the model would produce the right behavior. You can spend a life adding braces, patches, and gingerbread to the model, trying to correct little errors in its predictions, when all along the basic premise is false and the model is empty. This is what the S-R theorists did. They assumed that responses follow regularly from stimuli, but they never tested that idea. So they wasted a century of everyone's time and froze psychology in the 19th Century.

So that's my message for today. I'm speaking in ideal terms, of course. We can't do everything I'd like to see done. But I'd like to see a strong bias toward trying to do it, and away from defending positions.

Best,

Bill P.

Date: Wed Jan 15, 1992 11:04 am PST
Subject: Re: Social Control, Ecology

[From Rick Marken (920115)]

Ken Hacker [920114]

>Any assertions that human beings compare desired states with encountered
>states with no form of social influence are sheer poppycock.

Maybe. All I wanted to know was WHY?

> You need to start thinking in terms of HUMAN processing of signals, which
>by the nature of our constitution, complexity, and interactions, has CONTENT
>other than gateway impulses.

I think that when you start to understand PCT you will see that it is not about humans (or any organisms) as "gateway impulses". I know that "social influence" is important to you but, as I hope Bill Powers' post today should make clear, it is just a small part of PCT (though, as you can see from the volume of mail on the topic, one that is particularly interesting to people, especially people who imagine that their problems are created mainly by other people).

Avery Andrews (920115)

>We don't. But perception does not always succeed in discerning the variable
>that is the ecologically relevant one.

I believe that what you are calling ecological variables are what PCT people call constraints or disturbances. Your example of shooting fish in the water demonstrates how refraction changes the feedback function (the constraint) that relates output to input. The plant example shows the same thing -- a change in the function that relates output (eating) to input (feeling poisoned). I did an experiment with Bill to demonstrate hierarchical control-- it was a tracking experiment where, at times, the polarity of the connection between handle movement (output) and cursor movement (input) would reverse. So we were suddenly changing the ecology of the tracking environment; the only way the person knew about the change was because, say, moving the handle to the left suddenly started to move the cursor to the right instead of the left (like a very severe refraction change -- 180 degree change instead of 10 degree). The interesting result of the study was that, for about 1/2 second the subject continues to respond to "error" in the same old way -- which, because of the polarity change, means that the cursor starts to move away from the target EXPONENTIALLY -- until a higher order system can change the polarity of the subject's control loop.

The point is that your e-control is already part of the PCT model -- it is in the system equation relating output to input and, as I said, there are two components to it -- constraints (feedback function) and disturbance.

>To reiterate something, talk about e-goals only makes sense when one is
>analysing how an organism fits into its environment, especially when it
>doesn't do so very well

Evolution is a very interesting topic and I think that PCT has an enormous contribution to make (I think, for example, PCT applied at the molecular level (as broached in Mary Powers' post) explains the existence of punctuated equilibria in the fossil record). But this 'ecological variable' thing just doesn't seem to me to help much in terms of understanding behavior. The very heart of PCT is the recognition that variables are control-led in the context of constraints and disturbances in the environment (to the extent that the organisms is capable of doing so -- the evolutionary angle). In that sense, from the PCT point of view "ecology" is involved in the control of ALL perceptual experience -- from muscle tension to religious ritual. All of these variables are controlled in the context of an 'ecology" (constraints and disturbances). So who needs a special "ecological perspective".

If you just want PCT people to talk a certain way so that certain interest groups will listen then, my advice is, forget it. People will only try to learn PCT if they WANT TO. If they think there is something to PCT, but they don't really want to learn it, then they will not be happy until they are able to convince themselves that PCT is just whatever it is that they thought it was in the first place (ecological variables, social cybernetics, reinforcement theory, chaos, neural networks, whatever). I have learned that it is really not worth it to prozeletize (sp?) -- though I can't resist sometimes. All I (or anyone else on this list who understands PCT) can do is try to teach PCT and hope that someone wants to learn it.

>If clairvoyance existed, e-control would equal p-control, so it doesn't,
>so it doesn't.

So there is only p control after all.

>   Sometimes e-goals do appear to be maintained in part by

>simple S-R systems.

Oops. Lost it again. It appears that goals are maintained by SR systems--
but they aren't. SR systems can't control.

> Bird imprinting is one example: There are e-goals
>of `staying near parents' and `feeding young'.

If imprinting involves control (and it does -- "staying near parents"
implies disturbance resistence ; if the parents move, the bird compensates
and maintains the distance at "near") then it is p control -- the only kind.

> But S-R systems are very limited in the extent to which they
>can achieve e-control,

Yes, they are limited to 0 -- there is no control.
I think I get what you are saying -- e control is just you noticing that
higher order variable that is controlled by an action of the organism;
the duck p controls proximity to parent and, as a side effect is
controlling its ability to stay safe and fed. If this is what you mean,
then this is, again already part of the control model and the higher
order (or possibly intrinsic variable) must be tested to see if it is
controlled. If it is, then it is p controlled.

>getting some story along these lines together would help in dealing
>with the sorts of philosophers and linguists I mix with.

See my comments above. If you want to control your relationship with these
folks that's up to you. I personally don't think it's worth it to describe
the swan of PCT as the ugly duckling of "ecological" whatever in the hopes
of fooling anyone into accepting it. The philosophers and linguists you
mix with probably already know what's RIGHT in their disciplines so they'll
dismiss PCT anyway -- unless you describe it as being what they already
though was right, in which case they will say PCT is "nothing but...".

Hasta Luego

Rick

Date: Wed Jan 15, 1992 2:21 pm PST
Subject: Re: Terminology

[Martin Taylor 920115 16:00]
(Bill Powers 920114.1000)

>This insane outpouring has got to cease. My command line says this is
>page 7. We are headed toward a disaster.
Despite what I wrote yesterday about the quantity of outpourings, I wouldn't
want insane ones like that to stop. But I have a quibble about an internal
inconsistency (probably more apparent than real):

>"Mental representations," in HCT, are identically neural signals arising
>from sensory receptors. Each level of signals enters a higher level of
>perceptual functions (neural computers), many functions acting in
>parallel, which re-represent subsets of the incoming signals as a new
>level of mental representations. There are 11 such levels in my model,
>covering (as far as I could) all phenomena of perception, all aspects of

>the experienced world, inner and outer, concrete and abstract. I refer to
>the mental-representation signals at all levels as "perceptions," rather
>than using different terms for low-level and high-level representations.
>
>"Comparison" implies two things to be compared. In the HCT model, one of
>them is a mental representation, a perceptual signal, indicating the
>current actual state of the perceived world, or one aspect of it. The
>second is also a mental representation, a signal, but it represents the
>state of the same aspect of the perceived world _as it is intended to be
>perceived_. This is the reference signal.

and

>>"Reference levels relating to these perceptions" are not black boxes.
>>We can identify what they are and how they are developed.
>
>Yes. In the HCT model, they are proposed to originate as memories of past
>states of perceptions, which are then selected by the outputs of higher-
>level systems to become active reference signals: "make your perception
>match this one."
>

When I read the first passage I said to myself "He's forgotten that he used to allow mental represesntations to be memories of perceptual signals," since they are presumably important, and are not within the "P-R=E and that's all there is" description. But a few pages of insane outpourings later, there are the memories of blessed memory. Let's keep them, shall we?

Martin

Date: Wed Jan 15, 1992 2:53 pm PST
Subject: Re: probability estimates

[Martin Taylor 920115 16:15]
(Bruce Nevin 920115 0709)
>
>
>So you as an external observer could in principle determine the
>probabilities of a given value for the perceptual signal p input to an
>ECS (if you could meter its inputs with some external measuring device),
>but the ECS itself cannot, nor does any other ECS.
>

No, no, no! I am saying that no external observer could, in principle, determine the subjective probability for anyone or anything else. The ECS can do it for itself and for itself only.

>
>In other words, for example, there could be no recognizer for bananas,
>or for a phonemic segment /f/, or for the word "fish" if the perceptual
>input requirements of each such ECS were unlikely to be satisfied.
>

Correct. That is a quite true statement.

> Probabilities may
>be estimated by an outside observer, and that observer may hold that the
>probabilities inhere in the things observed, without those observed
>things themselves controlling for probabilities in any way.

You can say this only because you still see probability as an equivalent of "past relative frequency", which can indeed be measured by and agreed upon by a wide variety of observers. But this says nothing about the probability as assessed by the "I" observer, who may have very good other reasons for doubting that the frequency relates to the future observations.

Consider a case in point. Judging by the frequency of observations of extraterrestrial flyinf devices by humans, some from inside the devices, what do you think is the probability that you will get to see the inside of one for yourself? It seems that people are chosen for this experience from the population at random, so you might just as well be one of them. But you have all sorts of other reasons (I presume) for doubting the veracity of the reports of these observations, which lead you to assign a subjective probability that is higher for the statement "all the observers were deluded or hoaxers" than for "these observations apply to people at random and may well apply to me in the future." Why? Because the whole structure of your physical understanding of the universe, plus the knowledge that a few of these reports are really hoaxes, is incorporated in your judgment of probability. At least that is true for me. I cannot know what is true for you, but I can presume it, and I judge from your no-nonsense approach to everything you address on CSG-L.

No. Probabilities cannot be estimated for you by an outside observer. Evidence can, and there are theories about how for a rational observer sufficient evidence will in the end overwhelm other factors, so that observers converge on the same probability estimate, given the same observations. But that isn't the same as saying that probabilities inhere in the things observed. That's a Newtonian view of the world--God's in his heavan, and I can ask him for the truth--which I abjure in favour of the Einsteinian view--I can see what I see, and from that I judge what I judge.

As for your snide response to Tom Bourbon about the potential utility of considering information rates, we will let the future judge. As a matter of fact, I do doubt whether there is likely to be much improvement in duplicating the control behaviour in a continuous environment without conflicts, which is the only environment in which PCT has, as far as I am aware, generated numeric data of great accuracy. That's because there isn't much difference in what could be predicted.

Martin

Date: Wed Jan 15, 1992 3:40 pm PST
Subject: BBS special issue

The following is from Stevan Harnad, Editor of Behavioral and Brain Sciences. Some readers of CSG-L might like to take appropriate action.
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To: BBS Associates (with apologies if you have seen these on another list already):

Below are the abstracts of 8 forthcoming target articles for a special issue on Movement Systems that will appear in Behavioral and Brain Sciences (BBS), an international, interdisciplinary journal that provides Open Peer Commentary on important and controversial current research in the biobehavioral and cognitive sciences. This will be the first in a new series called "Controversies in Neuroscience," undertaken in collaboration with Paul Cordo and the RS Dow Neurological Science Institute.

Commentators must be current BBS Associates or nominated by a current BBS Associate. To be considered as a commentator on any of these articles, to suggest other appropriate commentators, or for information about how to become a BBS Associate, please send email to:

harnad@clarity.princeton.edu or harnad@pucc.bitnet   or write to:
BBS, 20 Nassau Street, #240, Princeton NJ 08542 [tel: 609-921-7771]

Please specify which article or articles you would like to comment on. (Commentators will be allotted 1000 words to comment on one of the articles, 750 words more to comment on two of them, 500 more for three and then 250 more for each additional one, for a maximum of 3500 words to comment on all eight target articles.)

To help us put together a balanced list of commentators, please give some indication of the aspects of the topic on which you would bring your areas of expertise to bear if you were selected as a commentator. In the next week or so, electronic drafts of the full text of each article will be available for inspection by anonymous ftp according to the instructions that follow after the abstracts. These drafts are for inspection only; please do not prepare a commentary until you are formally invited to do so.

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1. Alexander GE, MR De Long, & MD Crutcher: DO CORTICAL AND BASAL GANGLIONIC MOTOR AREAS USE "MOTOR PROGRAMS" TO CONTROL MOVEMENT? bbs.alexander

2. Bizzi E, N Hogan, FA Mussa-Ivaldi & S Giszter: DOES THE NERVOUS SYSTEM USE EQUILIBRIUM-POINT CONTROL TO GUIDE SINGLE AND MULTIPLE JOINT MOVEMENTS? bbs.bizzi

3. Bloedel JR: DOES THE ONE-STRUCTURE/ONE-FUNCTION RULE APPLY TO THE CEREBELLUM? bbs.bloedel

4. Fetz EH: ARE MOVEMENT PARAMETERS RECOGNIZABLY CODED IN SINGLE NEURON ACTIVITY? bbs.fetz

5. Gandevia SC & D Burke: DOES THE NERVOUS SYSTEM DEPEND ON KINESTHETIC INFORMATION TO CONTROL NATURAL LIMB MOVEMENTS? bbs.gandevia

6. McCrea DA: CAN SENSE BE MADE OF SPINAL INTERNEURON CIRCUITS? bbs.mccrea

7. Robinson DA: IMPLICATIONS OF NEURAL NETWORKS FOR HOW WE THINK ABOUT BRAIN FUNCTION bbs.robinson

8. Stein JF: POSTERIOR PARIETAL CORTEX AND EGOCENTRIC SPACE bbs.stein

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1.   DO CORTICAL AND BASAL GANGLIONIC MOTOR AREAS

USE "MOTOR PROGRAMS" TO CONTROL MOVEMENT?

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KEYWORDS: basal ganglia, cortex, motor system, motor program, motor control, parallel processing, connectionism, neural network

ABSTRACT: Prevailing engineering-inspired theories of motor control based on sequential/algorithmic or motor programming models are difficult to reconcile with what is known about the anatomy and physiology of the motor areas. This is partly because of certain problems with the theories themselves and partly because of features of the cortical and basal ganglionic motor circuits that seem ill-suited for most engineering analyses of motor control. Recent developments in computational neuroscience offer more realistic connectionist models of motor processing. The distributed, highly parallel, and nonalgorithmic processes in these models are inherently self-organizing and hence more plausible biologically than their more traditional algorithmic or motor-programming counterparts. The newer models also have the potential to explain some of the unique features of natural, brain-based motor behavior and to avoid some of the computational dilemmas asscociated with engineering approaches.

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2. DOES THE NERVOUS SYSTEM USE EQUILIBRIUM-POINT CONTROL TO GUIDE SINGLE AND MULTIPLE JOINT MOVEMENTS?

E. Bizzi, N. Hogan, F.A. Mussa-Ivaldi and S. Giszter
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KEYWORDS: spinal cord, force field, equilibrium point, microstimulation, multi-joint coordination, contact tasks, robotics, inverse dynamics, motor control.

ABSTRACT: The hypothesis that the central nervous system (CNS) generates movement as a shift of the limb's equilibrium posture has been corroborated experimentally in single- and multi-joint motions. Posture may be controlled through the choice of muscle length tension curves that set agonist-antagonist torque-angle curves determining an equilibrium position for the limb and the stiffness about the joints. Arm trajectories seem to be generated through a control signal defining a series of equilibrium postures.

The equilibrium-point hypothesis drastically simplifies the requisite computations for multijoint movements and mechanical interactions with complex dynamic objects in the environment. Because the neuromuscular system is springlike, the instantaneous difference between the arm's actual position and the equilibrium position specified by the neural activity can generate the requisite torques, avoiding the complex "inverse dynamic" problem of computing the torques at the joints.

The hypothesis provides a simple unified description of posture and movement as well as performance on contact control tasks, in which the limb must exert force stably and do work on objects in the environment. The latter is a surprisingly difficult problem, as robotic experience has shown.

The prior evidence for the hypothesis came mainly from psychophysical and behavioral experiments. Our recent work has shown that microstimulation of the spinal cord's premotoneuronal network produces leg movements to various positions in the frog's motor space. The hypothesis can now be investigated in the neurophysiological machinery of the spinal cord.

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3. DOES THE ONE-STRUCTURE/ONE-FUNCTION RULE APPLY TO THE CEREBELLUM?

James R. Bloedel
Division of Neurobiology
Barrow Neurological Institute
Phoenix, AZ

KEYWORDS: cerebellum; Purkinje cells; mossy fibres; movement; proprioception; body image; kinesthesis; robotics; posture.

ABSTRACT: The premise explored in this target article is that the function of the cerebellum can be best understood in terms of the operation it performs across its structurally homogeneous subdivisions. The functional heterogeneity sometimes ascribed to these different regions reflects the many functions of the central targets receiving the outputs of different cerebellar regions. Recent studies by ourselves and others suggest that the functional unit of the cerebellum is its sagittal zone. It is hypothesized that the climbing fiber system produces a short-lasting modification in the gain of Purkinje cell responses to its other principle afferent input, the mossy fiber-granule cell-parallel fiber system. Because the climbing fiber inputs to sagittally aligned Purkinje cells can be activated under functionally specific conditions, they could select populations of Purkinje neurons that were most highly modulated by the distributed mossy fiber inputs responding to the same conditions. These operations may be critical for the on-line integration of inputs representing external target space with features of intended movement, proprioceptive and kinesthetic cues, and body image.

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4. ARE MOVEMENT PARAMETERS RECOGNIZABLY CODED IN SINGLE NEURON ACTIVITY?

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KEYWORDS: neural coding; representation; neural networks; cross-correlation; movement parameters; parallel distributed processing

ABSTRACT: To investigate neural mechanisms of movement, physiologists

have analyzed the activity of task-related neurons in behaving animals. The relative onset latencies of neural activity have been scrutinized for evidence of a functional hierarchy of sequentially recruited centers, but activity appears to change largely in parallel. Neurons whose activity covaries with movement parameters have been sought for evidence of explicit coding of parameters such as active force, limb displacement and behavioral set. Neurons with recognizable relations to the task are typically selected from a larger population, ignoring unmodulated cells as well as cells whose activity is not related to the task in a simple, easily recognized way. Selective interpretations are also used to support the notion that different motor regions perform different motor functions; again, current evidence suggests that units with similar properties are widely distributed over different regions.

These coding issues are re-examined for premotoneuronal (PreM) cells, whose correlational links with motoneurons are revealed by spike-triggered averages. PreM cells are recruited over long times relative to their target muscles. They show diverse response patterns relative to the muscle force they produce; functionally disparate PreM cells such as afferent fibers and descending corticomotoneuronal and rubromotoneuronal cells can exhibit similar patterns. Neural mechanisms have been further elucidated by neural network simulations of sensorimotor behavior; the pre-output hidden units typically show diverse responses relative to their targets. Thus, studies in which both the activity and the connectivity of the same units is known reveal that units with many kinds of relations to the task, simple and complex, contribute significantly to the output. This suggests that the search for explicit coding may be diverting us from understanding more distributed neural mechanisms that are more complex and less directly related to explicit movement paremeters.

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5. DOES THE NERVOUS SYSTEM DEPEND ON KINESTHETIC INFORMATION TO CONTROL NATURAL LIMB MOVEMENTS?

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KEYWORDS: kinesthesia, motor control, muscle, joint and cutaneous afferents, motor commands, deafferentation

ABSTRACT: This target article draws together two groups of experimental studies on the control of human movement through peripheral feedback and centrally generated signals of motor command. First, during natural movement, feedback from muscle, joint and cutaneous afferents changes; in human subjects these changes have reflexive and kinesthetic consequences. Recent psychophysical and microneurographic evidence suggests that joint and even cutaneous afferents may have a proprioceptive role. Second, the role of centrally generated motor commands in the control of normal movements and movements following acute and chronic of deafferentation is reviewed. There is increasing evidence that subjects can perceive their motor commands under various conditions, but this is inadequate for normal movement; deficits in motor performance arise when the reliance on proprioceptive feedback is abolished, either experimentally or because of pathology. During natural movement, the CNS appears to have access to functionally useful input from a range of receptors as well as from internally generated command signals. Remaining unanswered questions suggest a number of avenues for further research.

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6. CAN SENSE BE MADE OF SPINAL INTERNEURON CIRCUITS?

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KEYWORDS: interneuron, motor control, reflexes, spinal cord, flexion, muscle synery, presynaptic inhibition.

ABSTRACT: It is increasingly clear that spinal reflex systems cannot be described in terms of simple and constant reflex actions. The extensive convergence of segmental and descending systems onto spinal interneurons suggests that spinal interneurons are not relay systems but rather form a crucial component in determining which muscles are activated during voluntary and reflex movements. The notion that descending systems simply modulate the gain of spinal interneuronal pathways has been tempered by the observation that spinal interneurons gate and distribute descending control to specific motoneurons. Spinal systems are complex, but current approaches will continue to provide insight into motor systems. During movement, several neural mechanisms act to reduce the functional complexity of motor systems by inhibiting some of the parallel reflex pathways available to segmental afferents and descending systems. The flexion reflex system is discussed as an example of the flexibility of spinal interneuron systems and as useful construct. Examples are provided of the kinds of experiments that can be developed using current approaches to spinal interneuronal systems.

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7. IMPLICATIONS OF NEURAL NETWORKS
FOR HOW WE THINK ABOUT BRAIN FUNCTION

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KEYWORDS: Neural networks, signal processing, oculomotor system, vestibulo-ocular reflex, pursuit eye movements, saccadic eye movements, coordinate transformations

ABSTRACT: Engineers use neural networks to control systems too complex for conventional engineering analysis. To examine hidden unit behavior would defeat the purpose of this approach, because individual units would be largely uninterpretable. Yet neurophysiologists spend their

careers doing just that! Hidden units contain bits and pieces of signals that yield only arcane hints of network function and no information about how the units process signals. Most of the literature on single-unit recordings attests to this grim fact. On the other hand, knowing system function and describing it with elegant mathematics tells one very little about what to expect of interneuron behavior. Examples of simple networks based on neurophysiology are taken from the oculomotor literature to suggest how single-unit interpretability might degrade with increasing task complexity. Trying to explain how any real neural network works on a cell-by-cell, reductionist basis is futile; we may have to be content with understanding the brain at higher levels of organization.

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8. POSTERIOR PARIETAL CORTEX AND EGOCENTRIC SPACE

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KEYWORDS: posterior parietal cortex; egocentric space; space perception; attention; coordinate transformations; distributed systems; neural networks.

ABSTRACT: The posterior parietal cortex (PPC) is the most likely site where egocentric spatial relationships are represented in the brain. PPC cells receive visual, auditory, somaesthetic and vestibular sensory inputs, oculomotor, head, limb and body motor signals, and strong motivational projections from the limbic system. Their discharge increases not only when an animal moves towards a sensory target, but also when it directs its attention to it. PPC lesions have the opposite effect: sensory inattention and neglect. PPC does not seem to contain a "map" of the location of objects in space but a distributed neural network for transforming one set of sensory vectors into other sensory reference frames or into various motor coordinate systems. Which set of transformation rules is used probably depends on attention, which selectively enhances the synapses needed for a making particular sensory comparison or aiming a particular movement.

To help you decide whether you would be an appropriate commentator for any of these articles, a (nonfinal) draft of each will soon be retrievable by anonymous ftp from princeton.edu according to the instructions below (filenames will be of the form bbs.alexander, based on the name of the first author). Please do not prepare a commentary on this draft. Just let us know, after having inspected it, what relevant expertise you feel you would bring to bear on what aspect of the article.

To retrieve a file by ftp from a Unix/Internet site,
type either:

ftp princeton.edu
or
ftp 128.112.128.1

        When you are asked for your login, type:
anonymous
        For your password, type your real name.
        then change directories with:
cd pub/harnad
        To show the available files, type:
ls
        Next, retrieve the file you want with (for example):
get bbs.alexander
        When you have the file(s) you want, type:
quit

JANET users can use the Internet file transfer utility at JANET node UK.AC.FT-RELAY to get BBS files. Use standard file transfer, setting the site to be UK.AC.FT-RELAY, the userid as anonymous@edu.princeton, the password as your own userid, and the remote filename to be the filename according to Unix conventions (e.g. pub/harnad/bbs.article). Lower case should be used where indicated, using quotes if necessary to avoid automatic translation into upper case.

        The above cannot be done form Bitnet directly, but there is a fileserver called bitftp@pucc.bitnet that will do it for you. Send it the one line message
help
        for instructions (which will be similar to the above, but will be in the form of a series of lines in an email message that bitftp will then execute for you).

Date: Wed Jan 15, 1992 3:45 pm PST
Subject: Lost

From Tom Bourbon [920115 -- 16:35]

For various reasons, most of them having to do with our local computer facilities, I have been unable to access the networks since 10 January. The amount of mail I just found in my box, accumulated during that time, is overwhelming. I am certain I can never catch up!

From some of the earlier posts, I see that remarks by me, prior to LOS (loss of signal), were addressed by several of you -- Martin Taylor, Ken Hacker, Rick Marken and Bill Powers, among others. The lack of a reply from me did not reflect lack of interest, or of courtesy, on my part.

Now, with the volume of mail that has acumulated, I am reluctant to even try to pick up the threads on behaviorism and on social control.

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Date: Wed Jan 15, 1992 4:11 pm PST
Subject: imagination loop

[Martin Taylor 920115 17:00]

I have been trying to put my CSG house in order a little, by converting the last year of mailings into HyperCard stacks for the Macintosh. As a test, I looked through all the mailings on the "imagination loop," or that contained relevant words, and I find that there is only one place where that is described: a reposting by Gary Cziko in June 1991 (or thereabouts--I am not at the right place to check) of a mailing by Bill in Oct 1990. I looked for this, because at the time, I didn't understand it, and because as a consequence of my query last week about temporal effects I started looking for possible feedback loops within the hierarchy. There are none in the "classical" (Byte 1979) description, so I looked for places where useful feedback could be produced. One of these turned out to be exactly Bill's "imagination loop" in which the reference signal for ECS k is used instead of the perception signal used by ECS k. As Bill says, that replicates the effect of ECS k having performed properly.

(Aside: The issue was of how the perceptual input would work for a sequence controller, in context of the perception side of the net being seen as a multilayer perceptron. Bill pointed me to the shift register on p144 of BCP, which is one way of handling the issue, but which has been found to be inadequate for natural sequences such as speech. I won't go into that here, but might do so later).

In the Byte articles, Bill mentions something that, so far as I can remember, has not been mentioned explicitly since I have been reading CSG-L: that if there is a perceptual connection with a weight (wij) connecting ECS i's perceptual input to ECS j's perceptual transform matrix, then there is a reference signal connection rji connecting ECS j's output to ECS i's reference input matrix with a weight sgn (wij). Is this still considered to be true, Bill? It seems reasonable, in that it asserts that ECS j is affected perceptually only by signals whose value it can control (any other perceptual signal would have to be a disturbance, to be compensated by the ones the ECS can control, and it is unclear why internal disturbances would naturally evolve. It would also disagree with JGT's theory, which is permitted, but not desired by me!).

I would like to propose a change in the way ECSs are drawn, without in the slightest changing the actual connections among them. In the Byte article, an ECS has a group of perceptual inputs that go through some combining function that provides a single value that is used both as input to a comparator and as a feed to the perceptual input function of other ECSs. Similarly, there is a set of reference signal inputs that are combined in a summation matrix (vector, actually) having values of plus and minus unity, the total being fed to the comparator and the difference being passed through an output function befor being output as a simgle signal.

The change I would like to suggest (and would draw if we had a medium for it) is to place the reference signal weighting function in the output of the ECS providing the reference, rather than at the input of the one receiving it. The receiving ECS, in this view, would simply sum all the incoming reference signals positively before applying them to the comparator. No difference in function, only a difference in viewing what belongs to which ECS.
I actually have drawn this so that input Pij is drawn next to the reference signal line Rji, so that they look like a line-pair.

Why do I want to do this? Two reasons:

(1) The value of rji depends on the value of wij. wij is asserted to be in ECS j, and it makes for a cleaner description if the two are co-located, especially if we try (as I would like to do) to develop an object-oriented description of the hierarchy, in which an ECS is a natural candidate for being an object.

(2) If the imagination loop is real, then it must connect the reference signal AFTER IT IS WEIGHTED for input to comparator i so that it can substitute for the perceptual input to comparator i. This seems to require some kind of external control from ECS j, telling ECS i to send its reference, rather than its percept. If the reference weight matrix (rj.) is held together with the perceptual weight matrix (w.j), then the connection remains within ECS j. Furthermore, if one thinks of this as a semi-physical model, then the descending fibres (which Gary quoted David Hubel as saying match 1:1 with the ascending ones) would all be excitatory (or all inhibitory, depending on how you look at them), and would be aligned with their corresponding ascending fibres.

As I said, this restructuring makes zero difference to the function of the hierarchy, but I think it makes description easier, and computational production of simulated hierarchies also easier. And the "imagination loop" becomes a function of the ECS that is doing the imagining, rather than a cooperative process among two ECSs or something imposed on a higher ECS by a lower.

Martin

Date: Wed Jan 15, 1992 5:57 pm PST
Subject: Journal titles conundrum; CLOSED LOOP

From Greg Williams (920114-2)

[Gary Cziko 920114.2230]

>Greg Williams mentioned the following two articles and implied that they
>were in the _American Psychologist_. They are not.

Sorry about the confusion, Gary. Next time I'll put the name of the journal in the text of the post as well as in the subject heading to make it less of a puzzler.

INFO ON CLOSED LOOP

My sore throat/cold of two weeks ago became last week's tonsilitis which has become the killer ear ache of the present. So I missed the 1-15 deadline for getting Volume 2, Number 1 to Ed Ford for his mailing. Now I'm shooting for the middle of next week. Yes, the topic (due to Gary Cziko's long-ago request) is (non-recent postings on) "social control." Maybe a Part II in Volume 2, Number 2....

Greg

Date: Wed Jan 15, 1992 7:27 pm PST
Subject: Less testiness, more testingness, yay!

SORRY MY LAST POST WAS DATED 1-14; SHOULD HAVE BEEN 1-15 (I do FEEL at least one day behind!!!)

From Greg Williams (920115-3)

I think Bill Powers made some excellent points about the need to focus more on evidence in our threads. In the future, I promise to keep Bill's admonitions in mind.

A quote from that premier "fly-knower," Vincent Dethier, is appropriate in this regard:

> Theories are fine as tools to understanding but are not in themselves contributions to truth. Any clever scientist can sit down, marshall the facts at hand, and bounce out of his [sic] arm chair with a theory. The scientist who is great is the one who proposes a theory and then attempts to prove or disprove it rather than the one who proposes a theory and then goes off grinning to greener pastures leaving the onerous job of proof or disproof to others.
>
> - from TO KNOW A FLY

Greg

Date: Wed Jan 15, 1992 7:54 pm PST
Subject: Evolution

[from Gary Czik 92115.0830]

Mary Powers 920114

>One complex molecule accidentally
>configured just a little differently, so that it resisted
>dissolution -- one of billions that came and went, maintaining
>their integrity for shorter and longer periods of time -- but just
>one permanently successful one, that is not our ancestor, but is
>alive and well in the form of all living things today although
>bits and pieces die off all the time.

I can follow this, except for the part about this "control system molecule" NOT being our ancestor. Could elaborate on this a bit?--Gary

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Gary A. Cziko

Date: Wed Jan 15, 1992 7:57 pm PST
Subject: Re: Terminology

Ken Hacker [920115]

Bill (9290114) --

Bill, you have provided me with much food for thought. I am beginning to make my way through the "semantic chaos" a bit now and reaching what I think are key points of convergence in our views. I need about a day now to put together a succinct response. Thanks. KEN

Date: Wed Jan 15, 1992 8:18 pm PST

Subject: Re: Evolution

Ken Hacker [920115]

Mary Powers (920115) -

Sorry, Mary, I did not mean to sound defensive, but I am a rather outspoken individual and often hit the send button twice after thinking once! Being part of a discipline that is is young in the social sciences and diverse in its epistemological bases, I am looking for avenues of theory and research which make concrete useful connections to my own work and to the theories I am familiar with. I suspect that much of science as a whole is undergoing revolutionary changes and I look forward to the new directions for my own thinking and watch this, Bill, PERCEPTIONS.... :)   KEN

Date: Wed Jan 15, 1992 8:23 pm PST
Subject: externality, ecology, SR organization

Here is a stab at disentangling externality and ecology, and locating controlled quantities.

Externality:

Controlled quantities can be described in the external environment, at the input to the relevant comparator, or anywhere in between in the flow of information (energy?) to the comparator. They have to be described differently at each location, via a function that computes whatever happens on the way to the comparator. At the input itself, the function will be the identity, at the skin, whatever function is computed by the relevant portion of the hierarchy of sensory input systems, etc. So they are both internal and external, but calling for different descriptions at different places (something that Ned Block was confused about when he wrote the `problem of the inputs and outputs' section of his paper on Problems with Functionalism).

Ecology:

Sometimes it seems pretty clear that the wrong quantity is being controlled, often when a system moves into circumstances where the functions relating distal to proximal environment change (as in the spear-gun example). E.g. the function that is being computed coincides with the one that ought to be over a wide range of circumstances, but not all. For certain purposes it might then be useful to talk about the `condition' that the (now malfunctioning) control system ought to be `maintaining', but this isn't psychology, whatever it might actually be. Except that, failure of condition maintenance tends to lead to persistent error elsewhere, ...

Condition maintenance is supposed to replace my former notion of `e-control', and, since it isn't control, it can sometimes be achieved (albeit in a primitive way) by

S-R systems:

These can't effect control, but they can to a certain extent maintain conditions, although not very well. E.g. they tend to be used to fire up capacities under circumstances where these capacities might be put to use. Has anyone thought about the famous fight-or-flight response that pop psychology wants to identify as the root of all our modern problems? I'd conjecture that S-R organization will be pretty much restricted to circumstances where false positives don't occur (in the normal environment), or don't matter very much when they do occur. Has anyone analysed frogs and flies in PCT terms?

Avery Andrews

Date: Wed Jan 15, 1992 9:22 pm PST
Subject: e-control; likelihood; testing

[From Bill Powers (920115.2200)]

Avery Andrews (920114 etc.) --

Avery, concerning e-control and p-control, Rick Marken is right. But there's a question of practical epistemology here. When we make models, we are modelers relative to the organism, but naive realists relative to the environment. We pretend to be able to know what is going on outside a nervous system without having to use our nervous systems. This is a convenience, because mentioning the "physics model" every time we talk about external happenings would get very tedious, and it isn't the physics model we're working on. Rick was correct in saying that the PCT model takes the "ecology" into account, because the properties of the environment that make input depend on output are an explicit part of any working model of the organism's behavior, and effects of other variables in the environment are explicitly brought in as sources of disturbance. Of course we're not yet at the point where we can deal with a whole "virtual reality" in our models, but presumably we're headed in that direction. Modeling the environment, in fact, is the most complex part of any model of behavior. In my arm model, 90 percent of the core computations (other than presentation) are a model of the environment. The control system part is almost trivial -- a few lines of code.

Martin Taylor (920115) --

>I have a quibble about an internal inconsistency (probably more apparent
>than real) ...

I said that reference signals were representations selected internally, while perceptions were real-time representations. This seems to leave out memory. The apparent inconsistency is, as you suppose, apparent only.

What comes out of memory (hypothesis) is a playback of a recording of a perceptual signal (at least at higher levels). That playback is the reference signal. The great advantage of this arrangement is that it guarantees that the reference signal will represent the same kind of thing that the perceptual signal represents. Further, if any sort of translation is needed from the higher-order error signal to the lower-order reference signal, it is done by letting the higher system's output signal (derived from the error signal) select memories of lower-order perceptions -- a sort of table-lookup translation. Lots of complications here, of course, that I don't try to tackle. Just half-close your eyes and stand back a few feet and it will look perfectly clear.

In your post to Bruce Nevin you speak of subjective probabilities in a new way. I had been thinking in terms of signals that are fuzzy functions of other signals, not in terms of subjects specifically perceiving likelihoods. When you talk about them that way, I am immediately reminded of the "principle" level, which is supposed to cover perceptions that are generalizations, averages, heuristics, and other probabilitistic things like that. I think that when a person is trying to judge the chances that one perception rather than another is present, the perceptual process is very much what I have in mind for the principle level -- not a specific program or sequence or category, but a generalization about such things. Likelihood strikes me as the right sort of thing to belong at this level, and probabalistic processes the right sort of operation for deriving perceptions of this kind.

Of course this means that lower-level perceptions are NOT of that kind. This gets us back to a problem that's come up three or four times on this net: the danger of applying too high a level of the observer's perceptions to the operation of low-level systems in the model. Remember, for one or two posts a while back, Bruce was trying to see categories in all the levels. Or was it Gary. And people keep getting hung up on relationships, or sequences, because they can look at ANY level from those points of view and see that what's "really" going on is relationships and sequences, even at the intensity level. But this HCT model is parsimonious: if a certain kind of perception is the business of a certain level of organization, that is the ONLY level where the perceptual signals explicitly represent that kind of perception. Our problem is that we can't help using all our levels, even when we're considering only a low-level kind of perception or control system. So I offer for your consideration the idea that perceptions of likelihood and probability belong at the (possibly misnamed) principle level, while appearances of likelihood calculations or probabilistic properties in the operation of lower levels result from looking at them from your own principle level. The lower levels don't work in ways that would yield *explicit* perceptions of probabilities or likelihoods.

I want to talk about testing some more. One thing the control system model offers is a way to test the idea that an organism is perceiving any particular kind of variable. There's no cookbook way to do this -- you have to be a little ingenious to translate into a doable experiment -- but when you manage to apply this test it not only tells you what the subject is perceiving and verifies what the reference level is (you usually have to suggest a specific reference level or at least ask the subject not to change it), but it tests the control theory explanation itself every time you apply it.

It is, of course, the Test for the Controlled Quantity (or Variable). Suppose you want to test for the idea that people actually can perceive something that you define as likelihood. The first thing you must do is think up a situation in which likelihood can be perceived, at least by you -- and not just likelihood, but different degrees of likelihood. Then you have to think of some action the person could produce that would vary this "likelihood" over some reasonable range, the larger the better. And finally, you have to think of a way to get the person to maintain the perceived likelihood at specific levels (say between unlikely and certain) by using the variable action you have provided for. Also, if you can manage it, you should think of some way to disturb the likelihood independently of the subject's action, so you can verify that the effects of disturbances are cancelled by equal and opposite changes in the person's actions.

There are no rules saying you have to do this with control handles and moving spots of light. You can use language, drawings, anything that will work. You could, for example, use 7-card stud and verbal estimates of the likelihood of winning as new cards show up, or are substituted. All that is essential is to make sure you are using VARIABLES and not just CONDITIONS or THINGS. It's not necessarily easy to find a good experiment -- you may have to try a number of them to get one that will work with everyone. But the nice thing about CT experiments is that you can use yourself as a subject to refine the experiment until you know it's about right -- and it will then work with every other person. And the other nice thing is that when you do get it working right, your data will be absolutely beautiful (if your hypothesis about the controlled variable is right). Well, that leads to the third nice thing: if you've guessed wrong, and know what to expect when you've guessed right, there will be absolutely no doubt that your hypothesis has bombed. You'll get correlations down around the kind that get published.

Everybody:

Rather than get into more and more complex and abstract implications of the meaning of HCT, I would rather see the older hands at this game start trying to apply CT seriously to testing some hypotheses that they're interested in. I don't mind at all going on with explanations and introductions and clearing up misunderstandings about control theory and its details. I don't mean to shut the door on anyone, especially newcomers. This "Master Class" also admits beginners and disbelievers. But a lot of our discussions really go beyond the point where control theory has any answers -- without some real experimental tests. You can tell that I don't mind wild conjectures, but that's recreation. I want to see some serious work started before I begin to lose me prowess, in fields where CSG people haven't yet begun anything. The doors are wide open.

Of course you will do as you choose, and of course I'll go along with it as long as I have the impression that people are catching to to CT. I've stated my preferences. And my co-gurus may have different ideas.

Best to all

Bill P.

Date: Thu Jan 16, 1992 5:23 am PST
Subject: testy probabilities

[From: Bruce Nevin (920116 0746)]

(Martin Taylor 920115 16:15) --

Hoo boy! I'm going to back off on this one for a while, most immediatly because I've two books due to press yesterday, and also because we seem to be going so wildly wide of one another's respective marks.

I do want to affirm that my quoting Tom was in no way snide, and if in the attempt to be terse (and in haste) anything like that tone could be interpreted in my writing, absent body language, I do apologize.

It would be nicer for me if some monitoring and evaluating of probabilities of different values of sensory input p were an intrinsic part of every ECS. The relation to Harris's use of likelihoods as a diagnostic in linguistics would then be simple and direct. But it seemed to me that there is no *mechanism* within the ECS whereby probability (the probability of one value of p as compared with other values) itself becomes a neural signal.

There may be mechanisms other than individual neural signals, such as the strengthening of associative memory of a value of p, or the colligation of signals to the input function for p or for r (where much of the magic of control resides, seems to me), or in a related way colligative properties of many ECSs on different levels, or some kind of priming by neuropeptides in the immediate intracorporeal environment of ECSs, and so on. But such mechanisms are not related in any obvious way that I can see to the *perception* (a neural signal) that one perception is more likely than another, e.g. abduction in a UFO.

Probabilities, it seems to me, are related to how control systems control, without the control systems controlling for probabilities themselves. (In order it to control for a probability, the probability would have to be a perceptual signal entering a comparator.) As such, they are a proper part of a metascience about control modelling, without being a functional element within any model. In a similar way, channel capacity is important for understanding the limits of control systems and limits on possible varieties of control system design, without itself being something that control systems control for. (I read Bill's 920114.0800 belatedly on the train home last night, and this is my understanding of his comment about information-theoretic concepts and "metadiscourse".)

I quoted Tom Bourbon because it seemed that his reasonable request indicated a good and proper way to ground our discussion of information theory and probabilities in the testing of models.

Bruce
bn@bbn.com

Date: Thu Jan 16, 1992 9:35 am PST
Subject: Re: testy probabilities

[Martin Taylor 920116 11:45]
(Bill Powers and Bruce Nevin 920116 or nearly)

I seems to have said something that misled bith Bill and Bruce into thinking that probability could be considered a percept subject to control. I didn't mean to induce that impression. Sorry. (Of course, as Bill mentions, it could become a percept at some level program or above). It is ordinarily manifest in the behaviour of one or both of the perceptual input matrix or the comparator, I think. (Maybe the output transform, but that can be subsumed under comparator, perhaps).

Martin

Date: Thu Jan 16, 1992 10:37 am PST
Subject: re: testy probabilities

[From: Bruce Nevin (920116 1213)]

(Martin Taylor 920116 11:45) --

That was the problem.

Let that be a lesson to us! (Probably.) :-)

But I'm still puzzled how subjective probabilities are manifest in the comparator or elsewhere if they are accessible neither to an outside observer nor to the ECS itself nor to another ECS in the same control system. I understand intuitively that some signals are more probable than others and that probabilities presumably change in ways predicted from communication theory, but I can see no way of verifying that or operationalizing the concept within control theory.

And I do regret that Harris's likelihood-differences are not available within a model.

Our assigning of probabilities to perceptions, either intuitively or by statistical studies, is as you said arrived at by higher level processes such as logic--probably!

Bruce
bn@bbn.com

Date: Thu Jan 16, 1992 2:08 pm PST
Subject: COLLECTIVE ACTION; MULTIPLE PERSON RUBBER BAND DEMOS

<<<<<*****(((((FROM CHUCK TUCKER 921016)))))*****>>>>>
CGS-EM20

Re: Collective Action; Multiple person rubber band demos

On collective action---

I have not read all of the recent statements on social control but plan to do so this weekend and hope to have something to say about the issues especially those raised by Greg and Rick. In the meantime I just will repeat what Clark and I wrote in our article in the "Control Theory" issue that Rick edited. We point out three ways in which collective action takes place with living control systems (there are other ways but these are just elementary one) "1. Two or more persons can <<independently>> generate similar reference signals and make adjustments (to control their perceptual signals) yielding very elementray forms of collective action. 2. Two or more persons confronted with a mutual problem can <<interpendently>> generate similar reference signals and make similar or differentiated adjustments yielding somewhat more complex forms of collective action for a relativel samall number of people. 3. Two or more persons can adopt the reference signals developed by a <<third party>>, along with directions for similar or differentiated adjustments that, in combination with third party preparations and arrangements, can yield very complex forms of collective action for larger number of persons." Then in that article we provide examples of research that we or others have done which illustrate each one of these ways of generating collective action. And, as some of you know, CROWD V2 (now referred to as GATHERING) is a program by Bill which illustrates #1 above and by it some very complex forms can be illustrated without the control systems being able to interdependently generate similar reference signals. Thus, CROWD V2 can be used to illustrate control but can't show social influence.

On collective action rubber band demos---

I have thought some about how to devise rubber band demos for more that 2 persons (Bill and Phil and I all have written about 2 person demos) and have come up with some ideas that all of you could have (and perhaps have) with a moments reflection. Actually the ideas for these came from a discussion I recall at one of our meetings which was given by Ed Ford and his friend, Jim Soldani who used such demonstrations with people in organizations to show how conflict can arise if people don't understand one anothers' purpose. These demos can be used to illustrate PCT (obviously) as well as what many have called "social constraints" or "social structure" which are in my view mainly arrangements that someone devises to make it extremely difficult for a person to accomplish her purpose in addition to illustrating conflict and conflict resolution. In these demos you can give the instructions in verbal or written or graphic form or all of these forms, you can have the participants talk to each other or not, you can be one of the participants, you can try to restrict the "sensory input" of the participants by using screens, blindfolds, heavy gloves on their hands or have them hold the rubber band with a hook instead of their finger directly. All of these variations I can see as an attempt to illustrate different aspects of the model.

Let me illustrate with a 3 person demo. You will have three rubber bands all knotted on another one at equal distances from each other (oh, for that graphics program!). Using the picture of a circle with 360 degreees and treating a line intersecting the circle anywhere as 0 degrees tie rubber bands at 0, 120 and 240 degrees equally dividing the center rubber band into thirds. Now have the participants make triangles of various shapes, have one participant refuse to make a triangle with the others, have them make a triangle and tell them "Now, hold that position for 10 seconds and remember how that felt because I will ask you to do it again without being able to see what you are doing." Then blind fold each of them, place the rubber band on each ones finger and ask them to "Make that triangle again." (Take a picture of both performances so you can compare them). Now the parameters (literally) on the activity can be changed by tieing 3 rubber bands at 0, 160 and 200 degrees; then at 160, 180 and 200 degrees and so on. When these "structural conditions" are set then the types of shapes that can be made will be restricted UNLESS the paticipants devise ways (like crossing over each other's rubber band) to make the shapes BUT if they are all required to stay on the same plane (another structural condition) then the shapes they can make will be limited.

Now expand this to 4 participants and start with the simple one of a rubber band at 0, 90, 180 and 270 to make a square or other square-like shapes; ask the participants to make a triangle; to make a circle; to make a hexagon and on and on and on. Do 5, 6, 7 and on and on and on and long as you have rubber bands. (Maybe someone should start "Control Theory Rubber Band Company" with rubber bands of all different sizes, shapes and colors - how about you Bill, you are unemployed !?!)

I believe that these demos will not only be very useful in illustrating the ideas of PCT but are enjoyable and memorable for the participants. Clark told me that his students in his classes were very excited with the rubber band and coin games as well as Bill's demo. I will let you know how these "collective rubber band demos" work out this semester and I hope you will let me know your experiences with them if you try them (it seems to me that such experiences would by very useful in a book about PCT, don't you Rick).

More later.

Regards,
Chuck

Date: Thu Jan 16, 1992 2:54 pm PST
Subject: testing

[From Rick Marken (920116)]

Bill Powers (920115.2200) says:

>Rather than get into more and more complex and abstract implications of
>the meaning of HCT, I would rather see the older hands at this game start
>trying to apply CT seriously to testing some hypotheses that they're
>interested in.

> I want to
>see some serious work started before I begin to lose me prowess, in
>fields where CSG people haven't yet begun anything. The doors are wide
>open.

I'm game. My problem is figuring out what might be the best thing to spend time on -- there are so many possibilities. Here are some of the things that I am taking into account when deciding "what to do next" in terms of testing PCT.

1. I work alone so whatever I do is limited by my own meager skills. This means, for example, that it's easier for me to do projects on the computer than ones that require building hardware. This is unfortunate

because I have some ideas that require that I build electronic devices (for example, I want to study the ability to detect sequences of presses applied to the finger tips -- and compare this to the ability to produce finger press sequences).

2. I want to do things that will not only test the model but also show its generality (in terms of dealing with "higher level" kinds of behavior). I would like to think of tests of control of higher level variables -- programs or even higher -- but I don't know of a convincing way to do it. You (Bill) have made some excellent suggestions to me regarding possible research projects. I like the recent suggestion about studying the ability to control variable that might be called "likelihoods". But it's hard to settle down and decide which "test" to spend time on.

Well, there are probably other bases for my internal conflict, but that's enough for now. I think it might help me decide what to test if I could hear your thoughts (Bill) about which aspects of the model YOU would most like to see tested -- after all, it's your baby. I like the role of worker bee -- all this thinkin' sometimes makes my head hurt.

By the way, this also might be a good time to repost the list of variables that have been determined to be controllable by human subjects.

When my collection of research papers is published (hopefully, later this year) it will serve to give a small glimpse of the kind of research that can be done to test PCT. Maybe its time for someone (Tom B., Gary C. Wayne H., Bill, etc) to think about writing a textbook on "Research methods in control theory"? It might sell over 100 copies.

Chuck Tucker (920116) -- great demos. If PCT ever catches on you can bet I'll be putting a nice hunk of dough into the American Rubber Band Company.

Best Regards

Rick

Date: Fri Jan 17, 1992 2:01 pm PST
Subject: Clarifications: multiple rubber band demos

[<<<<<<==FROM CHUCK TUCKER 910117==>>>>>>]
My last post on this topic was not clear according to Clark. For a 3
person demo it requires 4 rubber bands (called rb henceforth); 1 rb for
the "center" (which is used to make the shapes) and 3 - one for each
person. So you have 3 rb tied to another rb in the "center."

My "degree" specifications might be better understood if converted to a
"clock face." Imagine the "center" rb as a circle forming the edge of a
clock. For a triangle shape the rb would be ties at noon, 4:40 and 7:30.
Now to place "constraints" on the action of the participants in this
experiemnt have the rb tied at noon, 5:30 and 6:0 and ask the to make the
SAME EXACT SHAPES they did whith the revious arrangement (above). Then
tie the rb at noon, 3 and 9 and ask them to replicate the shape from the first
experiment. You do get the idea, right.

For the 4 person experiment begin with 4 rb tied to another at noon, 3, 6,
and 9 o'clock and ask for a square, a rectangle, and squarelike shapes then
present them with the most "constraining" arrangement 2:59, 3:00, 3:01 and
3:02 and ask for a square.

Y'all should have the idea on this one; there are many, many very interesting
variations on this that can be very useful to illustrate PCT.

ANOTHER DEMO WITH A COMPUTER WORD PROCESSING PROGRAM

You may have noticed the designs that I use now to mark my post. I noted that
Bruce used such designs, e.g., <<<+++===+++>>><<<+++===+++>>> and so I now have
a file of them that I am using in my reader to mark of comments, questions for
the students when the read a paper. Today I thought - these could be used as
an experiment on feedback or testing an "open loop." Set up the computer with
your word processor ready to type. Tell another that you want them to type
a particular design and show them the design on a piece of paper. Let the
person practice the keys (not the design) [you may just want them to practice
the keys you know will be used in the design before you show them the design]
Ask them to type the design on the screen several times while you time the
task (do not let them correct their typing). Save that record. Then ask them
to do it with the computer screen either covered or turned around so they can't
see it. Time this and save the record. Finally, ask them to do it without
the visual example of the design AND without the screen. Time and record.
There is much "data" to examine here wouldn't you say. Let me know how it
works out.

Regards,
Chuck

Date: Fri Jan 17, 1992 2:03 pm PST
Subject: Re: Distributed coordinated control

[Martin Taylor 920117 14:00]
(Rick Marken 920117)

>
>
>1. I think your representation of the imagination connection may be
>incorrect. I don't have my copy of BCP handy, but it seems to me (and
>this is the way I implemented it in my spreadsheet model) that the
>imagination connection is from level n reference to level n perception
>(the system gets a perceptual signal that IS its reference signal -- it
>is perceiving what it wants to perceive). You have a component of
>the level n reference becoming a component of the level n+1 perception.
>I think this is an interesting architecture but I'm not sure what it maps
>to in experience.

Well, it just goes to show that a diagram or two is useful now and then. There are indeed two possibilities: "I imagine I have done the right thing and I pretend to you who asked me that I have" vs "I imagine that you whom I ask to do X have done the right thing." If the former is what people have been talking about as the imagination loop, it has passed me by over the year I've been reading CSG-L.

Let's see if there are any consequential differences, because each kind of imagining is possible, and both could actually exist in the same hierarchy, and indeed in the same ECS (as I redrew the ECS).

I will anthropomorphize a bit. It seems to make talking about these things easier. One ECS is an officer, who requests a servant to cause some percept. In the form of the imagination loop suggested by Rick as having been the one everyone was talking about, the officer says "Make X be true" (in military terms, "take that hill from the enemy"). The servant says back "OK, X is true" but does nothing about making it be so (OK, gheneral, the hill is taken). That deasn't seem to me to be very healthy for the officer (the general thinks "Now I can send my troops through the valley safely").

In the version of the imagination loop I thought had been intended, the officer says to himself "How would things be if X were true", and the servant isn't involved. (General: "Would it be a good idea to have that hill taken?"). This seems healthier, and can be elaborated into the kind of planning activity that seems a little hard to incorporate smoothly into the other model.

On a more abstract level, my non-standard version of the imagination loop seems to have some desirable properties, such as providing a direct source for control of training the perceptual function of an ECS. The substitution of the outgoing reference signal aimed at ECS j for the perceptual signal incoming from ECS j allows the officer ECS to determine whether ECS j is being asked to do a useful thing, given the current perceptual function of the officer, independently of whether ECS j is capable of control under the current environmental constraints. I haven't followed this through, yet, but it does seem to provide the possibility for directed reorganization and the rapid development of useful perceptual functions, a possibility that is absent in the standard imagination loop. All the standard loop says to me is that the ECS can say "I'm too lazy, boss. Let's not do it and say we did."

Laziness is not always a crime, though.

Martin

Date: Fri Jan 17, 1992 3:51 pm PST
Subject: imagination

[From Rick Marken (920117b)]

To Martin Taylor (920117 14:00):

I see your point. Let me just make some quick comments about imagination as it might be represented in your nice diagrams. Let's call the ECS at the bottom system n and the one at the top system n+1. As the PCT model currently sits a system is in "imagination mode" when the reference input to its comparator goes directly into the perceptual signal channel going to systems at higher levels. The way Bill drew it in BCP (I think), when in imagination mode, system n's comparator gets no reference OR perceptual input. So there is no error in system n and no change in output. Imagination takes system n out of the hierarchy (as a function control system).

The reference input to system n is determine by systems at level n+1. The perceptual signal going from system n to system n+1 is just one of the perceptual components of system n+1's perception. Other systems at level n+1 also get this perception (n) as a component. This perception (n), in imagination mode, is the perception that has been requested from the system at level n by all systems of level n+1 that use it (because it is an imagined perception -- a playback of the net reference input to system n -- which requests a perception from system n). So if only one system at level n is in imagination mode, then its perceptual signal (n) is just what it wants it to be -- the reference signal (although no perceptual signal is entering the level n comparator -- unspoken is the question of what it means to "have" a perception. I believe PCT would say that a perception is a signal in a perceptual neuron. So if the reference signal instead of the output of the level n perceptual function enters the perceptual neuron at level n (labelled "percept" in your diagram) then that is the perception you have.) For the systems at level n+1, which get the perception at level n as a component input, only part of their perceptual signal depends on an imagined perception. So the perception at level n+1 is partly imagined. This happens all the time, of course: much of what we perceive and control is imagined. Missing misspellings while reading is a good example.

Note that this model still says nothing about what places systems into imagination mode and what takes them out. What we need are some data about imagination in well understood control tasks. Phenomena first -- then we can tweek the model as necessary.

Hasta Luego

Rick

****

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Date: Fri Jan 17, 1992 4:41 pm PST
Subject: Bourbon/Powers article

[From Rick Marken (920117c)]

My computer plans to by unconscious for a couple days so I thought I would put this up real quickly before I leave work. I just received a copy of a paper by Tom Bourbon & Bill Powers called "Models and their worlds". I read it just now. I will probably read it many more times in the near future. It is extraordinarily great. A classic. Beautifully written, complete -- extraordinarily on-target and recent refernces (many articles cited are 1991). I want to thank Tom for sending me a copy. It is a GREAT paper.

The paper will be sent to a journal but, apparently, previous versions have been rejected by other journals. I probabaly won't get to see an answer to this question until Sunday if(at best) but I've got to know from Tom or Bill -- Which journal(s) rejected this masterpiece and what were the reasons?????

What a gorgeous piece of work, Tom and Bill. I am green with envy (I wish I could have written such a nice piece). I feel a bit like Salieri listening to Cosi Fan Tutti. Kudos.

Respectfully

Rick

****

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Date: Fri Jan 17, 1992 6:08 pm PST
Subject: Re: imagination

[Martin Taylor 920117 18:40]
(Rick Marken 920117b)

I don't think there is a great deal of difference between the two imagination connections in the eventual result (but who knows for sure-- it needs modelling). But I think there is another explanation for:

> For the systems at level
>n+1, which get the perception at level n as a component input, only
>part of their perceptual signal depends on an imagined perception. So

>the perception at level n+1 is partly imagined. This happens all the
>time, of course: much of what we perceive and control is imagined. Missing
>misspellings while reading is a good example.
>

The alternate explanation is that one element of the perceptual function is the correction of such errors as can be corrected using the redundancies in the pattern to which they are exposed. A misspelled word is seen as its correctly spelled equivalent because that is what is output by the perceptual function, not necessarily because it is imagined to be correct.

But your general point is quite valid. A good example is that a poor pianist like me will often hear what he intended to play, while driving other people out of the room. It takes hearing some tape recordings of one's own playing to be able to hear what one actually does, and thereby to improve it. I think this is a better example of the use of the imagination loop as a substitute for perception.

Martin

Date: Fri Jan 17, 1992 8:41 pm PST
Subject: Model architecture; actrons; rb experiment

[From Bill Powers (920117.2200)]

Rick Marken and Martin Taylor (920116) --

I think that Martin's proposal about the imagination connection is the correct one. Rick, in your models, the output signal from each contributing higher-level system gets summed at a lower-level comparator to yield a net lower-order reference signal. The net reference signal, however, is not what is required for imagination because it's affected by other sytems, too. EACH copy of the output signal has to be short-circuited to the appropriate input port of the input function (multiple inputs). Not only that, in general the signs needed on the various fanned-out copies of the output signal will not all be the same. Thus as each copy is fed back in the imagination connection, it has to have the correct sign applied to it, to compensate for the sign it gets as it enters the input function. Unless we want to have to coordinate two sets of signs, Martin's way is best. I think it's best to bet against duplication of function. Also, it's far better that the act of imagining be done knowingly -- the higher systems need to know whether the information they're getting is coming from outside or is being pretended.

An interesting feature of this hierarchical model -- once we start recognizing a multiplicity of systems at each level -- is that the reference signal of a lower-order system does not correspond to the output of any one higher-order system. In the imagination mode, however, the higher systems take no account of the other systems at the same level. So life is very much easier in the imagination mode. In fact, the higher system acts as if it were the ONLY system setting reference values for the subsidiary systems. I think this is very realistic. We imagine complicated plans, but when we put them into practice we find all sorts of interactions with other goals that weren't apparent in the imagination mode. The fatal weakness of the person who thinks he can plan out every detail in advance is that none of the interactions with unmentioned goals can be seen without actually acting. We might say that this is the main weakness of all planning.

Rick says, correctly, "The systems themselves cannot decide to imagine (in the current architecture of the model)." I think the decision to imagine has to come from a higher-level system. So, at least in the backs of our minds, why don't we add another signal connecting higher systems to lower, that operates the imagination switch? Just knowing that that signal is available may help us think of circumstances under which it might be needed. The higher system says to the lower, "Go to imagination. Now let's see what happens when I set reference signals this way, assuming that they could actually be attained." Maybe this is what "assuming" means. I don't know what would generate this signal -- maybe there would be systems that operate solely through this connection on lower systems: mode-control systems.

Re: zeroings.

I'm glad you're taking my (implied) side of the argument, but let's not include the infant in the effluent. There's an operational side to zeroing, in which we don't bother to repeat information that's recent in the memory of the listener. While I find the concept of literal zeroing of something that was previously there unconvincing (an alternative is that it was never there), we still have to figure out how implicit references (or whatever the problem should be called) are handled. The expansion-and-zeroing scenario is trying to describe something real. I think there's another explanation for it, but I'm positive that I, with my amateur linguistics, will not be the one to find it.

Re: output "actrons".

I'm not against this idea, Martin. I have probably gone overboard in demonstrating control systems with essentially no output computations in them, simply to emphasize that the input side determines what is controlled. I've considered it more important to communicate that message than to show really snappy control systems. In fact, the hierarchy will work better at all levels if conflict among independent control systems at each level is minimized -- if the actions, as well as the perceptions, are orthogonal. That requires choosing more than the sign of the output; it requires weighting the outputs. This weighting can be far cruder than it is at the input side where the form of the controlled variable is being defined. But the payoff of output weightings (and dynamic tailoring) is to reduce the disturbing effects of one system on another. I've dealt with the worst case first, showing that independent control can be attained without any output weightings at all (other than positive and negative 1). This suggests that perhaps output weightings begin as simple sign choices. But if there is unacceptable interaction between control actions, maybe reorganization could start fiddling around with those output weights to refine them, reducing overall error in all the interacting systems.

Chuck Tucker (920115 etc) --

Nice rubber band demos -- sounds like the basis for a clever little book
that might amuse and educate.

Perhaps what you didn't make totally clear is that the central rubber
band is opened up to make a circle, with the others knotted around the
single strand at different positions. I initially tried to imagine the
center rubber band as a double strand like the others, but it didn't fit
your description until I realized you were using the middle one
differently.

Best

Bill P.

Date: Sat Jan 18, 1992 1:02 pm PST
Subject: ANOTHER DEMOS WITH THE WORD PROCESSOR

[[[[[[[<<<<<FROM CHUCK TUCKER 920118>>>>>]]]]]]]

Y'ALL:

I thought of another demo today but I don't know how to set it up. My
bet is that you computer programers can do it. Since I make so many
typing errors I don't know why this did not occur to be before. But this
demo would make errors in typing in spite of the person's typing and when she
tried to correct the letter the program would, after a 2-3 second delay, change
the letter to an error. Of course, the person's purpose would be (like most
typing tests that I would fail) to type a statement in a certain period of
time. You should program the "letter changer" so that the task could be
accomplished (mainly so someone doesn't destroy your computer) but also for
ethical concerns. Variations on this would be whole word changes, indentation
introduced into the text, spacing introduced into the test and then the text
returns to "normal", commas inserted in the wrong place like the one I just
typed after the word `normal,` and a host of errors that should be corrected
by the participant. [You may have noticed that there times that I leave off
"s" and "d" at the end of words - that could be done]

Why am I beginning to feel like Allen Funk?

Regards,   Chuck

Date: Sat Jan 18, 1992 5:29 pm PST
Subject: Re: Model architecture; actrons; rb experiment

[Martin Taylor 920118 20:10]
(Bill Powers 920117 22:00)
>
>
>Re: output "actrons".
>
>I'm not against this idea, Martin. I have probably gone overboard in

>demonstrating control systems with essentially no output computations in
>them, simply to emphasize that the input side determines what is
>controlled. I've considered it more important to communicate that message
>than to show really snappy control systems. In fact, the hierarchy will
>work better at all levels if conflict among independent control systems
>at each level is minimized -- if the actions, as well as the perceptions,
>are orthogonal. That requires choosing more than the sign of the output;
>it requires weighting the outputs. This weighting can be far cruder than
>it is at the input side where the form of the controlled variable is
>being defined. But the payoff of output weightings (and dynamic
>tailoring) is to reduce the disturbing effects of one system on another.
>I've dealt with the worst case first, showing that independent control
>can be attained without any output weightings at all (other than positive
>and negative 1).
>

Actually, I didn't think I was altering anything in what you have previously said or written (said = e-mail, written = print). All I intended was to point out some implications that I had just seen and that no-one had mentioned in what I have read.

You did pre-empt me, though, in that I had expected later to bring up the question of whether the reference weights might not be learned in conjunction with the perceptual weights, rather than (as the Byte paper argues) just be controlled in their sign by the perceptual weights.

I have no idea how sensitive a coordinated action network would be to the relative reference weightings, and I would love to be able to try it out someday. But it's not realistic for me to try such a thing right now. All the same, this new insight seems to bring the PCT structure much more closely in line with my Layered Protocol model for intelligent communication, if things really do work as my intuition says they should (as you said, again pre-empting me, conflicting imaginations are not tested against one another, and may not work in the real world). And all this without changing a jot or tittle of the PCT model or the Layered Protocol model!

As for higher control of what is imagined, I tried a subjective, old-fashioned introspective "experiment" of imagining hitting a golf ball at different levels of abstraction ranging from a successful shot with the ball soaring away, through a nice easy swing, down to the muscle tensions in the swing. The latter kind of imagination is often suggested by sports coaches. It is quite easy to "control" these different levels of imagining, so at least subjectively it seems as if there is some kind of higher-level control of whether low-level systems perceive or imagine.

Martin

PS. I too am getting good "reinforcement" from your comments. Thanks.

Date: Sat Jan 18, 1992 5:36 pm PST
Subject: Re: ANOTHER DEMOS WITH THE WORD PROCESSOR

[Martin Taylor 920118 20:25]
(Chuck Tucker 920118)

Chuck,
Your typing experiment is what I experience trying to post from home. Luckily the error rate is low enough that I can do it, but it ain't easy some days!

How would you instrument this error-introduction problem as an experiment to show something of the subject's control structure? What new tasks would you expect the results to generalize to?

Martin

Date: Sat Jan 18, 1992 7:34 pm PST
Subject: Motor Commands & Behavior

Bill Powers (920111) said:

>There is, in general, no important correlation between "behavior"
>and the outputs of the nervous system.

This statement is still haunting me. As I already commented, this would have appeared to me as sheer nonsense three years ago; today it's almost frightening to realize that it makes sense to me and yet I can only imagine that it would still would appear as sheer nonsense to just anyone who doesn't understand PCT.

So my question is this. Have there been studies done to show that this is the case? And if not, why not? Is there a way to record the intensity of neuromotor commands to a muscle as a limb is moved? If so, what would be recorded from shoulder muscle as you kept your weighted arm extended in front of you and kept it there until you could no longer maintain it? Would the "crackling" of the neurons steadily increase in spite of the fact that the arm was not moving? Then what about moving the arm up and down both unconstrained and then with added disturbances (rubber bands; body moving up and down in a bumpy car, airplane, or roller coaster)?

I think it would be a real eye-opener to many if it could be clearly shown that "there is, in general, no important correlation between "behavior" and the outputs of the nervous system." So if this has been shown, why aren't we using it as PCT ammunition? And if it has not been shown, what are we waiting for? (Larry Goldfarb: Is this the type of thing that could be done in your department of kinesiology?).--Gary

P.S. I remember reading somewhere sometime that as muscles fatigue they need more of a electrical jolt to get them to respond. This makes some intuitive sense to me (they certainly need more of a mental effort; that last situp takes a lot more volitional juice than the first one), but I don't understand the physiology. I would appreciate it if someone out there could make this understandable to me (particulary since gasoline and electrical motors don't seem to work this way).

Gary A. Cziko

Date: Sat Jan 18, 1992 7:34 pm PST
Subject: Deceptive Hand Study

[from Gary Cziko 920118]

I ran across an interesting study the other day. I want to use it to see if PCTers can "predict" the study's findings. If you know the study, please don't respond and give it away.

Method:

A subject is given a pencil and asked to draw a straight vertical line (let's say from north to south).   The subject must look into a box to see his hand as he draws. But, unknowst to the subject, he does see his own hand but another person's hand (both are gloved to make the deception easier). As the subject attempts to draw the straight line, the stooge's hand draws a line that starts out north to south but then curves to the west.

Question:

1. What does the subject draw?

2. What does the subject experience?

I will give PCT 10 points of credibility if more than half the respondents come up with the right answers.

--Gary

Date: Sun Jan 19, 1992 2:31 pm PST
Subject: Miscellaneous

[From Bill Powers (920119.1300)]

Martin Taylor (920118) --

>The alternate explanation is that one element of the perceptual function
>is the correction of such errors as can be corrected using the
>redundancies in the pattern to which they are exposed. A misspelled
>word is seen as its correctly spelled equivalent because that is what is
>output by the perceptual function, not necessarily because it is
>imagined to be correct.

I think you're right, but you've inserted an unnecessary step. You're proposing, I believe, that the mistaken pattern is presented and perceived, then the redundancies allow detection of the error and replacement of the erroneous letter with the correct one, and then -- what? This method implies that the result is now perceived AGAIN as a correctly spelled word -- but there's still no perceptual signal indicating that the word is present.

All that's really needed is that the perceptual function respond to the correctly spelled word, and (perhaps to a lesser degree) to incorrect spellings up to some limit of atrociousness. The ability of an input function to respond to incorrect spellings might rely on some sort of redundancy in the word -- but it's not necessary actually to make the substitution and re-perceive the word unless you're typing the word or unless you just can't recognize it. The imagination-mode explanation would work. How would the recognition-of-redundancy explanation work?

Another thought. While we can recognize "Mississi" as "Mississipi," and while it's true that this word is highly redundant in its spelling, the *mechanism* for putting up the recognition flag before the whole word has been heard may not depend on redundancy at all. I can imagine a program (and actually wrote one once) that would follow a string of letters as it appears, eliminating alternative words as it goes, until it recognizes that there is only one word that could have this beginning or that there is no such word. Then it just stops paying attention to the rest, so "Mississ" is as good as the whole word. If no word exists, the last one that would fit is available via a pointer, and the "new" one can optionally be added to the dictionary. That verbal description of how the program I wrote actually works, by the way, has nothing to do with the algorithm, which is simple and mechanical and does not involve scanning all possible words. It's all in the way the dictionary is stored.

So there's a difference between saying that the input word is recognized despite a wrong letter and that its spelling is redundant, and saying that it is recognized BECAUSE its spelling is redundant or BY MEANS OF recognizing the redundancy. The fact of redundancy is just an interesting side-issue unless you can prove that this fact is actually used by the recognizing system or would be a parsimonious way to reach recognition.

Rick Marken (920118) --

Thanks for the admiring words about Tom's and my paper. It's been back and forth between me and Tom so many times, and so thoroughly revised so many times, that I think we both just want to see it in the mail. I no longer know if it's a good paper. I couldn't stand to read it one more time.

Tom found all those marvelous references, and did all the labor of drawing figures, doing the experiment, and creating the final version. Toward the end I was greeting every new version with "Yeah, yeah, Tom, just send the damned thing."

Rick Marken (920118b) --

>So if only one system at level n is in imagination mode, then its
>perceptual signal (n) is just what it wants it to be -- the reference
>signal ...

But in general there's more than one reference signal being varied by the output of each higher-level system. If the system at level n+1 operates by sending reference signals to several systems of level n, and constructs its perceptual signal at level n+1 from copies of the corresponding perceptual signals at level n, then to go into the imagination mode it has to short-circuit EACH copy of its output to the corresponding perceptual input. In general, some of the level n perceptions will be given negative weights by its input function, the rest positive weights. These weights must be compensated in the imagination connection for each weighted input, or else the feedback will be positive for some of the inputs.

So Martin is really, really right: the fan-out and assignment of signs has to be done in the higher system. Doing it Martin's way means that this needs to be done only once.

Martin Taylor (20118b) --

>I have no idea how sensitive a coordinated action network would be to

>the relative reference weightings, and I would love to be able to try it
>out someday.

I've tried it out, and so has Rick. The BYTE article contains a working model of what is diagrammed, and it works, too. There are limits on how much interaction is allowable (created by placing the three muscles at various angles) and some cases make control impossible (for example, when no muscle is oriented so as to have an effect in the negative X direction, and so on). As you approach the boundaries of the feasible ranges of interaction, you get pairs of output signals becoming very large, creating almost opposed effects; that is certainly wasteful of energy, and in real systems would risk running into magnitude limits. But away from those boundaries, the system as a whole is very insensitive to variations in the output weightings. For most muscle placements that give reasonably large components in all the required directions, the weightings of 1 and -1 are perfectly sufficient to get control as precise as you like. Just crank up the loop gains.

Or were you referring to open-loop action networks?

I agree that PCT seems compatible with the Layered Protocol Model -- generalizing beyond computer applications, we've really recognized the same phenomena and arrived at the same conclusions. I was delighted when you got into the relationship between loop gain and stability -- no wonder you've had so little trouble with PCT.

Gary Cziko (920119) --

Re: no correlation between neural output and behavior:

>So my question is this. Have there been studies done to show that this
>is the case?

Only indirectly. We have plenty of tracking experiments to show that cursor position doesn't correlate with motions of the handle when the cursor is disturbed. The longer you let the experiment run, the lower the correlation gets. Yet the cursor, not the handle, is under control.

If you assume that neural output IS correlated with handle position, you still get no correlation of neural output with the final effect that's under control, cursor position. Considering disturbances like shifts in body position and grip, and muscle fatigue, it's clear that neural output signals will actually correlate less than perfectly with handle position, so there's no way to improve matters.

The difficulty with trying to prove the allegation directly is that measures of motor signals entering muscles would be far less accurate than measures of handle position. You'd have to detect all the signals entering the whole volume of the muscle, and I don't know if that's feasible. So it could always be claimed that the failure of correlation was just measurement uncertainty.

Probably the most direct verification of the principle would come through applying random force disturbances to the arm holding the handle while the handle operates an undisturbed (or disturbed) cursor. It's a physical necessity that the muscle tensions must oppose the applied force if control remains good. The inference of muscle tension would be through a very short path. It's then hard to argue that the neural signals would correlate with the final behavior pattern any better than the muscle tensions would: the best that could happen would be that muscle tensions correlate perfectly with the driving signals.

I think your intuitions about an increase of drive being needed as muscles fatigue is correct. Isn't there someone at U of I who has an electromyograph? Your experiment sounds like something elementary to set up. David Goldstein, don't you have some equipment that could do this?

Gary again --

I heard the answer to your puzzle (I forget from whom) so I won't reveal the answers. The basic CT diagram reveals what theoretically has to happen (and does happen). The question about subjective interpretation isn't so straightfoward, so I can give a hint. The interpretation of the cause of the deviation is the same one that would be placed on the experience if the hand had been your own and you were in fact controlling the drawing. And imagination makes this interpretation feel correct.

Best to all,

Bill P.

Date: Sun Jan 19, 1992 5:49 pm PST
Subject: language, plurals

Re: Powers (920115.2200)

>I have the strong feeling that all these words are naming things that
>aren't words, but are fundamentally nonverbal perceptions, ...

Such is my belief as well. I've actually spent a fair amount of time pondering on how to hook language up to perceptual reality. But as you say, it ain't easy. I actually have an ms. on the subject that some psychologically inclined people seem to like, but not logicians.

The basic idea is this. Some sentences, corresponding roughly to the `atomic' sentences of logic, or the `kernel' sentences of early Chomsky & Harris (and current Chomsky, according to stories that have recently been emanating from MIT), are connected fairly directly to perceptual scenes. E.g. `this is a dog' (this pointing to an object in the scene) `this is barking', `this is chasing this') etc. Jackendoff's semantics has *a lot* to say about how this connection might be effected. Other sentences, such as `every dog in the neighborhood is barking' are connected to perception via a somewhat more subtle route, via an intermediate concept called `commitment', a stance which one assumes towards sentences (or, more precisely, sentences with understood references for their demonstrative elements). The idea is this. If you are committed to `every dog in the neighborhood is barking', and `this is a dog in the neighborhood', you are thereby committed to `this is barking', even if you haven't noticed this yet. This means that if someone points out the commitment, you must either accept it, or reject one of the premises from which it follows (`this is a cat, not a dog', ...). Commitments to complicated sentences are supposed to bottom out in patterns of commitment to atomic ones, which are then linked to perception via the principle that you can't be committed to an atomic sentence about a given scene unless the scene supports it.

Well that's an off-the-cuff run-though of the general idea, though I'm still working on it. There are a lot of hairy issues in the philosophy of semantics that one has to be able to deal with in this area.

As for Bruce Nevin's plurals, my speculation runs along rather different lines. I think of plurals as being similar to (what I presume to be) the implementation of `patterned paint' in drawing programs. Suppose you see a long row of windows on a building in the distance. I propose that what gets internally represented is not something like:

window - window - window - window - window - window - window - window

But rather a specification that a region is occupied by multiple instances of `window' (with additional info about what the repeated unit looks like). So pluralization is in effect an instruction to use this `space filling' operator. But I have no idea of how to test who is farthest from wrong about this.

Avery Andrews

Date: Sun Jan 19, 1992 7:55 pm PST
Subject: how to get a beer

My suspicion is that understanding language depends on understanding other kinds of organized activity, so here is an attempt to describe a system which, when it forms the intention to grasp something, goes to where it is and picks it up (inspired by going to the fridge to get a beer), along with a vaguely PROLOG-ish notation for codifying the description. A guiding idea behind this is that a lot of what might be taken for planning of sequences is actually an automatic consequence of dependency of goals on antecedent conditions. E.g., it makes no sense to reach for something unless it is in a reachable position. Another goal is to try to come up with easy notations for lashing elementary control systems into more complex ones.

`Proposition-like' goals in general are supposed to be control systems taking reference signals `ON' & `OFF', `OFF' being motivated by the principle that when the reference is `OFF', then no error signal is produced. They also produce perceptual output signals `YES' and `NO' (truth & falsity of their associated proposition), linked to ON and OFF by the principle that when the perceptual output is YES and the reference is ON, no error signal is produced. NO on the other hand appears regardless of what the reference signal is. For convenience, I will assume binary-valued (step) functions, but continuously varying ones should actually be used.

Furthermore, when one propositional goal gets reference signals from several sources, the logic is summation: ON+OFF+OFF = ON.

```
getting(X) =              ;; a `complex goal', X an identifier for what
                           ;; is to be fetched.
```

```
  at(X,P->),                ;; P-> vector-valued in body-centered coords
                            ;;  Returns perceptual `YES' when P-> represents
                            ;;  position of X, otherwise NO (representing
                            ;;  ignorance of where X is, perhaps triggering
                            ;;  a search for X).

  hand_at(P->),             ;; Discussed below.

  magnet.                   ;; device picks up scrap-iron.
```

This complex goal is formed by combining three subgoals with the `,' conjunction, whose significance is as follows: if the perceptual output of all the comma-ed goals is YES, then so is that of the composite goal. Otherwise it is NO. If the reference to `F,G' is ON, then that of F is set to ON. If F's perceptual output is YES, then the reference ON is sent to G, otherwise OFF is. The operation is associative, so we can define it binarily, but leave out the parentheses. The logic of this is like that of comma in PROLOG, except for the time-variant aspect: if F's perceptual output changes from YES to NO, then G's reference level changes from ON to OFF (so that when the object the system is reaching for disappears, the reaching stops).

```
hand_at(P->) =

  graspable(P->),   YES iff P-> a reachable position.  More below.

  reach_to(P->).   issues reference levels to arm control.
```

The `graspable' goal is described by combining an input processing function & and a recipient for error output, with the `<-' conjuction `(F<-G)' might be read as `F is supposed to be maintained by G'. F is a function on perceptual input that yields YES/NO output, but no error signals. F<-G's perceptual output is then defined to be that of F. As for G, if F's output is NO, then ON is sent to G, otherwise OFF. So:

```
graspable(P->) =

  in_reach(P->) <- going_to(P->).
```

Note that <- does not combine control systems, but combines a perceptual function and error-signal destination to produce a control-system. Note also that the perceptual function here gets its input as a parameter (which presumably comes from a lower level).

```
going_to(H,V,D) =      ;; hor. angle, ver. angle, distance
  heading_for(H),      ;; controls heading for H = 0
  going.
```

A verbal description of `heading_for(H)', for H specified in degrees:

   If |H| < 45, perceptual output = YES, otherwise NO.

   If H<0, then `rotate left'.
   if H>0, then `rotate right'.

As described, this system will move to the edge of the region of graspability, and then stop. But what we want is for it to move to the region of optimal graspability, unless there are problems with this, in which case the best attainable will do.

Avery Andrews

Date: Mon Jan 20, 1992 2:43 am PST
Subject: social influence

Ken Hacker [920118]

Anyone still pondering CT and society (9201k)--

I believe that the issues of "social control" and "control" are confounded semantically, and what we are really talking about is, as Bill has said, the issue of control (personal like "cognition") and how social interactions are influenced by control and/or how control gains information from social interactions.

Still, in my view, there are gradations of social influence and the processes whereby social factors provide data or limit choices are nontrivial. Essentially, I see 5 levels of social influence, none of which negates the principles of control:

1. The first level is made of simple impressions of what others are doing and what might be more "normal" or "acceptable."

2. The second level is persuasion -- messages intended to change attitudes, knowledge, behaviors, and actions from one direction to another. Higher order adjustments to deviations may be altered by persuasion.

3. The third level is shaping -- recurring and redundant messages and interactions which make certain ways of perceiving and behaving more reinforcing in relation to the environment than others.

4. The fourth level is constraint. Social interactions can establish punishments, pain, withdrawal of privileges, etc. in order to discourage particular behaviors. Humans can reject the contraints or accept them as very real elements of their social adaptation.

5. The fifth level is coercion. In this level, control over one human being is ATTEMPTED by others and MAY succeed (albeit by some form of conscious relinquishment by the person being coerced).

I have listed these five levels to attempt to clarify past postings. I am all in favor of more succinct claims in further debate. My goal is not to win an argument, but to find a connection. Control theory has definite relevance, in my "newcomer" opinion, to what we call intrapersonal communication. I have yet to see where it can contribute to the science of human communication beyond the intrapersonal (self-self interaction) plane, but I am open to suggestions.

Date: Mon Jan 20, 1992 2:44 am PST
Subject: Tucker's list

Ken Hacker [920118]

Charles Tucker (920116) --

Your list of "sociocybernetics" statements is interesting and somewhat informative.

You are a sociologist from what I gather. I am curious about your assessment of control theory for doing any type of sociological analysis. As a social scientist myself, I am still scanning what I am reading on CSG for anything I can relate to communication theory. So far, most of what I have read is interesting, but appears more physiological and cognitive/neural than relevant for theories and research concerning social interaction or organization. What do you think?

I agree with your statements about society and social events/structures not determining human behavior, but I think that social influence as something causal is a straw man in CSG discussions. The issue is not deterministic connections between social factors and human perceptions, but rather influence connections. For example, you say that technology cannot MAKE anyone do anything. Of course, but technology can CHANNEL the MEANS of doing things and provide TECHNICAL LIMITS for our behaviors. If you don't believe this, try shaking hands through the telephone! Again, social force or total control is not the problematic. The issues are influence, limiting factors, contraints, contributions, etc. Social factors CHANGE the LIKELIHOOD of certain choices among certain alternatives. We are not thermostats, not are we creatures walking around in perceptual vacuums. There is a bit of a font fallace in some of your items. You argue that rules and norms are just there for people to use. While this is true and theories in my discipline, such as the Uses and Gratifications Theory of media use, verify the basic idea, we also know that norms and rules and other social factors have influence on how a human perceives choices, consequences, and possible ways of achieving goals.
Yes, people are not forced to do anything, but on the other hand, contrary to Idealism and Idealist theorizations, people are not free AND able to perform any behavior they wish to. They are free AND able to TRY, yes, but notice that limits are real.

Thanks for an interesting set of statements. I would like to read your paper. I am not sure how your are defining "sociocybernetics," and would like to read more about it.

Dr. Kenneth L. Hacker
Dept. of Communication Studies
New Mexico State University
Las Cruces, NM 88003

Date: Mon Jan 20, 1992 7:00 am PST
Subject: micromouse competition

This looks like it might be a place for visibility.

Subject: APEC Micromouse Competition: Weston Hotel, Boston, Feb 25-26, 1992
Keywords: Come one, Come all. Gentlemen, start your nicads!
Date: 17 Jan 92 22:59:25 GMT
Organization: University of Waterloo

There is an an invitational Micromouse competition happening Feb 25-26 at the Weston Hotel, Downtown Boston, as part of the annual APEC conference. Anybody with a qualified robot critter is invited to attend. Competitors are asked to show up on the 25th to calibrate their mice to the maze, and the actual competition will be at 2:00pm the following day.

For those that don't know, in this contest the contestant or team of contestants must design and build a small self-contained robot to negotiate a unconnected maze in the shortest possible time. Since 1979, the micromouse competition has been one of the ultimate challenges for robot enthusiasts. Sort of the Nasscar Circuit of the robot world.

So far, there are at least 5 high-quality mice guaranteed to be there. Anybody showing up will be witness to real state-of-the art.

Practical application quotient: 0.0, but it is very impressive. Lotsa fun.

For more information, you can contact David Otten (6-time Micromouse world champion) at 617-253-4691. I believe he can give you help with hotel bookings.

For all Canadian competitors, please confirm if you will be going by calling Louis Geoffrey (Canadian Micromouse Champion) before Feb 19th. His number is 514-449-5529. If there is sufficient interest from the Toronto area, a car-pool thing may be arranged.

Micromouse Rules are available on request.

See you there.

Is all.

```
--
Mark Tilden: _-_-_-__--__--_      /(glitch!)  M.F.C.F Hardware Design Lab.
-_-___       |              \  /\/            U of Waterloo. Ont. Can, N2L-3G1
     |__-_-_-|               \/               (519) - 885 - 1211 ext.2454,
"MY OPINIONS, YOU HEAR!? MINE! MINE! MINE! MINE! MINE! AH HAHAHAHAHAHAHAHAHA!!"
```

Date: Mon Jan 20, 1992 8:38 am PST
Subject: sequence and program control wp1-18.pln

[From: Bruce Nevin (920117 1234)]

Bill, you mentioned a while back that economies could be gained when sequence detectors begin with the same sub-sequence. I take this to mean something like:

```
ABCDEF        ABCGHI            Where a, b, c, d, e, f, g, h, and i
   |             |              are perceptions below sequence level,
   o             o              and ABCDEF and ABCGHI are sequence
  /|\           /|\             recognizers.
 | |||         | |||
 | |||         | |||            This looks like there is an ABC
 | |||         | |||            sequence recognizer that provides
 | def         | ghi            perceptual inputs both the ABCDEF
 |             |                and ABCGHI sequence recognizers.
 +------- o ---+                (I assume that d-e-f and g-h-i are
        / | \                   not recognized alone as sequences.)
        a b c
```

What is the difference between (1) the ABC portion of all the recognizers for longer sequences all just happening to have the same input connections below, (2) sequence recognizers that include a-b-c in their input all sharing the same neural mechanism as the ABC portion of their respective input functions, and (3) ABC being a separate sequence recognizer?

Might there be an evolutionary process and a developmental process in something like that order?

This has an interesting connection to Harris's determination of morpheme boundaries from peaks in the next-successor counts of phoneme sequences. A morpheme would be a sequence of phonemes that had many connections to other sequences of phonemes. Thus, if the ABC recognizer provides input to many other sequences, ABC is probably a morpheme.

Should we not not expect a similar differentiation in non-language perceptual control, that is, the differentiation of oft-repeated sub-sequences as perceptions on the next level up?

The order of acquisition for morpheme -ment might begin with (1), i.e. words like judgement, abandonment, assignment, comment, and cement all just happening to have the same input signals from detectors for m, e, n, and t.

On this basis, the presumption would be that it is the same -ment in comment and cement as in judgement, abandonment, assignment, etc. One would set up sequence recognizers for com- and for /si/ (the "ce-" of cement) and track them. Well, probably not for /si/. Stress patterns would distinguish unstressed -ment from the stressed second syllable of cement, just as it (among other distributional facts) distinguishes -ment from meant.

What distributional facts other than distribution relative to stress?

For a start, most words satisfy a condition for special recognition of -ment through development to (2) or (3): the other part(s) of the sequence also have multiple connections and/or stand alone as sequences. For this, "firmament" doesn't qualify, despite its etymology, but comment and cement do (despite their etymologies--of which the language learner knows nothing, of course). When the complementary part isn't recognized, the supposition appears to be that it must be a sequence too, and on the strength of that a recognizer set up that begins tracking it.

Most -ment words satisfy another condition on a higher level. The simplest way of putting it in this context is by reference to concurrent nonverbal perceptions. About those perceptions one can (and sometimes does) say some other utterance in which that complementary part stands without the -ment, but some other sequences (morphemes) are the same. In general, an equivalence something like the following may be determined without too much trouble:

```
        X-ment <==> a product of one's X-ing something
                    a result of one's X-ing something
                    the situation of one's X-ing something
```

So we appear at this point to be dealing with an equivalence between sequences of sequences. The first sequences are sequences of phonemes (or other phonological units), recognized as morphemes; the second are sequences of morphemes. The equivalence is a reduction.

Both the word with -ment and the unreduced sentences correlate with the same perceptions. Either you have a reduction system controlling the form of morphemes, or your mechanisms for associating words with nonverbal perceptions have to make identical associations in multiple places, redundantly. I choose the former.

The com- and /si/ recognizers fail this equivalence. The error on a detector for this equivalence should lead later to their being rejected as candidates for this reduction. If someone mentions "Bill's comment about the cement" we don't ask "What was he comming, and who cemmed it anyway?" Though we might ask what was he judging and who abandoned it if someone mentions a judgement and an abandonment.

The equivalence, in the PCT scheme of things, must be on the program level. Something like "if I can say ABC I can also say PQBRSCT". The word sequences stated as equivalent in these programs would accept input signals from sequence-detectors of both kinds. Thus, in the example given for -ment, X is any of many signals from various morpheme detectors on the sequence level, -ment is the signal from one morpheme detector on the sequence level. In addition, there are two sequence detectors for the word sequences before and after the X, namely:

        a product of one's
        -ing something

I have put this in a way that is misleading on another count. It is not the raw word sequences such as these that are detected, but shorter sequences (pairs, triples, rarely quadruples) representing word dependencies in operator grammar. Product is reduced from produce (by way of a relative clause). Of is inserted as part of that reduction. The 's and -ing are argument indicators. I am just ignoring the complexity of equivalences or reductions on the program level for clarity of discussion. As long as we understand that it is there and don't get too carried away about phrase detectors (though they must exist for idioms, fixed expressions, cliches, and the like) we can safely avoid getting getting our hands too dirty in that complexity just now.

One point to be made is that the program steps themselves are simple. Not so much deduction as the stating and playing out of axioms. It is the interconnections among them that is complex. There may be one program for each class of operators.

Another concerns the question I asked about evolution/development of morpheme detectors. A similar process, if it is plausible for morpheme detectors, might serve for morpheme sequence detectors--that is, detectors for operator-argument dependencies.

In both cases, we might in a wild flight of imagination suppose there was a program-level process looking at numbers of connections and deciding to grow a new input function (or part thereof) or a new sequence detector. But I assume this is wrong, a mistaken identification, where the invisible programmer's hand is something like neural Darwinism.

More when I get time this weekend.

Bruce
bn@bbn.com

Date: Mon Jan 20, 1992 8:49 am PST
Subject: Re: redundancy

[Martin Taylor 920120 11:15]
(Bill Powers920119.1300)
>
>>The alternate explanation is that one element of the perceptual function
>>is the correction of such errors as can be corrected using the
>>redundancies in the pattern to which they are exposed. A misspelled
>>word is seen as its correctly spelled equivalent because that is what is
>>output by the perceptual function, not necessarily because it is
>>imagined to be correct.
>
>I think you're right, but you've inserted an unnecessary step. You're
>proposing, I believe, that the mistaken pattern is presented and
>perceived, then the redundancies allow detection of the error and
>replacement of the erroneous letter with the correct one, and then --
>what? This method implies that the result is now perceived AGAIN as a
>correctly spelled word -- but there's still no perceptual signal
>indicating that the word is present.
>
Redundancy MAY be perceived, but it is more likely to be just used. When it is used, the result is as I said--the incoming pattern is reported by the perceptual function as the corrected version. That is what I meant by the passage you quoted, and what you claimed to be happening. But it is also possible that the "extra step" you introduced could happen, and

both the actual and the corrected versions are perceived, as in your Mississi example.

Your proposal that we recognize words by eliminating possibilities starting at the beginning has an influential set of advocates, led by Marslen-Wilson of MRC-APU, Cambridge, UK. It is called the "cohort" method of recognition. I don't entirely buy it, but that's an argument not (yet?) appropriate here.

On redundancy of action, and reference weights: What you and Rick have tried is a situation in which a very few control systems are not fully orthogonal and interact. The situation I contemplate is one in which many control systems are doing nearly the same thing, in groups whose control capabilities overlap at the fringes (like muscle fibres in a muscle, where other muscles can to some extent compensate for the paralysis or loss of one). That's the situation in which I would like to know whether there is more importance for the reference weights relative to each other.

Martin

Date: Mon Jan 20, 1992 9:13 am PST
Subject: Re: Deceptive Hand Study

[From Chris Malcolm]

> A subject is given a pencil and asked to draw a straight vertical line
> (let's say from north to south).   The subject must look into a box to see
> his hand as he draws. But, unknowst to the subject, he does see his own
> hand but another person's hand (both are gloved to make the deception
> easier). As the subject attempts to draw the straight line, the stooge's
> hand draws a line that starts out north to south but then curves to the
> west.

> 1. What does the subject draw?
>
> 2. What does the subject experience?

I would be interested to know if there was any attempt to correlate the variation in responses with personality variables. I recall a tipping room experiment -- a room with stiff wires suspending the lights, fake water in glass, etc, so that as the room tips there are no clues to the tipping. The subject sits in the room in a kind of dentist's chair with three degrees of rotational freedom. The room is moved about a bit, and then left stationary, and the subject asked to arrange the chair in which they sit to be upright.

Some will set it parallel to the room walls, and some will line it up with gravity, most being in between. The only personality variable which correlated with this difference was being politically left-wing or right-wing, i.e., the left-wingers preferred gravity, whereas the right-wingers preferred the room walls as a reference.

Fascinating!

Date: Mon Jan 20, 1992 9:14 am PST
Subject: imagination, silly articles

[From Rick Marken (920120)]

Martin Taylor and Bill Powers -- you guys are right. The way I set up the imagination connection in my Excel spreadsheet is wrong (I think you have a copy of that version Martin, right?). Anyway, the correct way to do it (based on Bill's concept of imagination) is to play output of system n right back into its perceptual function, with appropriate duplication. Very easy and elegant -- I will start modifying the spreadsheet model as soon as possible). For example, say that a system's perceptual input is a linear function of three inputs so p = a+b-c. Ordinarily, a,b and c are perceptual signals from lower order systems. But, in imagination mode, these perceptual signals are exactly what would be specified by the output, o, of the system -- so, p = o+o-o. Very simple (when p is a linear function of inputs -- but it should work in others cases; we'll see).

This is a nice clarification of the imagination component of the hierarchical model, Martin. Thanks. It will be fun to see how the model behaves when a system switches in and out of imagination mode. It would also be nice to think of some experiments to test this aspect of the model -- sounds a lot like cognitive psych to me.

Gary -- I'm the one who mentioned the results of your experiment (where the person sees an arm drawing that is not really his) to Bill. So I know the results also, so I'm out of the competition. I discovered that experiment LONG ago -- before I even knew what PCT was. Although I didn't really understand the results, I liked it as a demo; it must be a fascinating thing to experience -- though not unlike the "open loop control" experiment that I did once -- where the subject sees only the target-cursor display from a previous closed loop run but thinks that s/he is controlling the display. The control handle feels "sticky" or "slippery" to the subject -- although it actually has no effect on the cursor.

Greg -- I got copies of the articles by Locke and Kline in Motivation and Emotion. They are just silly -- not even worth a reply really. Locke hates control theory -- Kline loves it; neither understands it -- AT ALL. I would tell you more but I found myself falling asleep after almost every sentence as I read these things. Not once do they mention that control theory is about control -- they never define control; they don't know that the most important thing about the theory is that it shows that control involves the control of perceptual input. Just amazing stuff.
What the articles did show is that there is a growing group of advocates of control theory (Kline, Carver/Schier, Hollenback, Lord and Campion) who don't understand what they are doing. They know nothing of modelling. They are just using some of the terminology of control theory as parts of their verbal explanations of the usual multiple subject, significance test, garbage data.

Ah well.

Hasta Luego

Rick

Date: Mon Jan 20, 1992 9:41 am PST
Subject: Re: Semantics

[From: Bruce Nevin (Monday 920120 11:22)]

(Bill Powers (920117)) --

>Bruce, suppose we expand all sentences into their completely unreduced
>forms. Let's just say that we've agreed to communicate only in the
>unreduced forms, which I presume means we're being as absolutely
>completly explicit as possible.

That is the result.

>NOW how do the sentences relate to experience? For example, what does the
>"and" in "someone and someone" mean? I don't want an answer like "'and'
>indicates the conjunction of two terms." If that's closer to what "and'
>means, then substitute it and begin again: "conjunction someone someone,
>conjunction someone someone" or whatever. Now what does "conjunction"
>refer to? What does "someone" refer to? What does it mean to put those
>words together that way?

>I have the strong feeling that all these words are naming things that
>aren't words, but are fundamentally nonverbal perceptions,

I've been agreeing very strongly with this. But it is only in a fully explicit base form, without reductions, that we can determine what it is in language that is correlated, one for one, with perceptions. Without undoing the reductions, we have one kind of correlation for the and in

    1. John and Bill

and another kind of correlation for the and in

    2. John came and Bill left.

By undoing the reductions in

    3. John and Bill left.

in two ways, we uncover an ambiguity. This can be either of the following:

    4a. John left and Bill left.
    4b. A group, which includes John and Bill, left.

(Or maybe "made up of John and Bill.") The relative clause goes back to:

    A group includes John and a group (same as mentioned) includes Bill.

The perception associated with and (4a) and (4b) is the same here as it was in 3. Harris derives and as argument indicator of a zeroed performative of the "say, state" family, something like "I co-state . . . and" or "I say both . . . and". The sense of grouping together that we associate with and is due to a word like group or set, as in (4b). There is no such perception associated with 4a, only the perception that the speaker is asserting two things together.

>I have the strong feeling that all these words are naming things that
>aren't words, but are fundamentally nonverbal perceptions, EVEN WHEN
>YOU'RE TALKING ABOUT LANGUAGE ITSELF.
>When you say "someone and someone and someone" you're trying to
>point to the nonverbal perception that is also pointed to by the words
>"people" or "everyone." It's as near as you get get with words alone to
>evoking the perceptual situation to which "people" or "everyone" point
>DIRECTLY. When you say "John is gone today; he has a dental appointment,"
>the "he" doesn't point to the WORD John; it points directly, if
>temporarily, to the nonverbal perception that was named or at least
>evoked by the immediately previous use of the word "John."

The reduced form is associated with the same perceptions as the unreduced form. Since a reduced form is typically ambiguous, it is also associated with the same perceptions as one or more other unreduced forms. This is one reason for my claim that the reduced form is associated with perceptions by way of a fully explicit unreduced source. An alternative is to say that reduced forms associate ambiguously with a set of different perceptions, and just not notice the concidence that each of the perceptions in the set is associated with one of the more explicit unreduced forms, which are not ambiguous. That is what you want to do, it appears. In either case, yes, even a grunted "UH-uh!" or a mumbled "Mmm mmm mmmmm?" with the mouth full maps to perceptions. I just claim that they do so by way of explicit, unreduced words. Undo the reductions, then you get a nice 1-1 correlation of words with perceptions, within the granularity of the base vocabulary. (Couldn't talk about NAND gates until they and the word were invented.)

>Maybe this is what drove Chomsky to
>look for deep structures, which aren't words or sentences but _nonverbal
>forms_.

No, Chomsky really isn't interested in perceptions.

I'll think some more about your post when I get time. Possibly not until the weekend again. Another relapse, very much by surprise about 3AM Saturday. We went to Lahey Clinic this time. Different antibiotic. Hopefully this time she'll not prematurely rejoice at complete recovery and take on too much, as she did last Friday.

Bruce
bn@bbn.com

Date: Mon Jan 20, 1992 9:47 am PST
Subject: Re: Semantics (Martin)

[From: Bruce Nevin (Monday 920120 1205)]

(Martin Taylor 920117 13:50) --

>Bruce doesn't know how to do it, either, because by the
>time all the zeroings have been reconstituted, almost every sentence will
>contain (as Bruce says "in the utterance") the whole life experience of
>the talker, as far as it is known to or presumed by the listener.

One of my purposes is to draw lightening to whatever is ill founded in my developing understanding of language and perceptual control. I think in this case the bolt strikes down a misunderstanding of what I am trying to say, rather than a part of what I am saying, and I am also controlling for that excellent result. See my response to Bill.

In 1969 or 1970, Strawson was visiting Penn, and in conversation asked me to synopsize Harris's theory--really an unfair request of a graduate student, I felt, but Harris was away, so what could he do. When I talked about the zeroing of whole sentences as common knowledge, but adduced for discourse structure, he objected "Harris can't put the whole world in his grammar!" My response then, as now, is that the grammar doesn't have to, because the language user has access to his or her knowledge about the world and is free to omit mention of what is felt to be so obvious that it would be tedious to say it. All that the grammar provides is socially normal means for doing so, given a perception that overt utterance would be redundant.

Thus, if I say "I'm taking my umbrella because it might rain," I don't add "and one uses an umbrella to keep off rain" defining the relation between umbrellas and rain. If I were talking to a Martian (precious little rain on Mars, I hear), I might add the definition sentence. Now the hallmark of coherent discourse is that words are repeated in successive sentences, in a patterned way. (I have said little about discourse structure, a second-order syntax.) The definition sentence creates a bridge of word sharing between the two assertions conjoined under "because"; and indeed such word sharing may be a requirement imposed by the program controlling the word dependencies of conjunction words like "cause" in the source of "because". If the words are actually present, uttered, the requirement is satisfied. If the perceptions associated with the words are present, the same program input requirements are satisfied.

If one of us had already said "it might rain" I would just say "I'm taking my umbrella," I would omit saying "because it might rain." The reason is that the words had already been said, so you already know that. It appears to be the perception "you already know that" that satisfies the program input requirements both with respect to "it might rain" and with respect to "one uses an umbrella to keep off rain."
In this I think you and I agree?

Bruce
bn@bbn.com

Date: Mon Jan 20, 1992 11:19 am PST
Subject: Re: Semantics (Martin)

[Martin Taylor 920120 13:50]

(Bruce Nevin 920120 1205)
>
>
>If one of us had already said "it might rain" I would just say "I'm
>taking my umbrella," I would omit saying "because it might rain." The
>reason is that the words had already been said, so you already know
>that. It appears to be the perception "you already know that" that
>satisfies the program input requirements both with respect to "it might
>rain" and with respect to "one uses an umbrella to keep off rain."
>In this I think you and I agree?
>
I think we do. (Expand that to I think you and I agree (on prestated referent) (Expand that to ???). Yes.

I don't know how we get into these micro-misunderstandings that threaten mountainous perceptions of molehills. I agree that the object of the language structures that signal zeroing is to point to elements that the speaker believes the receiver to be able to fill in, whether from prior discourse context or from the current or remembered perception of the world.

I hope that's what we are agreeing about. It seems to be, to judge from your posting.

Martin

Date: Mon Jan 20, 1992 12:20 pm PST
Subject: Various

From Tom Bourbon [920120 -- 13:12]

Rick Marken [920117], thanks, for the kind words on the manuscript by Bill and me. We have lived with various incarnations of the thing for so many years that, like Bill, I can no longer judge its quality, but I think it is better than when we started.

The day I shipped it off to the editor, my wife bought a bottle of champagne so we could celebrate its departure. (PCT people do it that way, rather than celebrating news of acceptances --
you never have an excuse for champagne, if you wait for successes!)

As for the earlier rejection, that was by _Science_, where the editor rejected it without review. The form letter said something like "not of sufficient interest," and "nothing novel."

WAYNE HERSHBERGER [920117]. I was unaware you no longer receive _Science_ (that bastion of PCT), so I thought my post about the article on presaccadic shifts in receptive fields would be old news to you. I am happy that I went ahead with the post. I do recall your presentation on the phantom array, at the last meeting of CSG, and I read your 1987 article on the phenomenon. What I did not recall was that the psychophysical effect occurs 80 msec prior to onset of the saccade -- precisely the lead time found in cells in the parietal cortex of monkeys. In a way, I am not surprised: The psychophysical data you presented from Scott's dissertation were sufficiently crisp that they suggested you fellows were on to a principle, not an artifact or an epiphenomenon.

I do not recall the angle subtended by the phantom array, but I do remember you saying, and writing, that it is large. Can you give an estimate of how large? Large enough that it might produce a series of distinct evoked responses in human visual cortex? This June, I will be at the magnetoencephalography lab, in Galveston, Texas, where we might look for physiological evidence of the shift in humans. It looks as though this might be the first, or one of the first, clearly recognized instances in which behavioral, psychophysical and physiological data converge to reveal the setting of a reference signal, and the behavior that brings perception in line with that reference. NICE WORK!

GARY CZIKO [920118], there is a literature in which people examine relationships among force, sensed effort and magnitude of the electromyogram. (Bill Powers [920119] was right, recording and interpreting the electrical events that accompany the actions of muscles can be quite difficult. EMG is a crude measure, at best.) In general, when a person holds force constant, both the amplitude of EMG and reports of felt effort increase. When a person holds a feedback signal that is an analogue of EMG constant, force decreases, and there is USUALLY no reported change in felt effort (the published rresults concerning effort are somewhat contradictory.) Finally, when a person holds constant reported sensed effort, force decreases and the reports concerning EMG are ambiguous.

Tomorrow (920121), one of my thesis students, Laura Hamilton, will begin collecting data in an attempt to resolve some of the ambiguities I mentioned above. She began planning her project about three months ago, out of curiosity over the same points you raised, Gary.

Reading the literature on these topics is enlightening -- there is much talk about "neural commands" to the muscles, and hardly any mention of control, as we have come to know and live it. (What an interesting typo, that! To know it is to live it is to love it.) The notion that neural events, like the handle movements they produce during tracking tasks, might be uncorrelated with the consequences of the meural events and the movements is, as you said, one of the fundamental principles of control, and one of the most baffling and confusing points for people unfamiliar with PCT.

I will keep you posted on Laura's results.

Best wishes,

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Date: Mon Jan 20, 1992 1:37 pm PST
Subject: Re: saccadic suppression

[Martin Taylor 920120 15:45]

In reference to Wayne's equation of 80 msec presaccadic psychophysical and physiological effects, I thought it relevant to quote much of the abstract from the thesis of a colleague of mine (I have mentioned this privately to Wayne on a couple of occasion, suggesting he get hold of the author for more information). The number cited here is not 80 msec, but a larger number.

Attentional Focus and Saccadic Suppression. R.G.Angus, York University (Toronto), October 1974.

Abstract

An attenuation of vision has been found to be associated with voluntary saccadic eye movememnts. This paper reviews [...]. None of these explanations is adequate to account for the saccadic suppression of short flashes presented against a dark background, especially since the effect begins 150-200 msec or more before saccadic onset. The alternative hypothesis states that the attenuation may, in part, be influenced by a saccade-contingent direction compensation process, which results in perceptual displacement of suprathreshold flashes beginning about 200 msec before saccadic movements. Thus, threshold flashes presented during direction compensation may not be detected because S's attentional focus may not include regions where displaced flashes appear. In five detection experiments 3 experienced Ss task was to report onsets, increments, or decrements of one of two test lights presented before voluntary saccades. In a direction experiment Ss task was to report the occurrence of perceived displacements. The results of one detection experiment verified the occurrence of saccadic suppression in darkeness; the onset of suppression approximated estimates of the beginning of direction compensation. The results of other detection experiments indicated that performance improved when visual reference cues were provided to direct Ss attentional focus. Some attenuation remained, however, suggesting the influence of other factors. Detection also improved, although to a lesser extent, when the temporal variability of the test flash presentation time was reduced. Since compensation progresses over time, flashes presented at the same time should appear in similar locations and attentional focus should be directed to regions where signal occurence is most probable. The results of the direction experiment indicated that many more displacements were perceived in darkness than when the visual reference cue was provided. In general, the results were interpreted as support for the notion that direction compensation an attentional focus contribute to the attenuation of detection of flashes presented prior to voluntary saccades in reduced laboratory conditions.

Martin

Date: Mon Jan 20, 1992 3:14 pm PST
Subject: Language; Social control

LM0/.RM79/[From Bill Powers (920120.0900)]

Avery Andrews (920119) --

It always sets me up when a real linguist agrees that I have guessed somewhat right. You are a dinkum cobber. I am a hep cat.

Your comment on "windows" brings up a question I've noted but never answered:

> Suppose you see a long row of windows on a building in the distance. I

>propose that what gets internally represented is not something like:

>window - window - window - window - window - window - window - window

>But rather a specification that a region is occupied by multiple
>instances of `window' (with additional info about what the repeated unit
>looks like). So pluralization is in effect an instruction to use this
>`space filling' operator.

This phenomenon, which I agree exists and tells us something important about perception, doesn't seem to work very well in my pandemonium model. How does the perception of windowness get attached to each region of the building? Surely we don't have as many independent windowness detectors as there happen to be windows on that particular building -- and a different number for a different building.

The nearest I've come to an answer is to suppose that there is just one windowness detector as usual, which is applied at the center of attention. The recent Science article on remapping of visual fields to correspond to the "intended direction of looking", which links to Scott Jordan's and Wayne Hershberger's work on the "phantom array," lends a little support to this idea. So the phenomenon is "no matter where you look (or attend) on this building you see a window." That comes out "windows" without actually counting them (you can say "windows" long before you can say how many there are). My subjective experience is that form recognition moves with the locus of attention in the visual field and is limited in angular range. Things outside the circle of attention really aren't seen as configurations. This idea might be experimentally testable -- Wayne, what do you think?

I agree with the general direction of your work on getting perceptual meanings into the linguistic picture. This task may not be as difficult as it seems, especially if you take advantage of the HCT model to let the nonverbal perceptual control systems do what they are good at and separate out what linguistic-type processes are good at. For example, in getting yet another beer (you lush), you don't need language to handle the details of getting within "reachable" distance of the bottle. That can be done just by selecting any workable relationship-perception, a desired distance between you and the bottle. Once the reference signal for the relationship-controlling system has been established in perceptual terms, the analog systems from there on down will take care of getting the body into that position in relationship to the bottle. To grasp the bottle you need only pick the specifications "reach" and "grasp", which actually may be part of a well-learned nonverbal sequence that doesn't require verbal direction (you automatically reach before grasping).

To be a little more general, you might ask yourself what we need language and language-type processes for. What are the things we do better using language than we do without using it? What things do we do that we couldn't do at all without naming, assembling sentences, reasoning with symbols, generalizing, and any etc.s you can add? Communication is an obvious one, but there are others. I really can't perceive or control complex situations without reverting to symbols and symbol manipulations in some way -- words, algebra, programs. Even when I'm all by myself. But these processes yield only symbols as output -- those symbols then have to get translated into specific nonverbal reference signals. They are names or descriptions of outcomes, but we need an image of the outcome to serve as a reference signal.

Your idea of "committment" could be made a lot clearer using HCT. I understand what you mean, but there are unspoken nonverbal phenomena behind this word (that's how I know what you mean). The words themselves have no logical functions: just saying "every dog is barking" doesn't imply anything. This sentence, to become a logical proposition, has to be converted to logical variables -- neural signals -- and passed through a logical device that can derive implications and conclusions -- the program level in the HCT model. Words and sentences are not logical devices; they're just words and sentences.

Just taking a stab in the dark, I would guess that "committment" involves something like this scenario:

1. Several verbal propositions are stated. They are just sentences.

2. They are converted into imagined perceptual situations: a collection of dogs, every one of which is barking, and a single dog, which is not barking.

3. These imagined situations are reduced to symbols standing for the elements of the situation: barking, all, X, not-barking, or whatever. The symbols may be just neural signals, not even words. The signals are compared logically according to the form of the logical perceptual function: if the signal indicating "all dogs barking" is TRUE (nonzero), and the signal indicating "one dog not barking" is also TRUE, then the output of the logical perceptual function is FALSE. The conjuction of the two situations is perceived as FALSE (no signal) by a NAND device.

4. Only the logic level cares whether the compound statement is true or false. At lower levels, the statement is made and corresponds to a perceptual situation, but implies nothing. At lower levels, it is possible to hold in mind two statements: "all the dogs are barking," and "one dog is not barking." This does not cause any problems, because the "and" merely separates items on a list and is not the logical AND. The conjunction of these statements generates the evaluation "false" only at the level that's concerned with such things: what I call the program level.

So perhaps "committment" means just moving your attention to the logic level and bringing its kind of perceptions into consideration. To "take the stance" is to decide to consider what the logical level has to say about the conjunction of perceptions.

Would you post a reference to Jackendoff's book for me? I need to read it again, but will have to get it through interlibrary loan. Also, a snail-mail copy of your ms. would be nice.

Gary Cziko (920119) --

You might as well send personal mail to me at the FLC address. I got behind on the Boulder stuff and just deleted it all (89 files). I am still waiting for red tape to clear me for a logon of my own at FLC. Hal Mansfield, co-chair of the psych department, seems to think the appointment is a foregone conclusion: he's asked for another copy of my vita, and for a list of books on CT for the library to purchase with some spare funds. Roger Peters doesn't mind -- I'm managing the mail for him anyway, and it won't be for much longer. I haven't got a convenient way to send personal mail yet -- hence using up CSGnet space for this. A drop in the bucket. Really really personal stuff -- use snail mail.

David Goldstein has biofeedback equipment, which I believe includes an electromyograph. Cut it out with the Freud stuff.

I haven't seen the announcement, but I trust you. Go ahead.

Ken Hacker (920119) --

I like your list of ascending degrees of influence. It would be nice, however (from my point of view if not yours) to weed out all behavioristic implications -- that is, implications that the "influences" can alter behavior all by themselves. When they work, they work for reasons having to do with human nature on all sides.

>2. The second level is persuasion -- messages intended to change
>attitudes, knowledge, behaviors, and actions from one direction to
>another. Higher order adjustments to deviations may be altered by
>persuasion.

Persuasion almost always includes bargaining of some sort. "Come on, you'll do it if you're a nice guy." In other words, "if you do it I'll treat you as a nice guy, and if you don't I won't." So if doing what is asked and getting treated as a nice guy suits the hearer's goals, the hearer will comply. Persuasion does not have any _necessary_ effect.

>3. The third level is shaping -- recurring and redundant messages and
>interactions which make certain ways of perceiving and behaving more
>reinforcing in relation to the environment than others.

Nothing is reinforcing unless (a) the reinforcee has already established a nonzero reference level for it, (b) the reinforcee can't get it in some other and easier way, and (c) the act required to get it doesn't have side-effects that violate any of the reinforcee's other goals.

"Shaping" is a misinterpretation of what is going on, although the method (hot-and-cold with progressively narrower requirements) is a valid and valuable teaching tool. I think shaping was one of Skinner's major contributions, although he didn't understand how or why it works.

Shaping only works if the shapee wants to learn, or is placed in a situation where not learning is punishing and is kept in that situation by brute force. The effectiveness of reinforcement depends entirely on the reinforcee's goals, and on whether the reinforcee judges that the act required to achieve the goal is worth the effort. So the procedures of shaping may or may not have any effect.

>4.   The fourth level is constraint. Social interactions can establish
>punishments, pain, withdrawal of privileges, etc. in order to discourage
>particular behaviors. Humans can reject the contraints or accept them
>as very real elements of their social adaptation.

To put this in the active voice, I'd say that a person may decide that engaging in particular behaviors has outcomes that violate his or her goals, and not engage in them (or do so only under circumstances where the unwanted consequences can be avoided). The social interactions (i.e., other people) may attempt to control a person's behavior by applying or threatening punishments, pain, withdrawal of privileges, etc. (in the end, only applying such constraints convinces). Whether this has an effect depends entirely on the person being controlled. If control fails, only overwhelming physical force can overcome the person's resistance and noncompliance. If the person sees reason, no force is necessary -- and there is no control.

>5. The fifth level is coercion. In this level, control over one human
>being is ATTEMPTED by others and MAY succeed (albeit by some form of
>conscious relinquishment by the person being coerced).

This level always works if the controller has sufficient physical resources and is willing to go as far as necessary to get from the other the behavior that is wanted. Shackles are effective. Cattle prods are, too. Confinement in a cell effectively limits disobedience. If you want the other to do voluntarily what you want done, torture is very efficient, provided you set no limits on what you're willing to do and don't destroy the lower levels of control in the process. By turning on the reorganizing system in the controllee, one can alter any aspect of that person's organization, from top to bottom. Unless the methods become accidentally lethal.

You have described here a progression that is followed when one person desires above all to control other people. It begins at level 1, presenting examples of "good" behavior and relying on the other to want to imitate them. If that doesn't work, persuasion is applied. Persuasion relies on guessing at goals the other has, and offering easy satisfaction of those goals for cooperation.

If persuasion doesn't work, the next thing is to get control of something that the other wants and can't get from anyone but you. You then make giving this something to the other contingent on performing the behavior you want to see.

If the controlee decides that doing what the controller wants is just too tedious or tiring or painful or ignominious, the controllee will look for other sources of the reward. To prevent this, the controller brings bigger guns to bear. Physical control must be established first. Once the controller has established the fact that actual physical control will be used if necessary, a threat may work. Then it is possible to start withholding what the other wants, applying pain, assigning punishments, and so on, because now the other is physically not allowed to prevent these means from being applied. The contingency of reinforcement is established without consent of the reinforcee, and the reinforcee's ability to change the contingency is removed by force.

And if none of that works, yet the desire to control is unabated despite what the controller finds himself or herself doing to another human being, the last vestige of civilization is abandoned and we get the extremes to which we can be assured some members of our species are willing to go.

Progression up this scale is all but assured when a sufficient number of people establishes the control of others as the primary principle of social interaction. The natural human response to attempts at control is resistance and opposition. This results in increased error signals inside the controllers, and thus increased energy and scope in the attempts to control others. It is probably also interpreted by the controllers as an attempt at reverse control, the main principle being projected onto the behavior of others as well. The more the resistance to control, the higher on the scale of coercion the controller goes. And the greater the pressure put on the controllee, the greater the resistance. Thus the basic orientation of controlling others, given enough people adopting it and having enough physical resources, leads inevitably toward level 5, the only restraint being whatever effect there may be on self-image. If the reference level for self-image is one of absolute power and control, there are no such restraints.

I should think that this prediction from control theory would be quite amenable to experiments of the type pioneered by Milgram.

Control theory suggests quite a different principle of social interaction: not the gaining of control over others, but the granting of control to all. Attempts to control living systems automatically create opposition. Understanding of exactly how this leads to escalation of conflict and violence is sufficient to show that control of others is self-defeating as a general principle. When others succeed in reaching and maintaining their goals, there is nothing with which to reward them. Neither do they have any reason to interfere with anyone else, except by accident. And vast stores of energy, locked up in mutually-cancelling conflict and competition under the other principle, are free for use in positive directions.

Getting There from Here, it seems to me, is something that would be worth paying some attention to.

Date: Mon Jan 20, 1992 11:50 pm PST
Subject: jackendoff, windows, sequences, realty

Bill Powers:

Jackendoff References:

  Semantics and Cognition (1983)

  Consciousness and the Computational Mind (1987)

  Semantic Structures (1990)

  all MIT Press.

Jackendoff has his limitations - he's a strict `neo-rationalist' cognitivist (neo-rationalism means assuming universal definitions of a fixed list of primitives, plus recursive rules for composites). He has also taken no heed whatever of discourse, so far. The 87 book is probably the most suitable for getting an overview.

Tell me a current p-mail address so I can send you copy of the paper on commitment, etc.

Windows:
I suspect that it might be possible to build a gadget that would detect `instances of the same thing', without registering much about that that kind of thing is. Then noticing (a) a window (b) that there are more of them would add up to noticing that there are windows. There must be a psychological literature on this: surely someone has done experiments where someone looks at a display like:

   TTTTTTTTTTTTTTTTTTTXTTTTTTTTTT

and has to notice quickly (a) whether the letters are all the same (b) what they are. My guess is that people could to (a) more reliably and quickly than (b).

>To be a little more general, you might ask yourself what we need language
>and language-type processes for. What are the things we do better using

Indeed. I started out thinking that language might be based on sequential facilities that we need for all sorts of things, but in thinking about getting a beer, all the sequencing so far looks like an epiphenomenal consequence of the fact that some states have others as preconditions. Syntactic goal states would on the other hand crucially involve temporal integration: one wants to perceive oneself as *having done* something with a given structure, rather than just as being in some state (without temporal internal structure).

Can dogs intend to have done something? I suspect not.   But they do same able to intend to be doing things, e.g. chasing a ball, staying near X, etc.

Reality:
I still think it matters, but also that the subject needs a rest.

For the present I'll be content if people think that the `controlled variables are everywhere' story is less bad that e-/p- control.

Avery Andrews

Date: Tue Jan 21, 1992 4:37 am PST
Subject: CT & human communication; CT ethics

From Greg Williams (920121)

Ken Hacker [920118]

>Control theory has definite relevance, in my "newcomer" opinion, to what we
>call intrapersonal communication. I have yet to see where it can contribute
>to the science of human communication beyond the intrapersonal (self-self
>interaction) plane, but I am open to suggestions.

Ken, notwithstanding Bill Powers' protestations that knowing a LITTLE about someone else's control structure doesn't help much for manipulation, I think that knowing (modeling) even the rudiments of others' control structures can be enormously important in communicating with them -- and I think that most everyone realizes and applies this idea (practically none know explicitly about control theory and the Test, of course, but I maintain that they use control-theory notions and, especially, the Test, in a "folk" way, because they have been found to be so important). Modeling others' control structures as the Royal Road to manipulation and as the Royal Road to benign social control, as discussed in my recent posts, can be subsumed to a large extent under the slogan "the Royal Road to adequate/efficient/effective communication."

So, the science of human communication could progress, I think, by studying how and when individuals attempt to make and utilize models of others' control structures, "why" they think they do it, "why" they sometimes don't try to do it, and how those models could be improved. The investigator could begin with a scenario as simple as the greeting (control-structure probe?): "How are you doing today?"

>The issue is not deterministic connections between social factors and human
>perceptions, but rather influence connections. For example, you say that
>technology cannot MAKE anyone do anything. Of course, but technology can
>CHANNEL the MEANS of doing things and provide TECHNICAL LIMITS for our
>behaviors.

Watch it! You threaten to take away some of the "gee whiz" counterintuitive nature of control theory, which draws potential converts like ripe fruit draws flies. (I'm being sarcastic, of course. But it certainly isn't as impressive (or befuddling) to the neophyte to say: "I can't FORCE you to do something you decide you don't want to do, except by PHYSICAL FORCE. If I do 'force' you (without overwhelming), then that shows you changed your mind, and I really didn't FORCE you.") Of course, I agree with you, Ken. But there is a larger issue here. Sometimes influence (in interpersonal relationships) does involve interpersonal CONTROL, as I have pointed out (and as Bill Powers has agreed, at least in part -- he hasn't answered yet about the extension to "benign" social control). The nonliving environment CANNOT control individuals; but human individuals SOMETIMES control other human individuals (technically, the former control which perceptions the latter control, so you might want to call it joint control -- still, I think the differentiation is valid and important to set such situations off from influence-but-not-control).

[From Bill Powers (920120.0900)]

>If control fails, only overwhelming physical force can overcome the person's
>resistance and noncompliance. If the person sees reason, no force is necessary
>-- and there is no control.

If the person sees reason because another is controlling for the person seeing that reason, no force is necessary, and there is at least JOINT control.

>Control theory suggests quite a different principle of social
>interaction: not the gaining of control over others, but the granting of
>control to all.

Control theory "suggests" different things to different people. Inherently, it does not even say that "conflict is bad." Conflict is JUST THERE under certain circumstances. PEOPLE take the descriptive theory and ADD the ethics to it. Another possible ethics besides the one you add is "use control-theory modeling of others to control them in ways which will minimize opposition." I'm not arguing for this as the "best" ethics based on control theory, but I think it is important to realize that control theory can lead in more than one ethical direction, some more "controlling" than others.

>Attempts to control living systems automatically create opposition.

Opposition CAN be created by attempts to control, and is OFTEN created by the outright-coercive techniques of those who ignore others' control structures. But there certainly can be interpersonal (at least joint) control with NO opposition.

Enough recreation for one day....

Greg

Date: Tue Jan 21, 1992 5:40 am PST
Subject: walls of windows

[From: Bruce Nevin (Tuesday 920121 0704)]

Re walls full of windows, some words for that perception include group and set. The plural on "windows" I would account for as in one of my posts this weekend, e.g.

```
        a group consists of a window and a group consists of a window
          and a group consists of a window . . .
        a group consists of a window and a window and a window . . .
        a group consists of windows
        a group is of windows

        A group--said group is of windows--is visible
        A group which is of windows is visible
        A group of windows is visible
```

There are some interesting issue here about attending to the perception of the aggregate (group is visible) or to the members individually (group of windows are broken), but I haven't time just now to do more than indicate it. I think it underlies the confusion prevalent among users of English regarding number agreement with aggregate nouns (a number of windows is vs. a number of windows are).

Bruce
bn@bbn.com

Date: Tue Jan 21, 1992 8:48 am PST
From: jbjg7967
          EMS: INTERNET / MCI ID: 376-5414
          MBX: jbjg7967@uxa.cso.uiuc.edu

TO: * Dag Forssell / MCI ID: 474-2580
Subject: Re: your note of Jan 14?

[from Joel Judd]

Dag,

Sorry for the ambiguity. I had wanted your e-mail address and deleted the post before copying it.
My address is an internet one.

I am writing because I am commencing that enjoyable process known as "job search." I have gotten the impression that you are affiliated with some sort of human relations/consulting operation? I think I saw a brochure that Gary has. In any event, from you and Ed and others I have felt that there is useful work that could be done in consulting/counseling settings (based on PCT ideas) for businesses and other groups that need certain types of skills. In my case, I have been considering how I might approach firms which need intercultural linguistic skills for their employees. There is one group I know of which does something like this (Clark Consulting of San Mateo) and I've written them directly for any information they're willing to divulge about their work. I thought I would ask you for the same. If there's prepared info that won't take you too much time to gather, that's fine. I'm just looking for ideas on how to apply PCT right now. If I'm not being clear enough, or if I've misunderstood what you do, please let me know.

Regards

Date: Tue Jan 21, 1992 8:58 am PST
Subject: help with imagination

from Ed Ford (920121.09:30)

To Bill, Rick, Martin, Tom, and others...........

I was intrigued by an example of Martin Taylor's (920117) piano playing and his comment "It takes hearing some tape recordings of one's own playing to be able to hear what one actually does, and thereby improve it." After much reading and rereading of the various postings on this subject, I am still confused as to what part the imagination plays in the perception signal when one is a) trying to recall something in the past or even recent present and b) trying to imagine what one is going to do in the future. One of my daughters had recently decided to quite her very secure job as owner/operator of a dog kennel and just groom dogs out of her home. My son, Joseph, wisely suggested she write out what all her expenses would be in her intended way of living. Once she did this, her imagination soon met the reality of what her future income vs expenses would be and she found her present situation much more to her liking. When working with clients, I find when clients begin to write things out, set specific goals, keep track through charts and lists, of what they are doing, they are more inclined to achieve their future goals. Some how, in my mind, I think the theoretical explanation of how the imagined perception interacts or relates to the perceptual signal and/or the reference signal should be most helpful to those of us who are tring to help others more effeciently reorganize their lives. Often, I find the more specific I get with clients in terms of either past or present actions or future plans, the more concerned they seem to be. I would ask that all comments be loaded with nice, easy-to-understand examples. I need lower order experiences to help me understand the higher level theories.

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

Date: Tue Jan 21, 1992 11:40 am PST
Subject: EMG Literature

[from Gary Cziko 920121.1230]

Tom Bourbon (920120.1351) said:

>In general, when a person holds force constant, both the
>amplitude of EMG and reports of felt effort increase. When a person
>holds a feedback signal that is an analogue of EMG constant,
>force decreases, and there is USUALLY no reported change in felt
>effort (the published rresults concerning effort are somewhat
>contradictory.) Finally, when a person holds constant reported
>sensed effort, force decreases and the reports concerning EMG are
>ambiguous.
>   Tomorrow (920121), one of my thesis students, Laura Hamilton,
>will begin collecting data in an attempt to resolve some of the
>ambiguities I mentioned above. She began planning her project about
>three months ago, out of curiosity over the same points you raised,
>Gary.

Tom, you or Laura Hamilton post some references to this literature? At the least, I'd like something showing that:

>In general, when a person holds force constant, both the
>amplitude of EMG and reports of felt effort increase.

I realize that all this stuff is already inherent in the tracking data, as Bill Powers noted. But somehow I find the lack of correlation between motor signals and behavior even more amazing than the lack of correlation between cursor and behavior. And I'd like to be able to tell people that this is the case and know the literature to back me up.--Gary

Date: Tue Jan 21, 1992 12:17 pm PST
Subject: Sequence detection

[From Bill Powers (920121.1100)]

Bruce Nevin (920120.1030) -- also Martin Taylor and Joe Lubin

A little more detail on the sequence detector's possibilities.

Apparent digression: in the BYTE article, part 3, there are two big diagrams. The second one shows an arrangement closer to real neuroanatomy, with all the perceptual functions grouped together (sensory nucleus). With all input functions located near each other, interactions can happen: in fact, the whole nucleus may be considered one very complex computer, with perceptual signals being pulled out of it at many places (this may be of interest to Martin and Joe, too). Each perceptual signal then goes to a comparator and output function (in a motor nucleus) to make a control system. The output function establishes connectivity to lower systems, and does any weightings that are done.

Suppose this is also the case at the sequence level, and furthermore that sequence detection is done by the linked-latches approach. I will review the linked-latch model for those who don't have BCP handy.

Each latch is turned on by one or more input signals. The total excitation must have a value of 3 to allow an output to appear (all latches have a response threshold of 3 units of excitation). A single signal with three presynaptic branches, giving the signal 3 times the effect of one signal, can cause the latch to produce an output signal. Or one input with a weight of 1 and a second one with a weight of 2 can result in an output. I'm assuming that signals are continuous, and are either ON (maximum frequency) or OFF (zero frequency).

The first latch in a chain receives a perceptual signal indicating presence of the first element of the sequence (presumably, the output of a category detector). This signal has an input weight of 3 (3 synapses). The output of the first latch branches, one branch feeding back positively with a weight of 3. Thus if the first element is sensed, the latch responds with a steady output signal and, via the positive feedback connection, holds itself on even if the input is then removed.

All subsequent latches receive a copy of the output signal from the previous latch with a weight of 1 (no branching), and the corresponding feedback signal has a weight of 2. The perceptual input signal to each latch after the first has a weight of 2. The weight-1 signal from the previous latch, plus the perceptual signal with a weight of 2, is sufficient to turn on the latch (total excitation = 3). The positive feedback signal with a weight of 2 plus the input from the previous latch with a weight of 1 is sufficient to keep the latch turned on even if the second perceptual input then disappears.

Thus each latch turns on and holds itself on, and enables the next latch, as the relevant perceptual input occurs.

The entire string of latches can be turned off by an inhibitory signal that turns off the first latch. That removes the enabling input from the second latch, which turns off, removing the enabling input from the third latch, and so on. Because the weights of the perceptual inputs are only 2, the latch will turn off when the enabling input turns off even if the perceptual input is still present.

A particular sequence-recognition chain can be selectively turned on if the first perceptual input also has a weight of 2. Then an enabling signal with a weight of 1 will turn on that sequence recognizer and those that are linked to it.

There are 10 zillion other ways to accomplish the same effects, but this one would work.

One pertinent fact. When a neural signal branches, it carries a copy of the same signal in both branches. The amount of signal is not divided by 2. So we can tap signals off any pathway without weakening the signal in either path. This property results from the active transmission property of neural fibers.

Now, in the following diagram, each O represents a latch, with the positive feedback path not shown. A simple sequence detector would be arranged this way:

```
         (enable next)       (Sequence present)
          /                      /
     O ---> O ---> O ---> O ---
     |      |      |      |
     Inputs from lower systems
```

We can now let this sequence-detector grow some branches. The * symbols indicate where a sequence-present signal appears (two different sequences having the same two elements at the beginning are detectable).

```
                 O ---> O ---> O ---> O --- *
                /|      |      |      |
               /
     O ---> O ---> O ---> O --- *
     |      |      |      |
```

There is nothing to prevent the second chain from re-entering the first, if two different sequences are not distinguished:

```
                 O -> O
                /      \
               /        \
     O ---> O ---> O ---> O --- *
```

A more sophisticated sequence detector could use a layer of AND functions to distinguish between these two sequences (only the first three elements are shown -- signals would be extracted from all relevant latches for each AND):

```
     Seq 1      Seq 2 (has occurred)
       |          |
      AND ___    AND
     |   \   \/ /  |
     |     \ /\/   |
```

```
     |   ---|-/ \  |
     | /    |    O -> O
     |      |   /  |   \
     |      |  /   |    \      (inputs from lower systems not shown)
     O ---> O ---> O ---> O
```

Clearly, this kind of network, with or without the AND layer, could have many interconnections. Also, the ability to return to the original path allows for sequences that have a common ending as well as a common beginning.

Signals can be extracted from this network anywhere, so a word like "establishment" could also lead to a signal indicating "establish", at the same time (or appearing slightly earlier). I assume here that letter-sequence is being detected, as in spelling, rather than whole-word sequence, as in making sentences. Either would use this same sequence level, but the input signals would indicate different kinds of configurations.

The above network with N output signals extracted could be re-represented as N perceptual functions, each producing a perceptual signal that indicates presence of a different sequence. This is the best representation for drawing control systems. But the above network diagram shows that the duplication of function needed in the re-representation need not actually occur.

This principle may work at most if not all levels of perception -- a dense and highly interconnected network of computations with perceptual signals being extracted from various points within the network. The KIND of computation that recurs in the network determines the meaning of the level of perception. Of course there can be several independent networks at the same level, based on the same kind of computations but not interacting directly.

Martin and Joe, the kind of perceptron you get depends on the assumed elementary computation that takes place, doesn't it? If the elementary computation is weighted summation, I think you get sensation-type outputs. Perhaps a different kind of elementary computation would be needed to derive true configuration signals for form recognition. And if the basic computation is latching, as above, you get sequence detection.

Enough for one post...

Bill P.

Date: Tue Jan 21, 1992 12:28 pm PST
Subject: Re: Sequence detection

[From Joe Lubin (920121.1400)]

Bill Powers (920121.1100)

Your latching diagrams look very similar to proposed neural architectures for motion detection involving unidirectional lateral inhibitory links to confer directionality.

> Martin and Joe, the kind of perceptron you get depends on the assumed
> elementary computation that takes place, doesn't it? If the elementary
> computation is weighted summation, I think you get sensation-type
> outputs.

Sounds fine to me.

> Perhaps a different kind of elementary computation would be
> needed to derive true configuration signals for form recognition.

I don't follow this.

> And if
> the basic computation is latching, as above, you get sequence detection.

Yes.

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

Date: Tue Jan 21, 1992 1:09 pm PST
Subject: REMARKS - BOB CLARK

REMARKS - BOB CLARK

from Bob Clark

Having now caught up a bit on higher priority activities, I've reviewed the astonishing number of messages in my IN BOX. A quick scan revealed a great diversity of interests, ideas, etc.

I note there is another CLARK included in the CSGNET. I don't find him on my copy of the distribution list, but we should probably be somehow separately identified. If it is ok, I'd just as soon be called "Bob Clark."

My orientation is one of trying to clarify and simplify the basic structure of the Hierarchical array of Control Systems. In the process I noticed the discussion of Thermostatic Systems between Powers and Izhak (in Closed Loop #4) which seemed to end rather inconclusively. In my submission COMMENTS ON CLOSED LOOPS #3 & #4, I suggested the role of the OBSERVER should be included in the discussion. (I coauthored a paper with McFarland and Bassan, "Integrated Data Collecting and Processing Systems in Psychophysiology," New York Academy of Sciences in 1964.)

Further thought about THERMOSTATS led me to notice that they not only provide a convenient illustration of a Control System (I've used them repeatedly for this purpose), but they can provide a tie-in illustration of a Hierarchical Structure of systems couched in terms familiar to most people these days.

As I developed these relationships, I found I was using some familiar words that are usually taken for granted, but could well have their basic definitions clarified -- especially from the Hierarchical Structure standpoint.

I plan to offer these (hopefully, soon) in a more extended form in a separate communication.

Regards, Bob Clark

Date: Tue Jan 21, 1992 7:01 pm PST
From: Dag Forssell / MCI ID: 474-2580

TO: csg   (Ems)
          EMS: INTERNET / MCI ID: 376-5414
          MBX: CSG-L@VMD.CSO.UIUC.EDU
Subject: Promoting PCT
Message-Id: 90920122030109/0004742580NA3EM

From Dag Forssell [920121]

Subject: Teacher List

In recent months, a few people have mentioned on the net that they will be teaching classes on PCT.

As I begin to promote PCT to industry, I would like to have a list of these teachers as a reference document for those who may ask.

Following is a list of those who to my understanding are teaching PCT in some form now. By implication, I assume that other CSG netters are doing research, personal studies and preparing to teach at a later time.

Please correct me as required and let me know if it is OK with you to be on such a list. I will be happy to republish the list with corrections.

To put this into perspective: How many universities, departments and professors are there in the education, psychology and sociology universes?

I will mail this post to Hugh Gibbons, David McCord and Phil Runkel, as they are not on the net.

Tom Bourbon, Professor
Department of Psychology
Stephen F. Austin State University
Nacogdoches, Texas 75962

Gary A. Cziko, Assistant Professor
Educational Psychology
University of Illinois
1310 Sixth Street
Champaign, Illinois, 61820-6990

Edward E. Ford, MSW, Lecturer   Author: Freedom from Stress
Arizona State University   Brandt Publishing, AZ

Phoenix, Arizona   ?????

Hugh Gibbons, Professor   Author: The Death of
Franklin Pierce Law Center   Jeffrey Stapleton.
Concord, New Hampshire 03301   Exploring the way
                                                     Lawyers think.

Wayne Hershberger, Professor   Editor: Volitional Action,
Department of Psychology   Conation and Control
Northern Illinois University   North Holland, N.Y.
DeKalb, Illinois 60115

David M. McCord
Department of Psychology
Western Carolina University
Cullowhee, North Carolina 28723

Clark McPhail, Professor   Author: Myth of the Madding Crowd.
Department of Sociology   Aldine de Gruyter, N.Y.
University of Illinois
Urbana, Illinois 61801

Hugh G. Petrie, Dean
Graduate School of Education
State University of New York
Buffalo, New York 14260

Richard J. Robertson, Professor   Author: Introduction to Modern
Department of Psychology   Psychology;
Northeastern Illinois University   The Control Theory view.
Chicago, Illinois ??????   CSG, KY.

Philip J. Runkel, Professor   Author: Casting Nets and
Education and Psychology   testing specimens;
University of Oregon   Two grand methods of
                                                     psychology. Praeger, N.Y.

Charles Tucker, Professor
Department of Sociology
University of South Carolina
Columbia, South Carolina 29208

Comments and suggestions anticipated. Thanks!

Subject: How is PCT different?   Take one.

(Question anticipated from prospective client). (Recognition due to
Rich Marken in recent post and Tom Bourbon in Indiana meeting).

Let me answer by asking you two questions. First, let me ask you: What is the predominant idea or paradigm about how and why people behave? ... People respond to their environment, right? -Right! - So what you experience or perceive in your environment determines what you do.

That is why most management programs on how to deal with people tell you how to push peoples "buttons" so they do what you want them to do.

You must know what buttons to push in what situation. That makes it hard, right? - Right!

It works some of the time, but not all the time; OK?

Our program will show you how this is true, but not the whole truth.

Now let me ask you: Is there another idea or paradigm of how and why people behave? ---- Have you ever played Solitaire? You sit alone in a quiet room and just think. There are no disturbances in your environment. You just imagine something, set a goal and do it. For instance, you think about the cards, set a goal to place the red queen on the black king and do it.

So another paradigm or idea is that what we think, our goals, determine what we do.

This is true some of the time, but not all the time. You are not always alone.

Our program will show you how this is true, but not the whole truth.

We will show you how both perceptions of the environment and internal goals relate to action. You will see how a system of perceptions, goals and action works all the time.

This means that you will always understand the nature of what is going on in yourself and others.

Subject: How is PCT different?   Take two.

(Recognition due to Greg Williams for n-1 explanation idea).

PCT is a paradigm or theory of human activity which offers you greater powers of explanation than any other approach I know of.

Let us talk about theories.

For many people, a theory is nothing more than a generalization of some observed fact. As an example, I might suggest: I have a theory that all Swedes are blond. (All the ones I have ever seen in the movies sure look that way). You might disagree and say that only 80% of them are blond. Some will argue that if you can find one who is not blond, the theory is refuted.

Some scientists argue over percentages of observed facts. These are sometimes called "soft" scientists.

Other scientists do not think that percentages of observed facts without explanation qualifies as science, but want explanations, what they think of as theories, for the observations.

Think about the "hard" sciences. If you ask about a mechanical phenomenon (a fact you have observed), a scientist can provide an explanation. If you ask him about the explanation, he can explain it. In the hard sciences, these explanations or theories go many layers deep. Explanation for explanation for explanation for explanation and so on. As they have been suggested over time, these explanations have been used to predict what will happen under many varied circumstances and when they appear to predict without ever missing, the scientist thinks of his explanations as laws of nature. The explanations seem infallible.

When you dig very deep, you get down to the atoms and parts of atoms. You learn explanations for forces within atoms as well. When you press the scientist, he will admit that our explanations at this level are guesses only. There is no agreement on these phenomena. Interestingly, you find that chemists, biologists, physicists, electrical engineers - you name them - are all looking at the same explanations at the bottom of the explanation "layer cake."

What is different about PCT is that instead of being satisfied to argue statistics about phenomena of the mind (without any explanation at all), PCT provides several layers of explanations in a detailed model. The model is used to provide predictions (under many varied circumstances) which can be tested against experience and give you confidence.

PCT in its approach to theories and explanations is more a hard science than a soft one. That explains why it is much more useful, but also why it requires more effort to study it in depth.

Our program, Purposeful LeadershipTM, starts out with applications, so the program is useful right off the bat. The structure is kept very simple and fun. Anyone who wants to, can study it in depth, many layers down.

This seems too long for casual conversation. Perhaps OK for brochure. Any suggestions for cuts, re-wording?

Greg Williams:

Just noticed in Closed Loop #4, page 45, line 4: Should be Bill Glasser's followers, not Powers'. If it is ever re-printed, please correct.

Tom Bourbon:

May I ask for a copy of "Models and their worlds". Will be happy to pay.
Thanks!

Thanks to all.
Dag Forssell
23903 Via Flamenco
Valencia, Ca 91355-2808
Phone (805) 254-1195   Fax (805) 254-7956
Internet: 0004742580@MCIMAIL.COM

Date: Tue Jan 21, 1992 8:29 pm PST
From: Dag Forssell / MCI ID: 474-2580

TO: csg   (Ems)
          EMS: INTERNET / MCI ID: 376-5414
          MBX: CSG-L@VMD.CSO.UIUC.EDU
Subject: Promoting PCT, continued
Message-Id: 84920122042948/0004742580NA3EM

From Dag Forssell [920121b]

Further on take two.

An upside down layer cake pyramid of explanations/theories.

```
   Mechancial    Electrical    Chemical    Biological     Living
   Phenomenon    Phenomenon   Phenomenon   Phenomenon   Phenomenon

\  Explanat   \  Explanat  |  Explanat  |  Explanat /  Explanat     /1
  \____________\___________|____________|__________/______________/
    \    explanation for expl  |  exp f exp  |     exp f exp    /    2
      \________________________|_____________|________________/
        \    explanation f exp f exp  |  exp f exp f exp    /        3
          \___________________________|___________________/
            \       explanation f exp f exp f exp       /            4
              \_______________________________________/
                \         Fifth level deep          /                5
                  \_______________________________/
                    \         Atoms             /                    6
                      \_______________________/
                        \     Protons       /                        7
                          \_______________/
                            \  Quarks   /                            8
                              \_______/
                                \ ? /                                9
                                  \/
```

This is intended as a conceptual chart. I have no basis for hom many
levels to suggest.
Recent discussions on the net have been very helpful to me in sorting out
the Popper vs Kuhn vs Powers vs Statistics arguments in my mind.

Dag Forssell
23903 Via Flamenco

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Phone (805) 254-1195   Fax (805) 254-7956
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Date: Tue Jan 21, 1992 8:17 pm PST
Subject: DEMO 1 NOW AVAILABLE!!

[from Gary Cziko 920121]

[Preliminary note: This is a message that you will want to SAVE for future reference if you are interested in obtaining computer programs or document files from the "CSG file server."]

I am pleased to announce that Bill Powers's Demo 1 ("The Phenomenon of Control") is now available electronically, thanks to the cooperation and assistance of Bill Silvert of the Bedford Institute of Oceanography in Dartmouth, Nova Scotia. Demo 1 will run on IBM PCs and compatible machines with 286 (AT class) or "better" CPUs and a mouse. Demo 1 will demonstrate what perceptual control is using you or another person as a subject. It is organized in a step-by-step tutorial format with explanation alternating with hands-on demonstrations and data analysis.

Rick Marken (920120) has observed that:

"There is a growing group of advocates of control theory . . . who don't understand what they are doing. They know nothing of modelling. They are just using some of the terminology of control theory as parts of their verbal explanations of the usual multiple subject, significance test, garbage data."

DON'T LET THIS HAPPEN TO YOU! Working through Demo 1 and Demo 2 (soon to be available) will save you from this deplorable fate.

There are two ways to obtain Demo 1:

First Method: File Transfer Protocol (ftp)

Using ftp is the easier of the two ways to obtain the program as you will receive a single binary file ready to run on your PC. (My understanding is that just about everyone on Internet can ftp. Bitnet and MCI Mail users may have to use e-mail as described below.) If you know how to obtain files via anonymous FTP, all you need to know is that you want the binary file dem1a.exe on the pub/csg subdirectory of biome.bio.ns.ca.

If you have not used ftp before, here is a guide to getting this file:

Start off by typing:
ftp biome.bio.ns.ca (or ftp 142.2.20.2)
You will get a login prompt. Login as anonymous.
You will get a password prompt. Enter your last name (this is just a courtesy).
You will then receive a message, followed by a prompt. Issue the following commands:
cd pub/csg
binary
get dem1a.exe   (This copies the file to your machine)

quit

After you get the file dem1a.exe off your mainframe, put this file on a subdirectory on your hard disk or on its own otherwise empty floppy and type "dem1a.exe". This will "explode" the zipped file into a dozen or so program files. Then to run Demo 1 type "demo1" (but make sure you have your mouse connected and active first). You can then discard dema1.exe (or better yet, archive it) since it is no longer necessary to run Demo 1.

You can do the above directly from your PC if it is connected to Internet in which case the file will be transferred to your machine's active disk. If you are using a terminal connected to a "mainframe" machine, you then need to figure out how to get it off your mainframe onto a PC diskette readable by your computer (your local computer services office should be able to help you with this, but make sure they know that it is a BINARY file).

Second Method: Electronic Mail (e-mail)

Anyone reading this message (including those using MCI Mail) should be able to obtain Demo 1 via e-mail. You cannot send binary program files via e-mail since e-mail systems expect to receive regular ASCII ("typewriter") characters and binary files contain non-ASCII information. Therefore, the file dem1a.exe has to be encoded into ASCII before it can be sent. The program used to change program (binary) files to mailable ASCII files is called uuencode and the program used to turn the ASCII encoded files back into binary programs files is called uudecode.

So, to get the uuencoded version of Demo 1, you just send a ordinary mail message to server@biome.bio.ns.ca and include in the message the line:

uuencode pub/csg/dem1a.exe

You will then receive probably four files from the mail server. You need to concatenate these files in the proper order and decode them. If your system has only the standard uudecode program, then you have to edit the concatenated file to remove the headers, so the file starts with a line that says 'begin 644 dem1a.exe' and ends with a line saying just 'end', with a whole bunch of cryptic lines in between (most of these lines begin with M, but there are two other lines that you have to leave in just before the end statement). You can use a word processor for this editing and concatenation, but make sure that you save the file in ASCII format (also sometimes called nondocument or plain text format). However there are many enhanced decoders that can do this automatically, so you just have to save the parts in order into a file and decode them without editing.

* *

We plan to make other programs available in the future, including Bill Powers's Demo 2 and the Arm demonstration. While it may seem like a hassle to get these files, this is in the long run much more convenient than mailing floppies. Anybody taking the time to read CSGnet traffic should definitely work through Demo 1 and Demo 2 to get a feeling for what perceptual control is and what the basic PCT model is.

Finally, I attach Bill Powers's invoice form for these programs. Since Bill is now living on fixed retirement income, I'm sure he would appreciate being properly compensated for his work on these marvelous programs. If only half a million of these programs are purchased, Bill will be able to buy an old hotel in downtown Durango for conversion to the much needed Institute for Control Systems Research!

Date: Tue Jan 21, 1992 8:18 pm PST
Subject: Thermostat vs. Cruise Control

[from Gary Cziko 920121.21.45]

Bob Clark (920121) said:

>Further thought about THERMOSTATS led me to notice that they not only
>provide a convenient illustration of a Control System (I've used them
>repeatedly for this purpose), but they can provide a tie-in illustration of
>a Hierarchical Structure of systems couched in terms familiar to most
>people these days.

Kent McClelland introduced the automobile cruise control system for this in a   paper which I borrowed for a paper and book chapter of mine. What's nice about the cruise control system is that they just don't click something on or off, but have a continuous output (throttle). They also control against disturbances on both sides of the reference level (more throttle if speed to low, less if speed to high) while thermostats usually just work on one side of the reference level (the furnace doesn't do anything to cool the house if it gets to hot; the air conditioner does nothing if it gets too cold).

I look forward to your extended comments.--Gary

Date: Tue Jan 21, 1992 10:25 pm PST
Subject: Zeroings, words, and meanings

[From Bill Powers (920121.2100)]

Bruce Nevin (920121) --

In your post commenting on the "windows" problem you propose a base form of a statement:

>   a group consists of a window and a group consists of a window
>   and a group consists of a window . . .

This, I think, may get me a little closer to defining the problem concerning base forms, as I see it.

This form is made up of repetitions of the unit "a group consists of a window," connected by "and". Now I ask what perceptions are indicated by any one letter-group subunit, or any combination of the subunits. As stated, they are no more meaningful than asdf weporiu wqeoriu zxc. What I have been driving at all this time (or at least toward) is that although a phrase like "a group consists of a window" seems to be fraught with meaning, as if the very letters and groups of letters were speaking to us, in fact it is a nonverbal process hiding behind the word-symbols themselves that is giving us this sense of meaning. If the arrangement of the words is significant, then some nonverbal sense of arrangement is providing the significance. If the word "a" is different from the letter "a", the difference is a silent perception. There is no way to get the difference out of an analysis of letter-groups alone.

Or to put that a little differently: it is nearly impossible to view these familiar groups of letters and the familiar arrangements of them relative to each other without bringing into play the basic machinery of perception that gives them and their arrangement an aura of meaning. It is nearly impossible to avoid attributing meaning to the letters and groups themselves. The only way to avoid this is to create nonsense groupings of letters at random, so that neither the groups nor their arrangement in space and time evokes any sense of familiarity. But then, of course, one does not get any sense of looking at "language." The underlying machinery stops; the background hum of meaning disappears. One is just looking at a bunch of letters which have no voices and remind us of nothing.

It would be possible to create an artificial vocabulary of random groups of letters. Some of these letters could arbitrarily be assigned the status of operators; others the status of arguments (or verbs and nouns and so on). Sentences could be constructed in which all the statistical rules of operator (or other) grammar were followed. I suppose there would be other classifications such as articles and pronouns. All the rules that have been derived from usage could be applied using this vocabulary -- and the resulting sentences would convey nothing, at any level. Word order would mean nothing, because the "words" would not relate to perceptions, and their ordering would not be given meaning by the ordering of perceptions as they actually occur. Prepositional phrases (xtpo dsof prng) would yield no sense of relationship because there would be no underlying perception of a relationship between the perceptual referents of the words, and the typical behavior of those referents -- not the words -- in relationship to each other. Classifications would be arbitrary and meaningless, because there is no underying sense of treating a group of perceptions as equivalent or interchangeable.

To repeat a story I have told a tedious number of times: I came to this realization about words after spending a couple of years trying to find a hierarchy of perceptions written as words and layers of words and dependency and inclusiveness relationships among words. Bob Clark knows all about this: he went through this with me, putting as much energy and ingenuity into it as I did (Two Years Before the Blackboard -- remember, Bob?). It was Bob who finally characterized what we were doing as "castle building" -- building hypothetical dream-castles in the air, out of words. The insight that put an end to this futile project came out of the air between Bob and me: it was not the words we should be looking at, but the perceptions to which they point, which are not words. Control systems control perceptions, not the names of perceptions (unless one is specifically controlling for the construction of sentences and so on). And even a WORD is a nonverbal perception, in the final analysis. It is just a signal, distinguishable from other signals but having no inherent meaningfulness or special properties that other perceptions don't have.

This is what finally put us on the track of the hierarchy. The relationships and typings we were looking for were not to be found in the words we used, but only through looking at words as pointers and trying very, very hard to become conscious of the experiences, the nonverbal experiences, to which the words referred. Bob and I were both highly verbal people, used to communicating in words and pretty good at it, so this was a very difficult and drawn-out exercise. It required wrenching apart the words from their meanings, so that the meanings could be apprehended alone, without the words. I don't think either of us really understood at first what it was that we were trying to do. But when we came up with a "level", it was a nonverbal level for certain. Since then I have seen that what we then took to be unitary levels merged types of perceptions we had not yet distinguished from each other. I think that Bob considers my 11 levels too many; I suspect they are too few. But that's another subject. The central point here is that separating words from meanings is no easy task, and carries no assurance of being right in any final sense, but it is the essential KIND of thing that must be done even to know what is meant, in PCT or HCT, by the term "perception."

One other point. Bob and I came up with general principles, but they were principles of control and hierarchical relationship -- not generalizations about any particular perceptions (or words) at any particular level. The specific structure of the hierarchy that was proposed did not come from generalization, but from detailed examination of real examples, one at a time, as we discovered and explored them. There is no generating principle that will tell you, given knowledge of the nature of level n, what level n+1 ought to be. The content of each level, insofar as we were able to characterize any level, was found empirically.

The separation of word from meaning is an empirical phenomenon. There is only one way to appreciate what that separation signifies, and that is to practice doing it. Doing it means picking out words and trying to see what experience they indicate -- not what other words, but what experience. One's vocabulary quickly sorts itself out into two classes: one class contains the words that indicate things you can in fact experience. The other class, which is far larger than anyone is likely to be comfortable with, is very close to being devoid of experiencable referents. Words in the second class sound as if they ought to be meaningful, but they aren't. Not when you *actually look* to see what is indicated by the word.

When doing this, it's essential not to pick on words that only indicate concrete things. There are many "abstract" words that indicate easily experienced referents. The referents are higher levels of perception, but there is no doubt that they are there. The definitions of levels may make it easier to discover what these perceptions are, although the type-names are only the names of the levels and not the levels themselves.

Having found high-level words that do indicate high-level perceptions, it then becomes much more obvious when a high-flying word proves to have no anchor in experience. This is not to say that it becomes any less embarrassing to make this discovery about favorite words.

Bruce, I haven't forgot you even though I'm clearly broadcasting to a wider audience by now. In the light of the above, I'm asking you (and anyone else so inclined) to do a little research in that lab between your ears. To what experiences do the following terms point -- not in principle, not in general, not according to theory or a dictionary, but IN FACT and IN YOU and AT THE TIME YOU CONSIDER THE QUESTION?

Group
a
window

 (or AWindow)
and
consists
of
 (or ConsistsOf)

I suggest that the real "base form" of a sentence is composed primarily of wordless experiences. I suggest that some of the expansions you have offered are really attempts to describe the underlying nonverbal experiences to which the uttered form refers. I suggest that there is no level at which the fully expanded base form is present IN WORDS. These base forms, I suggest, sound prolix and awkward because they are only wordy approximations to the nonverbal experiences that are really there.

Of course that means that the base forms may lead us to see what the perceptions and types of perceptions behind language really are. That's more or less the path that Bob Clark and I followed.

Best,

Bill P.

9201D CSGnet

Date: Wed Jan 22, 1992 4:43 am PST
Subject: BURN/UNBURN

From Greg Williams (920122)

To judge by the protocol given by Gary C., downloading and decoding files made with uuencode is a pain. It is hard for some even to concatenate files. We sent a beta version of BURN/UNBURN to Bill Powers a while back, but have heard nothing -- did you get it, Bill? Does it work? BURNing and UNBURNing automatically work with large files -- no concatenation or header-stripping needed -- and a copy of UNBURN can be obtained via e-mail (decode by using DEBUG program which comes with MS-DOS). So, for IBM users, this easier option will be available when Bill tests it. The source (ANSI C) is available for non-PCers.

Greg

Date: Wed Jan 22, 1992 5:26 am PST
Subject: correlating language with experience

[From: Bruce Nevin (Wed 920122 07:05:46)]

(Bill Powers (920121.2100)) --

I still agree with the points in your broadcast about the relation of words (as "verbal perceptions") to nonverbal perceptions. I still am unclear what the mechanism of correlation might be.

```
Group                   Countable things perceived together
a                       [it's countable]
window                  stereotyped visual image: rectangular frame, +-+
                        clear panes top and bottom                   +-+
                        Seems to fit the "exemplar/extensions"       +-+
```

```
                        view of categorizing
 (or AWindow)           [The "a" Harris says is automatic  morphophonemics
                         on count nouns.  I would take it back to a real
                         or imagined act of counting "one" and "a/an" as
                         a reduction of "one"--historically true.]
and                     I've got more to say
consists                Focus attention on the next level of detail of
 (or ConsistsOf)        ["of" is empty, an argument indicator]
```

>I suggest that the real "base form" of a sentence is composed primarily
>of wordless experiences.

Not all aspects of wordless experience correlate with words. That is why the wordless experiences are not the base forms for language. I think what you mean is that the wordless experiences can be the origins of motivation for using particular words. (Can be, but are not always: as we well know, people spend much of their energy out of their senses with words--a kind of imagination loop replacing the correlation with experience.)

>I suggest that some of the expansions you have
>offered are really attempts to describe the underlying nonverbal
>experiences to which the uttered form refers.

Yes, language attempts to describe nonverbal experience.

>I suggest that there is no
>level at which the fully expanded base form is present IN WORDS.

I would say rather that the correlation of nonverbal experience to words is always incomplete. The fully expanded base forms for language have only a loose correlation with the experiences to which they refer. They evoke experiences in memory and imagination, which in turn may correlate with aspects of real-time experience. That evocation, and that further correlation, are together a process of interpreting the linguistic information in an utterance.

I think the distinction between the unreduced base forms for language and the experiences with which we correlate them is essential (has to do with their respective essential natures). Language is an agreed-upon and learned social artifact, and that is why the character strings I am typing are words for you where "c'e tuc:o tinaw:am:i" is not made up of three words for you. (It is for me, and would be for any speaker of Achumawi who had learned the writing system.)

>These base forms, I suggest, sound prolix and awkward because they are only
>wordy approximations to the nonverbal experiences that are really there.

The details of the correlated signals in the perceptual hierarchy for nonverbal perceptions at least as prolix and complex (and I believe even more so, because of detail that escapes the net of conventions that constitutes language).

If I perceive a group of things--windows, sheep on a hillside--I probably do not count them, but I have an immediate apprehension that I could if I wanted to. Perhaps the ECS producing the perceptual signal for the group accepts a signal in imagination mode, but that imagination-mode signal is a counterfeit for signals from ECSs that perceive the countable constituents of the group one by one. Similarly for the plural I do not need to actually say (inwardly) "a window and a window and a window . . .", an imagination-mode signal from the control systems that could say that will do; and in fact the ". . ." part of the source can only mean something going on in imagination.

>Of course that means that the base forms may lead us to see what the
>perceptions and types of perceptions behind language really are.

Yes.

The benefit of undoing the reductions and getting to the base forms is to clean up the words so that they say in a fully explicit way what we intend by the reduced forms. If I say "I want to eat" the thing that I am saying I want is described in words as "that I should eat" not that anyone else should eat. The unreduced source makes this explicit:

    I want that I should eat ==> I want to eat
    I want that you should eat ==> I want you to eat

There is no perceptual correlate of "to" that is obvious until you undo the reductions. When you undo the zeroings and the reduction of "should" to "to", then you see that there is a perceptual correlate of "to" precisely insofar as there is to the subjunctive "should" plus the particular dependencies under "want," the conditions in the base form under which the reductions can be carried out. Those perceptual correlates--whatever they may be--are present for the reduced form precisely to the extent that they are for the unreduced form. It's just that you can no longer see what you are talking about in the latter.

It gets worse with ambiguity, of course. "I saw Bill and Mary" could mean either "I saw Bill and I saw Mary" or it could mean "I saw the couple Bill and Mary" i.e. "I saw Bill with Mary" (roughly--the "with" subordinates one to the other rather than being transitive, like membership in a group).

You can't hope to investigate the correlation of words with nonverbal perceptions until you know the base vocabulary. Then you don't try to map from a fully expanded sentence to perception. Make the correlation as each operator is asserted of its arguments. Make the correlation for each word in the immediate operator-argument dependency subtree. Then go ahead and do the reductions that are immediately possible with the entry of that operater on its arguments. And so on: the successive operators come in on their arguments after the prior round of reductions. After each round of assertion, correlation, and reduction (preserving the correlation in "disguised" form), you have a manageable, usually conventional, and relatively non-prolix sentence. You keep the explicitness and the prolixity only long enough to make the correlation with perceptual experience.

Does that help?

We're moving our offices Friday. I'll be out tomorrow (birthday) so I must do all the packing up today or be damned (so to speak). I'll collect mail on Friday and try to look at it over the weekend. Sarah is doing much better and is I think chastened enough not to overtax herself again as soon as she feels recovered. The sun is shining in a clear blue sky belying predictions of snow. The parking lot below is slowly filling up. Thanks to all of you on CSG-net for making my days even more enjoyable. And especially to you, Bill. and Bob Clark, for coming up with Hierarchical Perceptual Control Theory (HPCT). I can't say how fortunate I feel.

Bruce
bn@bbn.com

Date: Wed Jan 22, 1992 7:37 am PST
Subject: synaesthesia

This seems to have a tantalizing connection with the "sentic" contours Manfred Clynes describes across sensory modalities.

Cross-posted from the linguist digest:

Linguist List: Vol-3-43. Thu 16 Jan 1992. Lines: 78

1)
Date: Mon, 13 Jan 92 09:23:34 -0800
From: slobin@cogsci.Berkeley.EDU (Dan I. Slobin)
Subject: Re: 3.24 Queries: Synaesthesia, Diachrony, WH-Questions

Synaesthesia:

A classic study is Brown, R. W., Leiter, R. A., & Hildum, D. C. (1957). Metaphors from music criticism. _Journal of Abnormal and Social Psychology_, 54: 347-52. They found agreement among musically naive listeners in assigning "non-auditory sense terms" to recordings of sopranos, tenors, and baritones (e.g. cool, dry, thick, chromium, closed, coarse). Roger Brown gives the following interpretation (_Words and Things_, 1958, pp. 148-9): "The principal sensory dimensions of the world are the same for men everywhere and are named in all languages. Though each of these dimensions is primarily associated with one receptor system its essential quality is inter-sensory. The quality is first detected in one sense modality and is named at that stage. Afterward the quality is detected in many other phenomena that register with other senses. The original name tends to be extended to any experience manifesting the criterial quality. And so it happens that unrelated languagesx extend their vocabularies of sensation in similar fashion. So it happens, too, that people in one language community can identify the basic inter-sensory qualities in operatic voices and exttend their vocabularies accordingly."

Dan Slobin (slobin@cogsci.berkeley.edu)

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Bruce
bn@bbn.com

Date: Wed Jan 22, 1992 7:40 am PST
Subject: Consulting and Art

[from Joel Judd]

Dag (920121)

I very much liked your post for reasons I wrote you about yesterday. With my background, after agreeing with someone the need for a PCT perspective is there, I would be in a position to offer pragmatic multilingual resources to go along with the theory.

Regarding education, you don't want to forget Hugh Petrie's book _Dilemma of Enquiry and Learning_.

Bill, Bruce (Bill's post 920121)

The repetition of some ideas regarding the language/perception interplay reminded of a 1st day activity I've used in English classes the last couple of times I've taught them. I started using it when trying to think of a way to help students not take language for granted as well as get out of the "dictionary mode" many of them get into in learning programs. Bill's metaphor of an "empty abyss" over which we often find ourselves when divorcing words from our experiences has also stuck in my mind.

Anyway I tell the students I want them to draw a simple picture of the _first_ thing that comes to mind when I say a word. Then I write 3-4 words on the board, one at a time. These usually include ones like HOUSE, BED, BREAD, etc. After they are all finished, I ask them to look at each others'. There's the inevitable snickering about lack of artistic talent, embarrassment. But I find differences in what they draw, and so I ask "Don't you all understand English? These are very simple words!" For bread, some draw a basic loaf with slices, others a long, rounded loaf ("French"), some a round ball; yesterday a middle-eastern woman drew a pita bread. I continued to complain, (looking at the pita bread) "That's not bread!" ("Yes it is!" she replied). Then I ask, "Why, if these words are so simple, is there so much variety in your drawings?" Very quickly comments start to refer to students' experiences, upbringing, culture etc.

So it becomes a good opportunity to point out how, with these and many other words, we share the same "language," but not the same experiences. We usually ACT AS IF we've had the same experiences, so when I go to a bakery, I don't have to ask for a rectangular shaped loaf made of flour, yeast, and water and rounded on top, I can just ask for some bread. But if I go into a store and get pitas when I ask for bread, I realize there's a difference between the baker and me.

Such an activity seems to have had a good effect especially on writing classes, where the students have some time to consider what they are going to say. At least they realize that just because they have learned the translation equivalent for some term they've used all their lives, others may not understand the same thing when they use it. And this activity is also good for exploding the myth of the "native speaker."

Date: Wed Jan 22, 1992 8:43 am PST
Subject: Re: BURN/UNBURN

[from Gary Cziko 920122] attn: Greg Williams; Bill Silvert

Greg Williams (920122) said:

>BURNing and UNBURNing automatically work with large files -- no
>concatenation or header-stripping needed -- and a copy of UNBURN can be
>obtained via e-mail (decode by using DEBUG program which comes with
>MS-DOS). So, for IBM users, this easier option will be available when
>Bill tests it. The source (ANSI C) is available for non-PCers.

As I understand it, part of the necessity for concatenating files is that mailers will often not send or receive large files without breaking them up into more manageable chunks. And ASCII-encoded program files tend to be on the big side. Therefore, for BURN to have maximum ease of use, it should work with fragmented files since this is what people may well receive by e-mail. So if an ASCII encoded ("Burned") file arrives split into files A, B, C and D, it would be nice if they could be "unburned" with a command statement something like UNBURN A B C D.

I am certainly in favor of making it as easy as possible for CSGnetters to obtain programs from the server. If Burn can do this, great. But then we will also need to convince Bill Silvert to port the program to his sever so that instead of saying UUENCODE DEM1A.EXE, we can say BURN DEM1A.EXE and get burned (burnt?) files sent over e-mail. Bill S., what do you think?

Finally, anybody with a direct connect to Internet would be silly to mess with any of this instead of learning how to ftp files. With ftp one can receive binary (program) files directly with neither fires nor fire extinguishers necessary at either end--Gary

Date: Wed Jan 22, 1992 10:15 am PST
Subject: Base forms; BURN works great

[From Bill Powers (920122.1000)]

Bruce Nevin (920122.0705) --

We're almost there.

You presented a "base form" that included "group consisting of a window". But if this is the true base form, there should be no other way of saying it. It can, however, apparently (by your definitions) be said

"countable stereotyped visual images at another level of detail perceived together"

So the "base" form actually contains expandable terms. When you expand those terms, you get a new sequence of words as the base form. But these words, too, need expansion: countable by someone? Stereotyped meaning an ideal form substituted for the actual form? Visual images that I, one, anyone sees? Another level of detail downward among many levels that exist? Perceived by me or someone and someone and someone ...? Together simultaneously or in the same vicinity one at a time (Like Bill and Mary in your example)? I can't see any end to this expansion process IF IT ALWAYS ENDS UP WITH STATEMENTS. There is no statement that has a self-evident or totally explicit meaning. This becomes an endless trip through the dictionary, looking up the meanings of the meanings of the meanings.... and ending up with nothing but more words.

How do we break out of this endless circle of words? Is there some principle you haven't mentioned such that any person who understands it will naturally come up with exactly the same base form that isn't expandable any further?

If there is more than one valid way to express the true base form of a sentence, then NONE of these ways is actually the base form. They all *refer to* the base form; they are paraphrases of some idea that is not verbal.

You say, "You can't hope to investigate the correlation of words with nonverbal perceptions until you know the base vocabulary." I can't accept that unless you can tell me the one and only expansion that any listener or speaker must construct for any cryptic sentence , and demonstrate that perceptual meanings can't be assigned directly to the "reduced" forms.

I agree that ambiguous sentences need to be made more explicit -- that's not the problem we're having here. The problem is how we tell that a sentence is ambiguous and what we do to eliminate the ambiguity. I say that we do so by converting it AS IS into perceptions, and realizing that something is amiss: we can convert it into several sets of perceptions that are sufficiently different to leave us with a strong sense of mismatch or uncertainty. When that occurs, and when we want to settle on just one perceptual meaning (this isn't always true -- consider poetry), and when the speaker does not elucidate, then we pick one meaning as most likely under the circumstances and (optionally) mutter the clarifying phrases to ourselves. The speaker says "I want to eat," and we think " ... and therefore everyone, meaning me, ought to go into the dining room is what she means." This expansion may or may not correspond to what the speaker intended, but it is the expansion we accept as operationally true, and act upon. We don't wait to see the hostess go into the dining room and start eating; we interpret the statement as an implied request, and proceed ourselves toward the dinner table.

The base forms that you've presented as examples so far haven't struck me as clarifications, but quite the opposite. I haven't any idea what a "group consisting of a window" is. How can a group consist of one thing? Seeing the whole expansion, with the connective "and"s, my brain doesn't come up with an array of windows, but with a list of groups, each consisting of one window, which, to my poor mind, is just a list of contradictions. This doesn't seem to me to accomplish what a base form is supposed to accomplish: narrow the meanings of the communication to as few alternatives as possible.

I agree that speakers often emit ambiguous sentences. They fail to perceive in what they speak the ambiguities that a listener might find there, because among the meanings the speaking evokes are the ones they intend. "The shooting of the hunters awakened me" is perfectly clear to the person who says it. There's no memory of carnage in the speaker's mind for this sentence to mean. It doesn't occur to him/her to eliminate the unwanted meaning by further specifications or by substituting a better sentence. Learning to communicate involves not only finding ways to evoke nonverbal perceptions, but being alert to unwanted meanings and insufficiently specified meanings and trying to correct them. This, and not picky grammar, is why one should not say "I only want one apple" when what is meant is "I want only one apple" or "I want one (piece of fruit), but only an apple." That is, unless you mean "I only *want* one apple; I don't expect to *get* one apple."

When people try to eliminate ambiguities, I don't hear them using anything like the base forms you describe. How do you explain that?

Greg Williams (920122.0538) --

Kwicherbichin. I'm going as hurry as I can. BURN works fine. I recommend adding an optional command-line specification for the destination directory and name (less .ash). When I BURNed a file from \tc\obj the BURNed version ended up in tc\obj, not where I was. And I didn't want it where I was, but in \bitnet. Also, for testing purposes it's nice to be able to BURN a file, UNBURN it, and not wipe out the original file.

Recommended. Best Buy.

That's some clever program Pat wrote. I understand everything up to where the table of letter usage frequency is created and then sorted on frequency. How the compression happens totally eludes me. But it works. Such a smart woman you married. SHE doesn't bug me to get busy.

Best to all,

Bill P.

Date: Wed Jan 22, 1992 10:30 am PST
Subject: re Base forms

[From: Bruce Nevin (Wed 920122 12:34:15)]

Bill, we are intending two different things by the words "base form".

Everyplace I say "base form" substitute "unreduced linguistic form" or "operator and its arguments before reduction".

Everyplace you say "base form" say "nonverbal correlates of words". Or say "base form" if that really represents in words what you intend. You can have that phrase free. But I doubt it does.

Can't say more now. Try what I said before with that distinction in mind.

Bruce
bn@bbn.com

Date: Wed Jan 22, 1992 11:50 am PST
Subject: correction

from Ed Ford (920122.noon)

Dag: Please use my address below. Since I'm a part-time teacher at ASU, my classes are not guaranteed. In fact, last fall my course was upgraded to a 600 level, with several requirements attached. Result, instead of the usual 20 or so signing up, as they have been doing over the past 6 years, only one signed up for this semester. The course was cancelled. It is truly wonderful being on the outside looking in. I'm

meeting with the Direct Practice Committee at the university's School
Of Social Work on Friday.

To All: Any comments to my post on 920121?

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

Date: Wed Jan 22, 1992 4:22 pm PST
Subject: Re: walls of windows

[Martin Taylor 920122 1800]
(Bruce Nevin 920121 0704)
>
>   a group consists of a window and a group consists of a window
>   and a group consists of a window . . .
>   a group consists of a window and a window and a window . . .
>   a group consists of windows
>   a group is of windows
>
The bottom 3 seem OK, but should there be some mechanism by which "consists"
is expanded to "has as a part" or some such. Each of the "a group consists
of a window" is false. This ought to come out as a natural expansion.
How would the top line come out to be something like:

"a group has as a part a window and said group has as a part a window
that is not the said window and said group has as a part a window which
is not the first said window and is not the second said window and ...."

Something of this kind must be happening, but it leads to a problem, in that
the perception is of a plural (more than 2 or 3, not linguistic plural) set
of windows, not of a window and a window... to exactly N windows.
The extended linguistic form loses the linkage with that percept.
The second of your forms has the same problem.

Martin

Date: Wed Jan 22, 1992 4:32 pm PST
Subject: Re: Sequence detection

[Martin Taylor 920122 18:15]
(Bill Powers 920121.1100)
>
>Martin and Joe, the kind of perceptron you get depends on the assumed
>elementary computation that takes place, doesn't it? If the elementary
>computation is weighted summation, I think you get sensation-type
>outputs. Perhaps a different kind of elementary computation would be
>needed to derive true configuration signals for form recognition. And if
>the basic computation is latching, as above, you get sequence detection.
>
Yes, I assume that the different layers will have different kinds of perceptual
functions. In local discussions here, I have talked about perceptual
functions such as "If input 1 > input 3 then output input 2 - input 4, else
output input 2". But a simple MLP can do a lot without that.

The type of sequence detector that you suggest, with its branchings, is quite reasonable for dealing with patterns that are reasonably regular, and in which the individual elements are identifiable. It has been developed into a functioning neural net system by Dominic Beroule, at LIMSI near Paris, and it works quite well, even allowing the detection of overlapping patterns. But if the inputs are uncertain, as they are in most speech, for example, then there is a lot of difficulty. Suppose the sequence detector is looking for "r oo l" (Rule). Was that first sound "r", or was it the end of something preceding it. Was there a "y" after it, before the "oo" or was that "o" as in role? And so forth. It is by no means easy, but something of the kind may be developed that can accommodate the problems.

Martin

Date: Wed Jan 22, 1992 4:37 pm PST
Subject: latching & phoneme sequences

For linguistic sequence detection, maybe the next issue to sort out is `phonotactics'. Which is the linguistic jargon term for the fact that all languages have strong generalizations about what kinds of sounds can occur where. For example, it is often required that no more than two consonants appear in a row, and there are often strong constraints on such sequences. In Japanese, for example, they must be either a double-length stop (nippon) or a nasal followed by a cons. with the same place of articulation (sensei, simbun, but not simte). These rules (which vary from language to language, though there are lots of strong cross-linguistic tendencies), are closely involved with syllable structure.

I suspect that it will require an actual linguist to work out how to adapt the latching idea to this stuff, but still, people ought to know that this problem is out there.

Avery Andrews

Date: Wed Jan 22, 1992 9:30 pm PST
Subject: Sentics; BURN; language

[From Bill Powers (920122.2030)]

Bruce Nevin (920122.0818) --

Re: synaesthesia and sentics --

I learned of Manfred Clynes' work a very long time ago -- possibly even during the years that Bob Clark and I were working together, if that's possible.

One hypothesis is suggested by the principle that all neural signals are alike. This won't necessarily work for every example, but with a little guessing about the correspondence between neural frequencies and perceived variables ...

When we hear sounds, a low pitch is represented by a low-frequency neural signal, a high pitch by a high-frequency signal. Also, a soft sound is represented (at a lower level) by a low-frequency signal and a loud sound by a high-frequency signal. So a pattern of sound going from soft to loud produces neural signals with ascending frequencies similar to the ascending neural frequencies produced by a sound going from low pitch to higher pitch. Likewise, events occurring at a low rate yield a rate signal that is low in frequency, events repeating rapidly yield a large (high-frequency) rate signal. So there is a common feature in all these neural signals that could possibly be detected and perceived at a higher level, such as the relationship level.

The terms "high" and "low" suggest how vertical position in the visual field is encoded as a frequency and how arm position in the vertical direction is encoded kinesthetically: low frequency = down, high frequency = up. Clynes, in fact, had people draw sentic forms as graphs of vertical position against time, combining the visual and kinesthetic definitions of "up" and "down." These, in turn, were correlated with sound pitches and loudnesses. A descending pitch goes with a spatial and kinesthetic transition in the downward direction. A decreasing loudness, likewise.

Also the sentic forms were associated with ideas like "gentle" and "angry." People showed astonishing agreement among the sentic forms they drew (independently) for such ideas. This suggests that the experiences being named entail similar behaviors of the envelopes of neural signals -- patterns that appear similar to higher-level perceptual systems.

I don't know what to make of all this. I suppose that sentic forms are telling us something about the sorts of variables that higher-level perceptual functions are concerned with. Darned if I know what.

Joel Judd (920122.0853) --

Nice illustrations of separating words from meanings in your classes. Your students are lucky; they're learning the difference between writing and communicating.

Gary Cziko (920122.0930) and Greg & Pat Williams --

Re: BURN

Good point, Gary -- Perhaps Pat could install a switch that would allow outputing BURNed files in 20K chunks (a little smaller than the chunks my uuencode automatically produces). The first chunk would contain info about how many chunks, and each chunk could start with an ID code that says it's another chunk.

Pat's programs are written in C. There's nothing fancy except use of a Qsort library routine for sorting, so perhaps these source codes are ANSI compatible (are they, Pat?). And of course file name conventions and directories could differ, but that's no big problem. If so, the source could be compiled to run on different mainframes, and non-PC users would be able to BURN and UNBURN files on the mainframes. The syntax Gary suggests would be an alternative. Anything to avoid concatenating files, which can entail removing garbage characters between them.

Bruce Nevin (920122.1117) --

>Bill, we are intending two different things by the words "base form".
>Everyplace I say "base form" substitute "unreduced linguistic form" ...

Ok. I'm still asking how you get from the reduced form to the unreduced form without first going to the perceptual meanings. The point I'm trying to make is utterly simple, yet so subtle that one can look right at it for years without seeing it (as Bob and I did).

The same problem is overlooked by Martin Taylor (920122.1616). He accepts

>   a group consists of a window and a window and a window . . .

as an expansion of "windows." But how do you get from "windows", which is what you hear, to "a group ..."? There's a hidden, unspoken, taken-for-granted process in there. I'm trying to call attention to that process, not to its outcome.

I'm suggesting that the hidden process is a direct nonverbal perception of the pluralness of "windows," indicated by the terminal "s", and that this sense of pluralness indicates a particular type of nonverbal perception of multiplicity -- which I think may be an aspect of the category level (although any other hypothesis is welcome and the correct identification of the level is immaterial here).

Martin inadvertently refers to nonverbal meanings when he says

> ... there be some mechanism by which "consists" is expanded to "has as
> a part" or some such.

By saying "or some such" Martin is indicating that there may be many phrases that would mean the same thing. This means that no one of those phrases is mandatory; ANY PHRASE HAVING THE SAME MEANING WILL DO. This fact points outside the linguistic universe of discourse directly to the world of nonverbal perception. "A group" points to the same kind of perception that the added "-s" points to. There is no direct connection within the world of words between the "-s" and "group." Likewise, "consists of" points to a kind of perception -- again probably category perception -- that would also be pointed to by "has as a part". The "mechanism" that Martin suspects is not a verbal mechanism: it is nonverbal perception.

This mysterious "sense of pluralness" is not basically mysterious. It's an easily observed empirical fact. Don't forget how this all began. Avery Andrews described a building with a wall of windows. Later he illustrated the same thing (with a twist) using just a string of letters: TTTTTTTTTTTTTTTTTTTTXTTTTT. Putting all words aside, and just looking at the string of characters, one can immediately perceive the multiplicity of the Ts. This is a set of configurations, each individually recognizeable as a familiar object, and ALL ALIKE (except one). The "all alikeness" that is perceived is the sense of configuration. But in addition to recognizing the configuration, we get the immediate impression that there are MANY of that configuration. That manyness is the sense of multiplicity to which I refer. It is not necessary to say to oneself, "A T and a T and a T and a T .." to perceive this multiplicity. However, having perceived the multiplicity, one can then _describe_ it by creating another multiplicity:" A T and a T and a T and a T ...". To recognize this string of characters as containing a multiplicity, one has to use the SAME perceptual system that was used to apprehend the manyness of the Ts in the first place. Then, and only then, one perceives that the string of characters contains a "multiplicity" of "a T and"s (or however one perceptually groups the words in the "expansion"). This, however, can create the false impression of a direct relationship between "Ts" and "a T and a T and a T ..."

So the linguistic expansion simply begs the question of how we perceive manyness in EITHER the perceptions themselves or in an "unreduced linguistic form" that ALSO shows the same apparent characteristic. One needs the same kind of perceptual function to see

```
     _    _    _
    | |  | |  | | ...
     -    -    -
```

and say it is the meaning of "windows" as one needs to hear

"a window and a window and a window and a window ..."

and say it is the same as "windows." In the one case we perceive the manyness of the window figure; in the other we perceive the manyness of the phrases. The sense of manyness is neither the window configuration nor a phrase. It is the result of a higher level of perception applied to both. It is a nonverbal perception.

By use of a terminal -s we can refer to the nonverbal sense of multiplicity directly and economically. We can also create an analogous and much more wordy phrase that procudes the same sense of multiplicity. The connection between a configuration name with "s" appended and the "expansion" is not between one linguistic form and another linguistic form: it is between either form and the underlying perception. We are simply paraphrasing a meaning.

If this is true, then characterizing the relationship between "windows" and "a window and a window and a window" as "expansion" or "reduction" is erroneous. What is erroneous is the assumption that the one can be obtained from the other by some sort of strictly linguistic procedure, without crossing the border into semantics.

Date: Wed Jan 22, 1992 11:23 pm PST
From: Dag Forssell / MCI ID: 474-2580

TO: csg   (Ems)
          EMS: INTERNET / MCI ID: 376-5414
          MBX: CSG-L@VMD.CSO.UIUC.EDU
Subject: Promoting PCT
Message-Id: 45920123072354/0004742580NA3EM

From Dag Forssell [920122]

Promoting PCT, continued:

How is PCT different?   Take three:

We all know people are different, right?

Most programs and books teach you to deal with many different kinds

of difficult people in many different ways. That makes it difficult
to know what rules to follow when.

PCT shows you in what ways people are similar.

That makes things a lot easier!

Joel Judd (920122):

Thank you for suggesting Hugh Petrie's book. I realize that I don't
know anything about it. There are of course other books by our
teaching friends. I am interested in knowing how they relate to
PCT. The ones I listed are the ones I am familiar with. In the end,
Ks?recommended literature.

I will not send you my brochures unless you send me your snail mail
address. Second request. I am trying to influence you, ok?

Dag

Date: Thu Jan 23, 1992 8:13 am PST
Subject: Re: Sentics; BURN; language

[Martin Taylor 920123 10:00]
(Bill Powers920122.2030)
>
>The same problem is overlooked by Martin Taylor (920122.1616). He accepts
>
>>   a group consists of a window and a window and a window . . .
>
>as an expansion of "windows." But how do you get from "windows", which is
>what you hear, to "a group ..."? There's a hidden, unspoken, taken-for-
>granted process in there. I'm trying to call attention to that process,
>not to its outcome.
>
>I'm suggesting that the hidden process is a direct nonverbal perception
>of the pluralness of "windows," indicated by the terminal "s", and that
>this sense of pluralness indicates a particular type of nonverbal
>perception of multiplicity -- which I think may be an aspect of the
>category level (although any other hypothesis is welcome and the correct
>identification of the level is immaterial here).
>
>Martin inadvertently refers to nonverbal meanings when he says
>
>> ... there be some mechanism by which "consists" is expanded to "has as
>> a part" or some such.
>
>By saying "or some such" Martin is indicating that there may be many
>phrases that would mean the same thing. This means that no one of those
>phrases is mandatory; ANY PHRASE HAVING THE SAME MEANING WILL DO. This
>fact points outside the linguistic universe of discourse directly to the
>world of nonverbal perception. "A group" points to the same kind of
>perception that the added "-s" points to. There is no direct connection
>within the world of words between the "-s" and "group." Likewise,
>"consists of" points to a kind of perception -- again probably category
>perception -- that would also be pointed to by "has as a part". The

>"mechanism" that Martin suspects is not a verbal mechanism: it is
>nonverbal perception.
>

You put into words something I thought went without saying. What interests me is Bruce's contention that there is in the linguistic structure itself the cue to these expansions. I would like to discover these cues, since they can be developed more easily into computer-based communicating systems than can the perceptual non-linguistic mechanisms that I believe dominate the human interpretation of language. Putting aside those mechanisms, what in the language itself guides the expansions, and how far can they go without requiring knowledge of all the world? Bruce has been doing a good job of showing that there is a lot more to be found in the actual structure than is immediately evident, and it may well support and in some cases control the non-linguistic stuff that (you and I claim) normally dominates.

The existence of ambiguity does not, in itself, point outside the linguistic domain.

Martin

Date: Thu Jan 23, 1992 2:26 pm PST
Subject: Code Generation Problems

Hi, Control engineers,

We're having a problem on the development of our research.

We work with implementation of control algorithms for industrial process control. These algorithms are implemented on a single-board 8088 based compact dedicated microcomputer, which don't use DOS nor BIOS operational systems.
Our problem is to find a high-level language compiler which works on DOS enviroment and generates a code machine without DOS nor BIOS calls.
It will be utilized to simulate the control on PC and to generate the code for the dedicated computer for the real control of the processes.
This compiler should have floating-point libraries for execution of such operations on the board. Still, the language should offer some facilities as type declarations and pointer manipulation, because the algorithms implemented are somewhat complex, utilizing matrix calculations.

We hope any help,


```
########################################################################
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########################################################################
```

Date: Thu Jan 23, 1992 6:46 pm PST
Subject: 8088 Programming

I' not a guru on such matters, but I'd expect that any reasonable C system could be used for high-level programming without DOS or BIOS. e.g. Turbo C or Borland C++. These systems will let you mix C and assembly code, though you need to get the assembler separately.

Avery Andrews

Date: Fri Jan 24, 1992 4:25 am PST
Subject: Language Analysis by Synthesis

[from Gary Cziko 920124.0600]

I have been following the linguistics discussion with more than casual interest and less than complete understanding ever since Bruce Nevin joined CSGnet very many words ago. But in reading the last few posts by Powers, Taylor and Nevin I feel that some light is finally starting to shine where there was little before. I may regret jumping in on this now (there is no way I can keep up with you guys in either rate of thinking or typing), but here goes anyway.

Powers (920122.2030) asks:

>Ok. I'm still asking how you get from the reduced form to the unreduced
>form without first going to the perceptual meanings. The point I'm trying
>to make is utterly simple, yet so subtle that one can look right at it
>for years without seeing it (as Bob and I did).

I am going to assume first that Bill is asking how the listener gets from the reduced form (which he or she hears or reads) to the unreduced form which points to the perceptual base of the sentence and thus the intended meaning of the speaker. Perhaps the answer is that the listener does not need to go from the reduced form to the unreduced form because he or she HAS ALREADY BEEN THERE. I am proposing that we seriously consider an analysis-by-synthesis (hypothetico-deductive; Neisserian; Popperian) model by which the listener (a) starts with trying to figure out what perception(s) the speaker may be trying to communicate are (i.e., makes a conjecture concerning the unreduced form), (b) uses this to create the reduced form (mentally), (c) tries to match his or her reduced form with that of the speaker, and (d) goes back to (a) if there is no match. Can it be this simple? Or have I really misunderstood what this discussion is all about here?

This could also account for the misunderstanding that can arise (particularly across cultures) even when the reduced form is agreed upon. I have one perception-unreduced form which maps onto a reduced form; you have a DIFFERENT perception-unreduced form which maps onto the SAME reduced form. So we can talk to each other until we are blue in the face and still not understand each other (isn't this what Deborah Tannen's book _You Just

Don't Understand_ is all about--male-female problems of this type? I'm not sure, I never read much of it).

Does any of this bring us closer to answering Bill's question?--Gary

----

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Date: Fri Jan 24, 1992 8:39 am PST
Subject: PCT in orbit

[Martin Taylor 920124 10:30]

Canada's first female astronaut is at present in orbit, and our papers are making big news out of it, describing the experiments in which she has a part. The last paragraph of today's report in the Toronto Globe and Mail should be of interest to PCT people, because of the fourth word:

"One early and surprising result of Dr. Watt's experiments was that it appears muscular changes that on Earth keep the human body in a stable, upright position increase rather than decrease in weightlessness."

Is there anyone reading this who would have used the word "surprising" rather than "expected"?

Martin

Date: Fri Jan 24, 1992 9:38 am PST
Subject: Perception and Word Meaning

[from Gary Cziko 920124.1015]

As a follow-up to my "language analysis by synthesis" post of earlier today, I offer a quote from Don Campbell.

While Campbell refers only to the meaning of single words here, I would hope that this type of analysis could be applied to the syntactic and discourse aspects of language as well. Notice the crucial role of perception in Campbell's view of the evolution of word meanings and the Darwinian/Popperian flavor of it all (the kinda stuff I like so much, so be careful if you feel inclined to criticize!).

==

"Moving to the evolution of human language, a social trial and error of meanings and their namings can be envisaged. Trial words designating referents which the other speakers in the community rarely guess "correctly" either fail to become common coinage or are vulgarized toward commonly guessed designations. All words have to go through the teaching sieve, have to be usefully if incompletely communicable by finite sets of ostensive instances. Stable, sharp, striking object-boundaries useful in manipulating the environment have a greater likelihood of utilization in world meanings than do subtler designations, and when used, achieve a greater universality of meaning within the community of speakers. Such natural boundaries for words exist in much greater number than are actually used, and alternative boundaries for highly overlapping concepts abound. Just as certain knowledge is never achieved in science, so certain equivalence of word meanings is never achieved in the iterative trial and error of meanings in language learning. This equivocality and heterogeneity of meanings is more than trivial logical technicality; it is a practical fringe imperfection. And even were mearnings uniform, the word-to-object equivalence is a corrigible contingent relationship, a product of a trial and error of metaphors of greater and greater appropriateness, but never complete perfection, never a formal nor entailed isomorphism (p. 433-434)."

Campbell, Donald T. (1974). Evolutionary epistemology. In Paul A. Schilpp (Ed.), _The philosophy of Karl Popper_ (vol. 1; pp. 413-463). La Salle, IL: Open Court.

Gary

Date: Fri Jan 24, 1992 9:44 am PST
Subject: BURN/UNBURN

From Pat Williams (920124)

I would be happy to add the suggested additions to BURN and UNBURN (i.e., improved file handling and breaking up/reassembling files) IF Bill Silvert is willing to recompile the programs for his system -- which might involve a few changes in the source. Bill, do you have time to deal with this sort of thing? If not, clunky uu... remains the way to go.

Joe Lubin, I'd be happy to distribute the source when it is finalized. Maybe you could aid Bill S. in compiling a UNIX version he can use?

Pat
phone 606-332-7606

Date: Fri Jan 24, 1992 10:53 am PST
Subject: Reduced forms; mystery request

[From Bill Powers (920123.2200)]

Martin Taylor (920123.0823) --

You intrigue me:

>You put into words something I thought went without saying.

HCT was built amost entirely by saying things that others assumed went without saying.

As you quoted several paragraphs of my post, I am unsure which part you considered to go without saying. I will guess that it is this part:

>>By saying "or some such" Martin is indicating that there may be many
>>phrases that would mean the same thing. This means that no one of those
>>phrases is mandatory; ANY PHRASE HAVING THE SAME MEANING WILL DO.

The reference was to expanding "consists of" into "having as a part" (and as you said, "or some such"). My question remains the same: what connects "having as a part" to "consists of?" I am trying to draw attention to something that both of these terms point to: the imagined perception that gives either one meaning.

If there were a mechanism or rule that makes "has as a part" a linguistic expansion of "consists of," then both input to and output from this rule would be nothing but words without meanings. Meanings in a purely linguistic analysis supposedly don't enter the argument. As a result, there could be only one output, given the input. There would be no need to check the perceptual validity of the output, because linguistic rules are assumed to be independent of meanings.

But in fact any number of verbal expressions will serve as the output, which fact is enough to show that the underlying process is not purely linguistic. All outputs THAT HAVE THE SAME MEANING are valid. Therefore the criterion for a valid expansion is not in any of the verbal phrases used, or in any transformation rule that converts one set of words into another, but in the constraint that all valid expressions must evoke in the hearer the same, or sufficiently the same, nonverbal meaning. I would include in this such evocations as are provided by words like "operator" or "argument."

I understand your hope for a programmable algorithm that will allow machines to understand human language without nonverbal experience. I don't think this is possible: a machine like those we know now will never know the circumstances under which saying "I feel sick" is appropriate. However, it may be that machines could accumulate their own nonverbal experiences of the kinds of perceptions they CAN have (at each layer of protocol) and build a linguistic/semantic structure on that. It would not be a human structure, but communication might still be possible in areas of experience that overlap with the human world.

Cary Cziko (920124.0600) --

>I am proposing that we seriously consider an analysis-by-synthesis
>(hypothetico-deductive; Neisserian; Popperian) model by which the
>listener (a) starts with trying to figure out what perception(s) the
>speaker may be trying to communicate are (i.e., makes a conjecture
>concerning the unreduced form), (b) uses this to create the reduced form
>(mentally), (c) tries to match his or her reduced form with that of the
>speaker, and (d) goes back to (a) if there is no match. Can it be this
>simple?

I think it can, and this is essentially what I propose. I see the steps this way.

1. The unreduced form is heard/read.

2. The noun-like words evoke the basic elements of the intended experience; the arrangement of words in sequence and special-purpose words (like "in") bring in higher-order perceptions, which the receiver uses to select appropriate ways of arranging the basic elements (now existing as imagined perceptions of the referents of the words). Thus "man" evokes a stick man, "dog" evokes a stick dog, "bites" evokes fangs snapping shut on something, and the arrangement "dog bites man" tells us to arrange the stick dog so its fangs are embedded somewhere in the stick man (anywhere we please, as the place was not specified).

3. We thus perceive an imagined situation that is a valid meaning of "man bites dog." It isn't the only image that would fit the words, but it is sufficient to match the description.

4. If the sentence goes on, "... in the foyer," we experience a little jolt at the final word because we have already (that rapidly) removed the fangs from the part of the anatomy we had imagined and are ready to attach them to the part following "in the." If we don't happen to know what a "foyer" is, we're left with an incomplete meaning. If we want to complete it we have to ask the speaker what "foyer" means, or look it up in a dictionary. In either case we expect a new set of words not involving "foyer" that will evoke either a part of the anatomy or a description of a spatial location in which the biting takes place, giving the missing directions on how to manipulate the perceptions.

5. All sentences are ambiguous, in that they leave unspecified a large number of details that we supply in imagination. We supply as many of them as it takes to satisfy us that we understand the communication. Where the line is drawn is up to the listener. Commonly, people are satisfied that they understand only because they have not realized how many crucial elements they have imagined without any element of the sentence to back them up. Also, they reach spurious understanding because of limited experience -- not realizing that other people might attach different images to the same words and still make sense of the sentence.

"Making sense", in CT, can be taken literally.

I am beginning to suspect that the "unreduced form" is really a description that includes the parts of a meaning (or possible meanings) that one must imagine in order to make sense of the sentence as given. There is no unique unreduced form because there are many ways to evoke the missing elements of experience; any one of them will do. My assertion is that constructing the unreduced form relies heavily on looking at an imagined picture of what a sentence might mean, selecting one perceptual situation as the most likely or appropriate meaning, and then describing the full situation in more detail. Perhaps linguistic methods provide an algorithm for doing this in an efficient way, or for reminding us of ways in which cryptic sentences can be incomplete in their meanings.

Of course if Bruce now tells us that for any reduced sentence, there is one and only one valid expansion into an unreduced form (that is, only one sequence of specific words that will result), my hypothesis about linguistics takes a dive.

Paulo Eduardo Maciel de Almeida (920123.1324)

I think you have the wrong network conference, and will probably not receive this because it isn't sent as a reply. At any rate, Avery Andrews is right. You can compile a program under DOS that doesn't use any DOS commands. Keyboard input can be taken directly from serial ports, and output can be generated the same way. Reading and writing disk information is much more complicated, but a controller probably wouldn't need it -- it could send its information by a serial port to a central machine with a disk operating system. You simply have to know how to program using direct hardware commands. High-level languages like C or Pascal can provide direct access to input-output ports on the target machine. Of course the different machine must contain sufficient programming to accept machine codes, store them in memory, and transfer control to the stored program.

Best to all,

Bill P.

Date: Fri Jan 24, 1992 12:25 pm PST
Subject: help with imagination

[From Rick Marken (920124)]

Ed Ford (920121.09:30) says:

>   I am still confused as to what part the imagination plays in
>the perception signal when one is a) trying to recall something in the
>past or even recent present and b) trying to imagine what one is going
>to do in the future.

Ordinarily, the perceptions you control have their origins at the sensory surface of your nervous system. Lift a pencil to some point above the table in front of you; you are controlling a relationship perception (the "above the table" perception) -- and this perception is made up of configurations (the "pencil" configuration is above the "table" configuration) and these configurations are made up of sensations of varying intensity. Ultimately, the perceptions are caused by something outside you (the real world) and you influence those perceptions via your effects on that world.

Now close you eyes (if you like) and imagine the pencil being lifted. You are having very similar perceptions to the ones you had before, right? But the cause of these perceptions is not outside you any more; this is especially obvious if your eyes are closed. Also, you are not "doing" anything to make the imagined perception of the pencil rise up (although you might "see" you arm doing the lifting in your imagined perception). According to the control model, what you are doing when you imagined the pencil being lifted is very similar to what you did when you actually lifted it. In both cases there was a reference for the perception "pencil avove the table". In the first case (non-imagination) the difference between this reference and what you were seeing led to an error signal that led to lower order outputs (muscle tensions, etc) that had effects in the real world such that what you were perceiving became "pencil above table". In the second case (imagination mode) what you were "seeing" was not coming from the outside -- but from the outputs of the very system that is trying to perceive "pencil above table". Since there are no disturbances (since there is no influence of the real world on perception) the system perceives EXACTLY what it intends to perceive. That is, the imagined perception is brought exactly to the reference level ("pencil above table").

So imagination is just the same as control -- without going through the "outside world" part of the control loop. There is still a loop but it is "short circuited"; output connects directly to input. One very interesting implication of this model is that you cannot do this short circuiting at the very lowest level of the hierarchy -- that is, you cannot connect the intensity sensors (like the pressure receptors in the skin) directly to outputs that influence the input to these detectors -- because these detectors are physically "detached" from these outputs; there is no neural connection between them. This suggests that it would be impossible to imagine control of intensity perceptions. I think this has some subjective validity; I can imagine controlling heat (a sensation) but the actual intensity is a bit fuzzy. I think this lack ability to imagine at the lowest level of perception (intensity) is what subjectively differentiates an imagined perception (which is good) from the real thing (which is really THERE). Imagined perceptions lack "intensity" because you cannot imagine at that level -- ordinarily. Perhaps "hallucinations" are cases where the "lowest level" loops get short circuited. I have never experienced this kind of halluciation so I have to trust the reports of others who claim to have actually, say, heard voices, when there was no one talking. But I suspect that in these cases, if there was actualy an intensity component to the imagined perception, there was some external basis for it (like noise in the room).

When you imagine something that has happened before you call it "recall". When you imagine how something might be in the future, you call it "planning". But both involve the same process. You are controlling imagined perceptions (self-produced -- by playing back you own outputs), making them match reference signals (as usual).

The current discussion of language understanding (as a process of creating perceptions based on the words and sentences you hear -- analysis by synthesis) suggests that imagination is probably a big part of understanding language. The sentence gets translated into references for imagined perceptions "on the fly". What we understand, then, probably depends on how our hierarchy produces perceptions AND references for lower order perceptions (since several levels are imagining at the same time when we hear language). Maybe there is a way to get at the study of imagination through the study of language.

Imagination is sure fun -- but it is not clear whether it helps or hinders real control; probably both depending on the circumstances. Athletes (the high jumper Dwight Stones comes to mind) often do imagined control before actually performing an event; it seemed to help me when I imagined gymnastics routines before performing them -- LONG AGO. But imagination can also conceal the flaws in the best laid plans -- as in the example of your daughter's plans to change careers. Maltz's book on "Psychocybernetics" was all about the virtues of careful imagining of intended results before trying to produce those results. PCT may have some useful things to say about imagination once we get down to doing some research and modeling in this area.

Hasta Luego

Rick

Date: Fri Jan 24, 1992 12:32 pm PST
Subject: Re: Reduced forms; mystery request

[Martin Taylor 920124 14:00]
(Bill Powers 920123.2200)
>
>
>As you quoted several paragraphs of my post, I am unsure which part you
>considered to go without saying. I will guess that it is this part:
>
>>>By saying "or some such" Martin is indicating that there may be many
>>>phrases that would mean the same thing. This means that no one of those
>>>phrases is mandatory; ANY PHRASE HAVING THE SAME MEANING WILL DO.
>
Actually, what I meant was not the bit about ambiguity, but the whole background of your posting, of which the long quoted part gave a flavour. What went without saying is that the main way of developing meaning from language is through non-verbal perception. And what I think Bruce is doing is showing that aspects which at first seem accessible only through the non-verbal route may also be signalled in the linguistic form. So it went without saying that the kinds of approach you discussed would be effective.

As for ambiguity forcing the use of non-verbal means to determine whether two representations mean the same thing, this again is an open question. Certainly you can use non-verbal equivalence to determine whether the linguistic forms are synonymous, but it is interesting to know whether the potential existence of ambiguity is signalled by the linguistic form, and if so, whether the converse, the possible synonymous relation between two utterances is also signalled. It might be, if the two forms when "unreduced" came out to be the same thing.

So: is the expansion process deterministic? If so, do utterances that are synonymous converge when they are expanded? If not, do synonymous utterances have common permitted expansions from their sets of possible expansions?

Whichever answers come from the foregoing, are they generalizable, or applicable to specific utterance forms? The non-linguistic way of finding the meaning of an utterance is always available. What does the linguistic way buy? I think it buys precision when the non-linguistic approach is vague, and if usable rules can be adduced, it reduces the requirement on a computer for real-world knowledge (but it doesn't by any means eliminate that need), and for parallel computation.

Martin

Date: Fri Jan 24, 1992 1:15 pm PST
Subject: Language and perception

> >
from Joel Walters:

Cziko (920123) states in his query of Powers (920123):
> >> I am going to assume first that Bill is asking how the listener gets from
> >> the reduced form (which he or she hears or reads) to the unreduced form
> >> which points to the perceptual base of the sentence and thus the intended

> >> meaning of the speaker.   not
> >
Are perceptions the same as intentions? I remember an earlier comment of Powers to Nevins that "words are perceptions." (did I remember right?) To get from perceptions to intentions, then, wouldn't you hav   e to go through the full control loop? Can someone unpack this for me in all its detail?

Joel Walters
Bar-Ilan University
inj3@musicb.mcgill.ca

Date: Fri Jan 24, 1992 2:00 pm PST
Subject: talk about perception corr. w/ words

[From: Bruce Nevin (Fri 920124 14:09:18)]

Just got connected up, a brief followup note in the midst of unpacking.

"Consists of" I agree causes problems, "contains" works better. Please be clear that the error is mine, not a defect in the theory. I didn't look up the reductions someplace, I put together quickly what seemed appropriate.

Bill: circularity arises because "I gotta use words to talk to you." You asked for the perceptions to go with the proposed base vocabulary items. But I could not give you the perceptions as I experienced them. I could only describe those perceptions with still other words. Hence re-entry into language at a different point than the unreduced forms.

The perceptions "group" and "window" and repetition under "and" (reducing to plural) and so on also correlate with the reduced forms.

```
        a group consists of a window and a group contains a window
          and a group contains a window . . .
        a group contains a window and a window and a window . . .
        a group contains windows
        a group is of windows

        A group--said group is of windows--is visible
        A group which is of windows is visible
        A group of windows is visible


Group                   perception: "group of countable things"
a                       [it's countable]
window                  perception: stereotyped visual image:        +-+
                        rectangular frame, clear panes top and       +-+
                        bottom.                                      +-+
and                     perception: I've got more to say
                        perception: repetition of "window"
contains                perception: focus attention on detail
                          of "group" perception
```

Leaving some things out, as you did, the same perceptions are associated with the reduced forms in the phrase "a group of windows":

```
                      / perception: "group of countable things"
                     |  perception: stereotyped visual image:        +-+
                     |    rectangular frame, clear panes top and     +-+
a group of windows  <     bottom.                                    +-+
                     |  perception: more to come
                     |  perception: repetition of "window"
                     |  perception: focus attention on detail
                      \   of "group" perception
```

Please don't identify the words on the right-hand side with the perceptions that they attempt to sketch. The perceptions are much richer and much more capable of variation around a center or type or norm.

I suspect that categorial perception is not of an abstraction but rather of a representative taken to be typical, with capacity to analogize and tolerate exceptions in detail.

I haven't had time yet to read more than a little mail, but these thoughts occurred to me yesterday as needing articulation with my earlier brief response.

Bruce
bn@bbn.com

Date: Fri Jan 24, 1992 3:00 pm PST
Subject: Adaptive Control

[from Gary Cziko 920124.1430, again]

OK, Mr. Powers, you got me into this. Now can you get me out?

Just came back from a lecture by Gideon Inbar here on sabbatical from the Technicon Israel Institute of Technology. His title was "Adaptive Control Model for Human Movement Control." This was in kinesiology (which I can hardly say, what happened to physical education?) David Goldfarb should've been there to help me but he wasn't.

He is a former electrical engineer. His model of just single joint movement, as far as I could make out, was a one-level adaptive control model. The system has a certain fancy equation as its reference level (reference equation) and the system compares this to what is actually happening and makes necessary changes (adaptations; I guess we call it reorganization). It seems that he is controlling the motor impulses to the muscles. I couldn't find the loop going outside the system to the environment. No mention of controlled variables.

He said that servomechanistic control is no good. The loop gain of the basic muscle system through the spinal cord is around one which can't do much. We need constant adaptation. We function under a wide range of dynamic loads. One control system no good. Walking on land not like walking on water. We need an adaptive controller condition the signal to get the desired output.

I talked to him briefly after. I mention Arm demo with gravity off and on which makes no difference; no adaptive control needed. He says: fine if the loop gain is high enough; but that is not how we work; with high loop gain you put your hand through your desk if you think drawer is open but is not. I mention hierchical organization with slowly moving reference levels changed by upper system. I gave him my card to look me up if he wants to see Demo1, 2, and Arm.

Bill, what do you make of all this? It seems that he is underestimating what a simple hierarchical control system can do and overestimating the role of adaptive control (which I can only understand as a type of constant reorganization). What can you tell me that will make me look as if I really understand this stuff if he comes to see me?--Gary
==
Gary A. Cziko   Telephone: (217) 333-4382

Date: Fri Jan 24, 1992 3:11 pm PST
Subject: Randall Beer

[from Gary Cziko 920124.1500]

[This note is primarily to CSGnetters on the UNIVERSITY OF ILLINOIS-URBANA campus (there are more than show up on the listserv list since people here access CSGnet via a local Usenet group).]

Randall Beer, author of _Intelligence as Adaptive Behavior: An Experiment in Computational Neuroethology_ will give a lecture here on Tuesday, February 4 at 7:30 pm in room 1005 of the Beckman Institute (across from auditorium on first floor, west side). The title is "Computer Modeling of Insect Walking Movement."

This is the same Randy Beer whose wandering, edge-following, eating computer version of a cockroach was converted to run on DOS machines by Pat and Greg Williams (most recently of BURN fame). This is intriguing stuff, since Beer gets his bug to do what appear to be purposeful things without using Powers's hierarchical control system architecture. I'm still trying to figure out how it works, and I hope to engage Pat and/or Greg in some discussion about this, especially since I can find quite of few of "bad" things in his book (by "bad" I mean apparently inconsistent with current HCT ideas).--Gary

P.S. Pat and Greg are still "selling" their program for $10 a shot. They've promised the newest version, fastest version will soon be mine so I can impress Beer when he gets here.
==
Gary A. Cziko   Telephone: (217) 333-4382

Date: Fri Jan 24, 1992 8:16 pm PST
Subject: Powers' CT vs. Beer's CT

From Greg & Pat Williams (920124)

>Gary Cziko 920124.1500

>This is intriguing stuff, since Beer gets his bug to do what appear to be

>purposeful things without using Powers's hierarchical control system
>architecture.

Well, sort of. We haven't really tried it, but we suspect that the bug's control structure and a PCT (Powers' control theory) control structure of some sort would correspond functionally, though not in detail. Anybody want to try figuring this out? What would be really neat is a bug that does what Beer's does, but has a PCT hierarchy. Demo 3, Bill???

>I'm still trying to figure out how it works...

The big difference between Beer's and Powers' control implementations, as we noted in a paper distributed at the 1991 CSG meeting, is that the former works by changing loop gains and the latter works by changing reference signal settings. Beer uses "IF... THEN" rules to simply inhibit certain loops, so others dominate. There is accumulating neurophysiological evidence that "control of control" (switching behavioral "modes") is in fact accomplished by inhibiting (setting the loop gain to zero of) some otherwise-competing loops.

>I can find quite of few of "bad" things in his book (by "bad" I mean
>apparently inconsistent with current HCT ideas).

Please enumerate the "bad" things you found. Our own impression is that Dr. Beer was making models at a different, but not inconsistent-with-PCT, level. Changing reference levels and changing loop gains can be complementary techniques. We've tried to interest Dr. Beer in PCT, but I don't think he has really given it a serious look -- probably just too busy proceeding along his original path. Maybe you can discuss the contrasts between BCT (bug or Beer's control theory) and PCT in person with him. The bottom line is that we consider Dr. Beer an ally in the struggle against lineal models of behavior. As Rick Marken would say, he is studying CONTROL, and that's the most important thing -- at this point, given the extremely limited data, why quibble about different details of alternative implementations of CONTROL? (As noted above, if one presses the issue, current evidence tends to favor the BCT implementation -- at least for bugs! But then, Beer didn't try to build a working model of a HUMAN. And, to date, no one else has, either!)

Best wishes,

Pat & Greg

Date: Fri Jan 24, 1992 8:25 pm PST
Subject: analysis by synthesis

Analysis by synthesis methods have been tried, but technically, they didn't seem to work well enough to seem like plausible candidats for exploration. Straight parsing is much easier, though it neds to be guided by plausibility (constraints on what the speaker might be meaning).

Avery Andrews

Date: Fri Jan 24, 1992 9:50 pm PST

Subject: a modest proposal

It strikes me that one of the useful functions of a conference group like this is to develop a list of projects that people might work on or direct a student to when the opportunity arises, but the way things are, such projects are likely to get buried in the ebb and flow of discussion and speculation.

So perhaps there should be maintained separately on the file-server a file of projects which have been discussed on the net, and for which there is a general consensus that they are sensible things to actually try to do.

  Avery Andrews

Date: Sat Jan 25, 1992 9:19 am PST
Subject: Language; adaptive control; misc

[From Bill Powers (920124.2100)]

Rick Marken (920124.1210) --

Nice tutorial on imagination.

Martin Taylor (920124.1240) --

>... it is interesting to know whether the potential existence of
>ambiguity is signalled by the linguistic form, and if so, whether the
>converse, the possible synonymous relation between two utterances is
>also signalled.

Zellig Harris claims that his analysis is not based on meanings, but only on empircally-derived linguistic forms. But whenever there is some ambiguity or other problem, he doesn't seem to go back to his distributional statistics, but to employ meanings as the discriminant between alternatives as in his example:

   I drove the Senator from Ohio to Washington (ambiguous original)

   I drove to Washington the Senator from Ohio (expansion I)

   I drove the Senator who was from Ohio to Washington (Expansion II)

The very ambiguity derives from the fact that "Senator from Ohio" has a meaning different from, for example, "President from Ohio." There would be no ambiguity in saying "I drove the President from Ohio to Washington." The difference comes from the meaning of "Senator" and "President". A Senator is spoken of as being from a state, while "the President from Ohio" would imply that there is a President from every state, which we know is incorrect. It isn't language that tells us this, but our experience of American politics.

Actually, Harris' expansion I above doesn't resolve all ambiguity, because the meaning could that I drove the Senator who was from Oregon (mentioned previously) from Ohio to Washington.

A stranger to our political system could conclude that the phrase "Senator from (state name)" is far more frequent than "President from (state name)." The stranger could view this as a distributional peculiarity of English. Parallel differences in usage could be found in many other languages, if in the countries where those languages were spoken there were political figures representing regions and one political figure at a time governing the whole country. The stranger might interpret the apparent structural pattern as indicating something universal to all languages (or some large group of them) when it fact it has nothing to do with languages, but is created only by common meanings.

Bruce Nevin (920124.1331) --

>Bill: circularity arises because "I gotta use words to talk to you."

I understand that. I guess what is still keeping me at arm's length on this is that when you describe perceptual meanings, you seem to do so in the same format that you use to present formal expansions, using the same words or synonyms. It would help me if you could answer my basic question about an expansion like this:

        a group consists of a window and a group contains a window
          and a group contains a window . . .

The basic question is, is this the one and only complete expansion of "windows", or could some other set of words be used to exemplify an equally valid expansion? I'm assuming here that the initial "consists of" should also have been changed to "contains."

If you could say "set" or "category" or "assemblage" just as well as "group", then I don't see how there could be a linguistic rule independent of meaning for deriving expansions from the original. You can't know that "set", "category," and so on are in some way equivalent without consulting meanings.

The term "contains" is no better than "consists of" without some perceptual reference. What perception is it to which you refer by the word "contains?" Can you describe this perception without using "contains" or any of its synonyms and alternatives? That is, without using a dictionary? If you can separate the meaning of "contains" from the word you will be able to do this in many quite different ways, although it may take a lot of words.

The same problem arises with respect to "group." Can you describe the perception to which "group" refers without using any synonyms of "group?" To do this you have to separate the meaning from the word.

And "repetition" ...

I guess the real question is, can you observe the meanings as separate from the words yourself, regardless of the problem of communicating them to me? You don't have to use words to *perceive* them.

Joel Walters (920124.1242) --

>Are perceptions the same as intentions? I remember an earlier comment

>of Powers to Nevins that "words are perceptions." (did I remember
>right?)

Perceptions, in PCT, are reports on the current state of affairs, attributed either to the external world or to imagination. They are only what *is* being experienced, not what should be or is intended to be experienced. If you perceive the word "word," then that is what you are perceiving.

If you are typing and perceive "ward", then you are perceiving "ward." You may have intended to perceive "word." That intention, however, is not what you are perceiving. The intention specifies the outcome that is supposed to occur; perception tells you what did occur. If there's a difference, you have to modify your actions to change what you are perceiving to "word." You type the letters again, this time producing "word." Now the perception matches the intention and you can go on.

Words are perceptions because you can perceive them (see them on paper or or a screen or hear them being spoken by someone else or by yourself). Anything you can experience is a perception, and you can certainly experience words. So words are indeed perceptions, although not the only ones.

One modest elaboration on the PCT model says that intentions -- more generally, reference signals -- are drawn from memories of past perceptions. Reference signals are thus just like perceptions, except that they don't come from the outside world. They are internal models or standards against which perceptions can be compared.

Gary Cziko (920124.1430) --

David Goldstein, Larry Goldfarb. Jeez, Gary.

Re: Gideon Inbar's "adaptive control" model.

>His model of just single joint movement, as far as I could make out, was
>a one-level adaptive control model. The system has a certain fancy
>equation as its reference level (reference equation) and the system
>compares this to what is actually happening and makes necessary changes
>(adaptations; I guess we call it reorganization).

From your description, it seems that Mr. Inbar isn't much constrained by the actual neural processes that go on in arm control. Where does this fancy reference equation reside, and who does the computations in the real arm system? How does his model utilize the tendon reflex and the tonic and phasic stretch reflexes?

>He said that servomechanistic control is no good. The loop gain of the
>basic muscle system through the spinal cord is around one which can't do
>much.

I don't know where he got that information. Is he talking about the tendon reflex, the tonic stretch reflex, or the rate feedback in the phasic stretch reflex? For the spinal systems, position control via the stretch receptors does have a rather low loop gain, but this turns out to be governed by the ratio of tendon to stretch feedback. The function of the spinal loops is mainly to achieve stability. Position control using joint-angle receptors has a much higher loop gain, on the order of 20.

And when visual feedback is introduced, the steady-state loop gain rises into the hundreds.

Inbar is right in saying that adaptation is necessary to handle varying load masses. Not as much is needed when negative feedback is involved. For a two-joint 3-df arm, the main requirement is the the amount of damping from phasic stretch feedback ought to be variable (it isn't in my model).

> He says: fine if the loop gain is high enough; but that is not how we
>work; with high loop gain you put your hand through your desk if you
>think drawer is open but is not.

But that is what you do -- maybe he's never got sore knuckles that way, but others have. What you learn to do is reach out and touch the thing first.

Actually the spinal loops involve touch feedback as well as the other kinds: touch receptors synapse directly onto the motor neurones. In that position they act as force feedback loops. The basic spinal loop is the tendon loop, which is a force-control system. The padding on the fingers is sufficient to allow this loop to operate smoothly when an obstacle is encountered.

>I gave him my card to look me up if he wants to see Demo1, 2, and Arm.

But if he saw them, he might run the risk of changing his mind about something. Don't hold your breath.

>What can you tell me that will make me look as if I really understand
>this stuff if he comes to see me?

If he comes to see you, show him the arm model and ask to see his model running. What does his model do if you switch gravity on and off?

Basically, I'd say don't try. If all he wants to do is prove that the control-system model doesn't work and that his model does work, he will do and say anything needed to prove this. You won't be in a position to know whether what he says is true or not; he'll throw a lot of mathematics at you and avoid discussing his premises.

Just hold out your hand and wiggle your fingers, and repeat after me, "Those are perceptions and I'm controlling them." Nothing else matters a heck of a lot.

Best to all

Bill P.

Date: Sat Jan 25, 1992 11:04 am PST
Subject: multiple perceptions

[Martin Taylor 920125 1310]

Bill Powers' response to Gary about Inbar tweaked an old experience of mine, of which I would like a PCT explanation, because it seems to involve conflict on the perceptual side rather than on the reference side of the hierarchy.

One morning, I awoke with the (apparently normal) feeling of my right arm being in a certain position (say, by my side--I forget exactly what it was). I may have moved slightly, but not much, as I was slowly awakening. Over a period of some tens of seconds I had the sensation of a second right arm appearing like a ghost in another position (say, over my head). The original one was still there, so I had, for some time, two right arms, of which the second (new) one responded to movement commands. The first one slowly faded away, until the perception became normal again.

I presume that the explanation is simple at a neural level. I had cut off by pressure or by stopping the blood flow one set of sensors during sleep, so that I had moved my arm without the motion having been reported back to whatever systems take note of such things. But sensors for absolute position also exist, and when I woke up, these started reporting where the arm truly was. As I moved the arm, the motion sensing system acted upon this new absolutely sensed system, and after a while it dominated the information available, eliminating the old integrated position indications.

In the PCT model, I see no immediately obvious place for this kind of effect. There are, of course, many independent control systems that cooperate in setting arm position, and one might argue that some of these were acting upon new sensory information and some on old, but this does not seem like a very satisfactory explanation because it just pushes the problem one level higher. The fact that needs explaining is how I managed to have an apparently convincing perception of two simultaneously present right arms, one veridical and one presumably remembered from an earlier position.

Conflict on the reference side is presumably handled by either averaging or selection among competing references. Conflict on the perception side may be similarly handled in the usual case. This situation admits neither solution, so far as I can see.

Martin Taylor

Date: Sat Jan 25, 1992 12:47 pm PST
Subject: Re: Language Analysis by Synthesis

[Martin Taylor 920125 15:30]

Gary Cziko proposes analysis by synthesis as a way of recognizing a talker's intentions. Avery Andrews points out that as a method of speech recognition it has been tried and found wanting. I take an intermediate position.

It is true that AbyS is not an effective method of speech recognition. I think this is because the speech signal is highly redundant, meaning that for any intended phoneme string there are numerous waveforms that can satisfactorily represent it. Simple decoding and pattern recognition fare much better than AbyS under these conditions. Nevertheless, I would not be at all surprised if AbyS were not a way of learning to recognize speech--i.e. to build the pattern-recognizing schemes by executing the acts that would produce a variant of the particular speech element.

I do not think it has much to do with actual on-line recognition, unless you think that the alteration of the subjective expected probabilities of incoming speech elements based on sitational and dialogue context should be called AbyS. It usually is not. AbyS usually implies the covert control of articulation (in PCT terms, using the imagination loop at the muscular control level).

AbyS may have a better chance of being valuable at higher levels, which is where Gary proposes that it is used. Redundancy at these levels is much lower, going from level to level, and the interpretation of the signal depends almost entirely on determining the intentions of the talker. These intentions may be clarified by feedback, and normally must be so clarified, unless talekr and listener are incredibly in tune with one another. It is likely that the listener is modelling what the talker is trying to do, and thus generating an imaginary control hierarchy that produces utterance possibilities for comparison with what the talker actually produces. If there isn't too much difference, the model is accepted as reality.

I think most appraoches to this problem mix top-down and bottom-up approaches to deciding what messages have been sent. Where they consider "top" and "bottom" determines which discipline is "in control"--acoustic wave to sentence = speech recognition, phoneme to paragraph or longer monologue = psycholinguistics, interchange in a conversation to whole dialogue = discourse analysis, long texts = rhetoric, extended interchanges = social psychology ... But I think the underlying problem is the same in all cases, and everywhere the expectations are merged with the data to produce best guesses as to the most probable interrpetations of incoming data, and everywhere the data producer tries to use conventionalized tricks to allow the data receiver to make the interpretation both accurate and precise.

Martin

Date: Sat Jan 25, 1992 2:18 pm PST
Subject: newsletter & perceptions

from Ed Ford (920125.15:07)

The CSG Newsletter was mailed today to those who haven't paid their 92 dues but who did pay their 91 dues. The latest edition of Closed Loop is at the printers and will be in the mail along with the newsletter no later than Wednesday to those who are paid up members.

I haven't a forwarding address for K. Copeland. Can anyone be of help?

Rick, appreciate your remarks (920124) concerning my confusion as to what part the imagination plays in the perception signal. Have you ever noticed how honest (or nervous, as with politicians) people become when you begin to take notes when they're speaking to you, especially when it is something for which they are going to be held responsible. Try taking notes the next time a salesperson tries to sell you something. They tend to swtich from the imagination mode to the perceptual mode (i.e. perceived outside world "reality" mode) very quickly. Any more thoughts?

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

Date: Sat Jan 25, 1992 8:48 pm PST
Subject: Language and bugs
[From Bill Powers (920125.2000)]

Avery Andrews (920124.2046),
Martin Taylor (920125.1344) --

RE: analysis by synthesis...

I doubt whether the version of this method that I've been describing has been tried, because it entails comparing meanings with meanings, not words with words. Sketched as a control system, the process I propose looks like this for language production:

```
                                            |
                         ^          Intended meaning
                         |                  |
                         |                  |     (error)
                          ---->-------- Compare ----->-----
                         |                                  |
               meaning found in words     convert meaning err to word err
                         |                                  |
                         |                                  |
             convert words to meanings     convert error to new words
                         |                                  |
                         | < - - - - imagination - - - - - -  |
                    Hear words                         Utter words
                         |                                  |
                           --<--Link for actual speech -----
```

For passive recognition, a higher-level system is needed to provide trial "intended meaning" reference signals. When the perceived "meaning found in words" doesn't fit the trial intended meaning, the imagination connection can be used to supply missing words, correct errors, and expand zeroings. If this fails (too much error remains), the unnamed higher level system must try another intended meaning. The higher-level system receives the current perceived meanings and compares them with some appropriate world-model to see what trial meanings would make sense. Of course if the communication leads to an unambiguous meaning, the trial meaning is quickly matched. (But see revised diagram below)
.
Obviously there's a lot missing here, but the role of meaning is clear.

It would be interesting to write a simulation program in which a limited universe of meanings is defined at several levels of perception, together with a network of rules that links words to them. Then, I think, we could see if Analysis by Synthesis actually works. Example universe of meanings: geometric shapes on a screen that can be moved in various ways to change relationships between them, states of motion, sizes (interpreted as distances) and other simple things. Words could be terms like moving, above, spinning, near, triangle, square, circle, clockwise, up, and their opposites (as appropriate). Each word would be associated with some range of states of the corresponding meanings on the screen: for example, A is "above" B if A is more than n pixels higher for every m pixels to either side. This could initially be played as a game between players. One player constructs sentences using the available words to describe what is on the screen. The other player hears the words, and manipulates the objects on the screen until a description of what is on the screen matches the description heard from the other player. Then the meanings -- the direct appearances of the two screens -- are compared.

Come to think of it, this game would be extremely instructive even if played using natural language -- whatever vocabulary the two players bring to it. When most players compare the two screens after "successful" communication, they will be in for a great shock.

I drew the above diagram before thinking of the game. Now the receiving end looks a little different:

```
                                      select recording
                                             |
       - - - - - record - - -> ref description = remembered
      |         description           description
      |                                      |
      |                --------->-------- Compare ----->-----
      |               |                            (error)  |
      |               |                                     |
      |        own description of                           |
      |         own environment                Adjust real or imagined
      |               |                               environment
      |               |                                     |
Other's description    ---  Real or imagined link -----------
of other's environment
```

"Describing" means converting from a perceived world to a set of words purporting to mean that perceived world.

The diagrams shouldn't be taken too seriously: playing the game will probably suggest better ones.

Avery, you asked for suggested projects. See above!

Martin Taylor (920125.1202) --

RE ghost arm:

In sleep we dream. Dreaming puts large parts of the hierarchy into the imagination mode: all perceptions are generated internally. I think it is fairly common (although not frequent) for the imagination connection to remain in effect for a time during the waking-up process, so the perceptions it is generating can be superimposed on real-time perceptions. I have experienced this at several levels. At a low level, I recall dreaming that something was different about my bedroom, and having the difference persist in awake perception for a few LONG seconds. Very confusing. At a higher level, I've dreamed that I had done something wonderful (actually, I journeyed to the moon and back), and awoke with this conviction still in my mind, so I had the experience of wondering, wide awake, "How could I have forgotten that?" The wonderfulness, of course, gradually faded and I realized that there was really nothing to feel so set-up about. That one was so vivid that even after I was wide awake it was with great reluctance that I finally accepted that it was only a dream. Probably one of THOSE dreams.

I'm not sure we should call this a "conflict" unless it leads to selecting contradictory or incompatible reference levels and trying to carry them out at the same time.

Greg & Pat Williams (920124.2046) --

The "official" HCT model has only one way to turn a system off -- set the reference signal to zero. This really turns the system off only if two-way (positive AND negative error correction) control systems are made up of balanced pairs with excitatory reference signals signifying opposite directions of action. When *neither* side gets an excitatory reference signal, the systems will do nothing regardless of the perceptual signals (which are then inhibitory). This is appropriate for systems that involve simple muscle actions, because muscles can only pull, not push, and must operate in pairs or multiples to achieve actions that can pass through zero. But it doesn't work so well for higher-level systems (what is the opposite of a category or a program?).

Rick and I have tested exactly one model in which a higher-level system does something other than adjust a reference signal. This was for the reversal experiment; the higher-level system adjusted the sign of the error signal of the lower system to maintain negative feedback. I can imagine other signals, including a gating signal that simply turns off the comparator. But I've never tried a model so complex that switching from one kind of control system to another would be required. I don't mean that this would apply only to complex systems -- only that behaviors involving a switch from one kind of control to another are more complex than any I've modeled.

So I have no basic objection to Beer's use of gating signals in his model. It's still actually a control-system model -- look at the behavior of moving so the cockroach's antenna scrapes along an obstacle. If the neural response were made proportional to the amount of bend in the antenna, you've have a continuous control system. The "turning" that results, by the way, effectively integrates the error signal, because turning continues until contact is lost. The resulting orientation is the time-integral of the rate of turning (or the number of turning-events). This gives the control system a very high loop gain. The reference signal for sensed contact is zero (it's omitted), but it could be made non-zero, in which case the cockroach would seek a certain amount of contact (it could then know when it has touched the food instead of having the program know that for it).

>The big difference between Beer's and Powers' control implementations,
>as we noted in a paper distributed at the 1991 CSG meeting, is that the
>former works by changing loop gains and the latter works by changing
>reference signal settings.

"Changing loop gains" is a little misleading, isn't it? As I understand it, the only change possible is from some fixed loop gain to zero. I think a more appropriate way to treat some of the loops (those that move the legs for walking, for example) would be to say that the variables are binary instead of continuous.

If I ever get the time, I'll fire up your program and get Beers' book and Martin Taylor (920125.1202) --

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Avery Andrews (920124.2246) --

Your statement about "parsing" working better than Analysis by Synthesis doesn't apply to language production, does it? I'd like to suggest another project, which is for you to analyse parsing as a perceptual process embedded in a control system. That is:

```
                           Desired structure
                                      |
                     ------>----- Compare ---->-----  (error)
                    |                              |
                    |                              |
      perceived structure of sentence      Revise sentence
                    |                              |
      Input function: parsing process              |
                    |                              |
                     -<- imagined or real sentence--
```

The "perceived structure" would be stated strictly in the terms that result from parsing -- not the words, but the labels for the functions of the words and their relationships. The desired structure would be stated the same way. This system would select for sentences that fit the desired structure, but would accept any sentence that resulted in a perceived structure that matched the reference structure. Other systems could adjust the same sentence with respect to other criteria such as meaning (e.g., substituting different nouns), with this system inserting corrections only if a structure error resulted.

Best to all,

Bill P.

Date: Sun Jan 26, 1992 6:08 am PST
Subject: BUG

From Pat and Greg Williams

>Bill Powers (920125.2000)

>So I have no basic objection to Beer's use of gating signals in his
>model. It's still actually a control-system model...

Exactly the point we were trying to make to Gary. Looking back through Beer's book, we did find some controversial claims about the supposed lack of "representation" of the "outside world" within the bug's nervous system. (Aside: not needing to "represent" the environment is a Big Deal in current AI, since it has proven so difficult to model representations. Points are being given for "non-representational cognitive structures." There are idealistic philosophical motivations for this trend, as well. Personally, we tend to side with the earliest cyberneticist, Kenneth Craik, who theorized that organisms build representational models of their worlds. PCT certainly is representational, which is one more reason for its hard row to hoe in the cognitive science of today.) Here, we think Beer is wrong -- we claim that he implicitly built in representations of goals (corresponding to the representations inherent in the reference signals of PCT), even though such representations aren't always explicit, as they are in PCT (where comparators directly compare perceptual signals with reference signals).

>"Changing loop gains" is a little misleading, isn't it? As I understand
>it, the only change possible is from some fixed loop gain to zero. I
>think a more appropriate way to treat some of the loops (those that move
>the legs for walking, for example) would be to say that the variables are
>binary instead of continuous.

Actually, depending on context, sometimes some "modes" are only partly shut down (intermediate loop gains), and there can even be oscillations between different modes with less than full loop gains. The switching is CONTINUOUS in all cases, but sometimes it happens quite quickly, and then a mode IS effectively "on" or "off."

>If I ever get the time, I'll fire up your program and get Beers' book and
>learn how to design cockroaches explicitly as control systems. This is
>not likely to happen soon, and I'd be just as happy if someone else did
>this.

An "ad" for PCT mentioning your book was included in every copy of our bug program shipped to date (over 100 copies total, mostly to readers of the Beer article in the Sept.-Oct. '91 AMERICAN SCIENTIST). It says that while the artificial cockroach doesn't explicitly utilize PCT ideas, it could. So far, we've not heard from anyone who is trying to remodel the bug with PCT. Sounds like fun! An expanded version of our program is planned, with modifiable synapses, improved speed, more kinds of neuronal types, easier user interface, and lots of other goodies -- it might make sense to wait and use it for making PCT models (including reorganization!).

Best,

Pat & Greg

Date: Sun Jan 26, 1992 8:24 am PST
Subject: L2 Analysis by Synthesis

[from Gary Cziko 920125]

Martin Taylor 920125 15:30 says:

>AbyS may have a better chance of being valuable at higher levels, which is
>where Gary proposes that it is used. Redundancy at these levels is much
>lower, going from level to level, and the interpretation of the signal
>depends almost entirely on determining the intentions of the talker. These
>intentions may be clarified by feedback, and normally must be so clarified,
>unless talekr and listener are incredibly in tune with one another. It
>is likely that the listener is modelling what the talker is trying to do,
>and thus generating an imaginary control hierarchy that produces utterance
>possibilities for comparison with what the talker actually produces. If
>there isn't too much difference, the model is accepted as reality.

Yes, this is more of what I had in mind. But I've just run into a problem with the idea. I don't know why I didn't think of this before.

I spend a fair amount of time listening to other languages mostly on shortwave radio. As I think I already mentioned, I can understand newscast Spanish with virtually no difficulty at all, and yet I speak it very poorly (good accent, lousy vocabularly and grammar). To repeat a sentence from Martin above:

>It is likely that the listener is modelling what the talker is trying to do,
>and thus generating an imaginary control hierarchy that produces utterance
>possibilities for comparison with what the talker actually produces. If
>there isn't too much difference, the model is accepted as reality.

But doesn't this imply that I should be able to speak Spanish much better than I do?

In fact, my knowledge of French and Spanish sometimes allows me to understand a Romance language to a fair degree when I don't even know what the language is! This happened the first time quite a few years ago when I picked Voice of America broadcasting to Brazil in Portuguese. Just a couple of months ago it happened again. This time the language turned out to be Catalan, the language spoken in and around the Barcelona.

So now I'm trying to figure out how AbyS work when I have no language-specific knowledge with which to synthesize?--Gary

Date: Sun Jan 26, 1992 3:09 pm PST
Subject: Re: Language and bugs

[Martin Taylor 920126 17:50]
(Bill Powers 920125.2000)

Interesting: your model for meaning in analysis by synthesis is one component of the Layered Protocol system as currently conceived. In fact, I am supposed at this minute to be reviewing a paper by Dutch colleagues who claim to be introducing the concept into LP theory, when I claim to have introduced it to them! In the current version, the higher level protocol has expectations as to what it will receive (we call those the content of an "Expected Input Queue" or EIQ). The EIQ is part of the Model used by the lower protocol to interpret the incoming message. But in the LP theory, the feedback loop is not only through the imagination of the recipient, but goes back to the originator of the message to ask for error reduction.

I can't say your model has been tried, but we are attempting to develop tools to incorporate it or something very like it into computer interfaces. Your game might be a reasonable test, although we want to use it in the first instance to build an "intelligent" layout system in which "Put the console group to the right of the data display" might be a reasonable command.

Second arm: I know very well the kind of dream continuation you describe, but the percept of the second right arm was not the least like that. It is because of its unique nature that I find it interesting enough to bring up here. Conflicting percepts are not impossible (consider the Necker cube, for example). Dual percepts are less common, and when they involve one's own body parts, they become quite interesting.

I am not convinced that conflict affects only reference signals.

Martin

Date: Sun Jan 26, 1992 3:17 pm PST
Subject: Re: L2 Analysis by Synthesis

[Martin Taylor 920126 18:05]
(Gary Cziko 920125)

>I can understand newscast
>Spanish with virtually no difficulty at all, and yet I speak it very poorly
>(good accent, lousy vocabularly and grammar). To repeat a sentence from
>Martin above:
>
>>It is likely that the listener is modelling what the talker is trying to do,
>>and thus generating an imaginary control hierarchy that produces utterance
>>possibilities for comparison with what the talker actually produces. If
>>there isn't too much difference, the model is accepted as reality.
>
>But doesn't this imply that I should be able to speak Spanish much better
>than I do?

As both you and I stated, that is more likely to apply at higher levels,
where the actual language is not much in evidence. Yes, it should kOhelp
you to learn Spanish, but I think we both would agree that "utterance" here
should be taken at quite a high level of abstraction{

Please forgive the huge numbers of spurious characters that may be in this
message. I don't know which are only on mu line echo and which are on the
transmitting end to the computer.

Martin

Date: Sun Jan 26, 1992 7:45 pm PST
Subject: Analysis by Synthesis, etc.

The prospects for AbyS depend on whether context determines what might
be said next to a sufficient degree that an `expectation driven'
regime will work better than a `data driven' one will. My guess
is that it doesn't, but of course we'll never be sure till we
have working models.

Modern grammatical theories all have the property that `overt'
(reduced) and `covert' (unreduced) structures are related to each
other much more directly than the were in the days of transformational
grammar, and furthermore are related in such manner that a hypothesis
about the covert form can be built up on a word-for-word basis
as the input came in. Hence the incrementally developed hypothesis
about the meaning can be continually checked for contextual plausibility,

and the parser told to go away and come back with something different when its current offering is unacceptable. E.g. one is chugging thru:

Mary saw the boy with a telescope.

and the parser attempts to treat the `with the telescope' as a modifier of `the boy', but since the resulting NP describes no known boy, the parser is told to try something different, and the PP is treated as an instrumental adverbial modifying `see'.

What strikes me as exciting is the idea that control-theoretic logic might provide a better way of orchestrating these sorts of interactions than conventional parsing/production methodology. My current conception of what might be going on is this. Production & Perception both involve the goal `I perceive a grammatical sentence structure'.  For perception, to this are added the goals `this structure be a structure of what is being said to me' and `the meaning of this structure makes sense in terms of what is going on around me' (an error signal from this goal activates language-learning, among other things).  For production, the additional goals would be `this structure means what I want to say' and `I say what it is the structure of'. I haven't figured out how to integrate Bill Power's suggestions into this, but it's early days yet.

My idea of a Grand Demo would be the Little Man driving his wheelchair thru the Crowd, obeying instructions to get the square and put it in the circle, or explaining why he can't do it, etc. Re which, I'm still wondering whether people think my story about how to get a beer is on the right track or out in left field.

Avery Andrews

Date: Sun Jan 26, 1992 10:27 pm PST
Subject: Bugs and language

[From Bill Powers (920126.2000)]

Greg & Pat Williams (920126.0711) --

>(Aside: not needing to "represent" the environment is a Big Deal in
>current AI, since it has proven so difficult to model representations.
>Points are being given for "non-representational cognitive
>structures."...

They must have a homocentric notion of what representation amounts to. If the bug's antenna generates a signal when the antenna is displaced, the the signal represents the displacement, in my usage. But I suppose we have to get more detailed than that. To be more precise: the signal from the antenna is the analogue of excitation of the receptor in the antenna, which we, but not the bugs, know is caused by a mechanical displacement.

The problem here is good old epistemology again. It's easy to be a human being looking at a bug and realize that we know more about the environment than the bug can ever know. It's not so easy to suppose that somewhere in the universe there's an intellect that can look at the environment and know things about it that we will never know -- in fact, know what the true story is behind what we perceive as "displacement." And of course that intellect must muse uncomfortably about a still greater one that would mock its highly advanced story about what is really going on.

So we can see how limited the bug's understanding of an "obstacle" must be, knowing that its information comes from a geometric encounter that is vastly too complex for its poor nervous system to grasp. At the same time, we have to admit that our own visual and tactile information about said obstacle comes to us in much the same way as the bug's information, although we have more and better organized receptors, lenses to help out, and many more hierarchical levels containing many more neurons with which to speculate about what might be causing these perceptions.

Once we get past this epistemological hurdle (assuming we ever do), we can see that we have the analysis backward. The representations are perfectly known to the nervous system; they are identically the neural signals arising from contact with the Outside. What is conjectural is the world that they represent. It isn't that we know what the world is and are trying to figure out how it is represented in the nervous system. It's that we know what the representations are, and are trying to figure out what kind of world could have given rise to them.

To the bug, the signals are the reality -- just as they are for us.

Gary Cziko (020126.0919) --

>I can understand newscast Spanish with virtually no difficulty at all,
>and yet I speak it very poorly (good accent, lousy vocabularly and
>grammar). ...

>But doesn't this imply that I should be able to speak Spanish much
>better than I do?

Understanding means beginning with words and translating them into perceptions. Production means starting with perceptions and converting them into any terms that have those meanings and correct grammar. I don't think these processes are simply "the same thing done the other way around." For one thing, going from heard words to meanings is a convergent process: you don't have to get a meaning for every word in order to piece together a perfectly plausible perceptual interpretation. Also, many words pop meanings up for you; you don't have to construct the basic pieces once the simple association is established. From the pieces you can construct a plausible set of meanings.

Grammar probably doesn't play much of a part in getting the meaning of a heard sentence, except in a language that uses word-endings to specify functions much more extensively than Spanish does (I'm thinking of the problem I had in decoding Latin, even WITH a dictionary at hand). Are you even aware of hearing poor syntax in these radio transmissions when it occurs?

But going from meanings to words is divergent: for any given meaning, there are uncountable sets of words that would convey it. So it isn't as though the meanings automatically suggest what words to use. You have to develop the ability to see what a meaning error implies in terms of how to change your choice of words. That isn't involved in recognition.

Recognition uses only the input part of the control system. Production requires the whole loop. Does that make sense?

Martin Taylor (920126.1605) --

>Interesting: your model for meaning in analysis by synthesis is one
>component of the Layered Protocol system as currently conceived.

How validating for both of us!

>In the current version, the higher level protocol has expectations
>as to what it will receive ...

This "expectation" could exist in two forms: one based on words and their likelihoods of occurring together (verbal contexts), and one based on the perceptions evoked by the words and their memory associations or roles in perceptual models. I favor the latter view, because the meanings we derive from words are far richer and more interconnected than the words are -- we imagine far more detail than the words justify.

You give an example of a command that you would like a computer interface to be able to carry out: "Put the console group to the right of the data display." If you say this to a human being who understands what is going on, you won't have to add "right side up, with the controls facing in the direction where the user will be, aligned with the data display, with the packing crate removed, with no more than a few inches of space between it and the console, etc." -- all the meanings that a human listener will add to the interpretation of the bare command without ever being told to add them. The human listener makes sense of the commands and chooses meanings for them that fit a reasonable picture of the result. Maybe not the picture you had in mind, but a reasonable one nonetheless.

The computer may not make exactly the errors mentioned above, but only because the human programmer, without even realizing it, has taken care of them by providing only those moves that will satisfy the unspoken requirements such as keeping the console right side up and allowing only those positions for it that will leave it aligned with other components and facing the right way. This is done by simply not allowing for other orientations or for rotations, by omitting the packing crate from the start, and by using predesignated positions as the only possibilities. One always has to be alert for what is being put into the model before it is ever run.

>Conflicting percepts are not impossible ...

Well, this comes down to a matter of definition. I would hold out against saying that conflict can inhere in percepts, even double-exposure percepts. So you have two arms on one side -- so what? This doesn't seem to me to entail a conflict unless there's something you want to do about it, or with both arms, or if some higher-level reference level is violated. I would rather make conflict a technical term that means two control systems trying to control the same thing in two different states of the same degree of freedom at the same time. This isn't the same thing as "difficulty" -- trying to do something and finding that it doesn't work. Only if the cause of the difficulty is a second control system opposing the efforts of the first would I use the term conflict.

Even in the usage you seem to be proposing, the problem with mutually-inconsistent percepts is something a higher-level system has to notice and remark upon. The percepts themselves aren't concerned with the inconsistency or contradiction -- those are judgements ABOUT the percepts, made by a different process that is examining them both. As you were examining this phenomenon of the ghost arm, and having certain thoughts about it. I see a statement that percepts can conflict in the same category as statements that words have meanings.

Avery Andrews (920126.2038) --

>What strikes me as exciting is the idea that control-theoretic logic
>might provide a better way of orchestrating these sorts of interactions
>than conventional parsing/production methodology.

Have at it. I'm sure you're aware that PCT is so young that everyone now interested in it is a pioneer. Some day the people now struggling to find applications will be cited as authorities, but right now there are no authorities. So pick a problem, explore it, and let us know what you discover. That will make you the Father of Whatever-it-is.

>My idea of a Grand Demo would be the Little Man driving his wheelchair
>thru the Crowd, obeying instructions to get the square and put it in
>the circle, or explaining why he can't do it, etc. Re which, I'm
>still wondering whether people think my story about how to get a beer
>is on the right track or out in left field.

Why not? As to the beer story, I've expressed my opinion that certain parts of it would be better left to the analog control systems, with the verbal parts limited to doing what is best done by symbol-manipulations. That latter doesn't include telling the arm how many degrees to move, etc. Think of it this way: let the lower levels do all the things that a very skillful and experienced chimpanzee or gorilla could do. Then see what capabilities language would add, remembering that chimpanzees and gorillas can probably carry out (simple) programs, order sequences, select categories of perception, and all the rest to the bottom, all without words. What differences do the words make?

Best

Bill P.

Date: Mon Jan 27, 1992 4:58 am PST
Subject: Re: BUG

[From Chris Maclolm]

>From Pat and Greg Williams

>Looking back through Beer's
>book, we did find some controversial claims about the supposed lack of
>"representation" of the "outside world" within the bug's nervous system.
>(Aside: not needing to "represent" the environment is a Big Deal in current
>AI, since it has proven so difficult to model representations. Points are
>being given for "non-representational cognitive structures." There are

>idealistic philosophical motivations for this trend, as well. Personally, we
>tend to side with the earliest cyberneticist, Kenneth Craik, who theorized
>that organisms build representational models of their worlds. PCT certainly
>is representational, which is one more reason for its hard row to hoe in the
>cognitive science of today.) Here, we think Beer is wrong -- we claim that he
>implicitly built in representations of goals (corresponding to the
>representations inherent in the reference signals of PCT), even though such
>representations aren't always explicit, as they are in PCT (where comparators
>directly compare perceptual signals with reference signals).

The important point in the current debate in AI about representations concerns whether they are global or local, i.e., cognitively penetrable or not. The difficulties in representation have arisen in devising complex detailed representations of the state of the world which permit the kinds of reasoning required.

It is true that any old number in a computer program can be considered to be some kind of a representation, but that is not the point at issue. Local goals, consisting of a number or two, or even a small record of some kind, are not representations in the sense of the current dispute.

Unfortunately it is also true that there are plenty of armchair philosophers willing to jump in and muddy the waters by arguing endlessly whether or not the set-point of a servo or the procedural architecture of a program constitute representations, and how much matters the extent to which these are explicit, implicit, or tacit representations. These are all side issues. The crucial question is whether or not the representation is cognitively penetrable.

Of course there is another can of worms labelled "cognitively penetrable from what's point of view?", but the general debate hasn't got that far yet...

Anyhow, the important point is that in the sense of the current representation debate in AI Beer's roach does not use representations. Impressively, it also manages to get more capability with even less of a centralised architecture than Brooks's subsumption implementation of insect gait in Genghis (which does have a central "gait machine").

Chris Malcolm
Dept of Artificial Intelligence
Edinburgh University

Date: Mon Jan 27, 1992 6:19 am PST
Subject: analysis by synthesis

[From: Bruce Nevin (Mon 920127 07:44:34)]

(Gary Cziko 920124.0600) --

>I am going to assume first that Bill is asking how the listener gets from
>the reduced form (which he or she hears or reads) to the unreduced form
>which points to the perceptual base of the sentence and thus the intended
>meaning of the speaker.

This is one reading of Bill's question (920122.2030):

>Ok. I'm still asking how you get from the reduced form to the unreduced
>form without first going to the perceptual meanings.

The other main reading is: how does the language user learn what reductions are available for the language. The reduction system does not give completely unlimited license to change the shape or position of anything you please. Nor is it governed by correspondence of the changed forms (or the changes in the forms) with nonverbal perceptions--too many arbitrary conventions. (Let's see, if the corresponding perception is supposed to be at a time before my saying this, I add -ed, except after a voiceless sound it's [t] ("missed"), and after "burn" it might be -t, and for "go" I say "wen" and add -t, and after "beat" I don't add anything, and . . . .)

It is to this reading of Bill's question that I have been responding. The reason is that once the language user has learned the reductions for the language, the answer to the other reading of Bill's question is that the language user (generic "you" in the question) uses the reductions that are available for the language--and also the correspondence of candidate reconstructed (unreduced) forms to nonverbal perceptions. Why should it be an either-or situation? I have not proposed that it was.

Given knowledge of the reductions for the language (and of course knowledge of the vocabulary, and for each word its argument requirement and its correspondences to nonverbal perceptions) the language user can determine the operator-argument dependencies that must be correlated with nonverbal perceptions.

In principle, the language user might be able to reconstruct the operator-argument dependencies for the whole utterance without referring to the nonverbal perceptions. I doubt very much that this happens in practice. It seems likely to me that operator-argument subtrees are associated with nonverbal perceptions as soon as the operator-recognizers pass along their perceptual signals.

>Perhaps the answer is that the listener does not
>need to go from the reduced form to the unreduced form because he or she
>HAS ALREADY BEEN THERE. I am proposing that we seriously consider an
>analysis-by-synthesis (hypothetico-deductive; Neisserian; Popperian) model
>by which the listener (a) starts with trying to figure out what
>perception(s) the speaker may be trying to communicate are (i.e., makes a
>conjecture concerning the unreduced form), (b) uses this to create the
>reduced form (mentally), (c) tries to match his or her reduced form with
>that of the speaker, and (d) goes back to (a) if there is no match. Can it
>be this simple? Or have I really misunderstood what this discussion is all
>about here?

I think this is quite plausible. Consider the "do you mean . . . ?" types of rejoinders. And yes, I agree about ambiguity, though the stuff Tannen is talking about is at a discourse level, beyond sentence syntax --presuppositions about the motivations and modi operandi of others may be. In Harrisian terms, these would be differences in the zeroed "common knowledge" sentences like the example I have "and one uses an umbrella to keep off rain." (Inclusion of these by undoing the zeroing ordinarily brings out formal regularities in the structure of the discourse, across sentences. In Tannen's examples of misunderstandings, the utterances produced by the two parties to the dialogue would show incommensurate discourse structure.) Similarly for more extreme forms of cross-cultural communication.

Bruce
bn@bbn.com

Date: Mon Jan 27, 1992 7:33 am PST
Subject: Bugs' representations/analogies

From Greg & Pat Williams (920127)

>From Bill Powers (920126.2000)

>They [AIers] must have a homocentric notion of what representation amounts
>to.

Exactly. They're used to linguistic issues, and when they run up against issues in non-linguistic AI (a phrase which probably still draws laughs from some of them as incoherent), they tend to see everything in terms of "cognitive" issues. At this stage, I think it is OK if Beer and others want to make the distinction between linguistic AI and non-linguistic AI by claiming that no representation (as used in the former) inheres in the latter. Still, in at least some non-linguistic AI models there is what you have termed (very rightly, we think) "analogy." Beer's cockroach's neuronal dynamics ARE analogical with respect to occurrences in the cockroach's "world."

>From Chris Malcolm

>It is true that any old number in a computer program can be considered
>to be some kind of a representation, but that is not the point at issue.
>Local goals, consisting of a number or two, or even a small record of
>some kind, are not representations in the sense of the current dispute.

It sounds to us like the REAL issue is whether there are symbolic manipulations going on which are organized systematically (as sentences in a language, for example). One could argue that AI notions of BOTH symbol manipulation AND systematic organization are not only homeocentric but rather poverty stricken. But who are we to argue with definitions already agreed upon? All right, in the AI sense of representation, Beer's bug doesn't represent. Granted, that is an important point to set the linguistic AIers aback. But the more important point from the PCT viewpoint is that adaptive behavior at the non-linguistic level REQUIRES analogizing (in Bill's sense) for robustness. The feedforward gait-producing circuitry of Genghis, for example, will work poorly (because it lacks feedback) under some circumstances. And where there is feedback, there is analogizing.

Greg & Pat Williams

Date: Mon Jan 27, 1992 10:49 am PST
Subject: cognitive impenetrability

[From Rick Marken (920127)]

Chris Maclolm says:

>The important point in the current debate in AI about representations
>concerns whether they are global or local, i.e., cognitively penetrable
>or not.

Chris. Could you please explain what "cognitive penetrability" is? It sounds great -- veddy intellectual with just a touch obscenity. If PCT people could make up names like that we'd be famous in no time.

> The crucial question is
>whether or not the representation is cognitively penetrable.

Please, Chris, what is this? I really want to know.

>Of course there is another can of worms labelled "cognitively penetrable
>from what's point of view?", but the general debate hasn't got that far
>yet...

Great. Then I can be ahead of that game if I can just figure out what "cognitive penetrability" means.

>Imagination Update

I have implemented the "imagination connection" properly in my Excel hierarchy (three levels, six systems at each level). One thing I realized while implementing the hierarchy is that control of imagined perceptions must have constraints in terms of speed of operation and system gain that you have when controlling "real" perceptions. I sometimes had stability problems when I switched a system from real to imagination control mode using the same systems parameters (slowing and gain). I have not tried to figure out what is going on yet -- maybe you have some suggestions Bill P.? Anyway, I realized that, according to the PCT model, control of imagined perceptions involves dynamics -- the model cannot make the imagined perception match the reference signal instantly -- even though the only thing that would physically limit this is how quickly neurons can change from one firing rate to another. I think there is evidence that imagined perceptions do change more slowly than might be possible if there were only neural constraints. This is the work of Shepard and Metzler and others on "mental rotations". People seem to rotate imagined perceptions of a 3-D figure to test for a match with another figure, at something close to the same rate at which they would rotate the "real" figures. The PCT model might suggest some details of how this imagined control is done. I don't know if there are any studies of direct control of the perception of match between rotated figures. I think it would not be too hard to set up a task where a subject is asked to rotate a figure into convergence with another (on a computer screen) and then compare the time it takes to do this (on the screen) with the time to do it in imagination.

Avery -- possibly another study for your list?

Hasta Luego

Rick

Date: Mon Jan 27, 1992 2:58 pm PST

Subject: getting beer

Bill: are your remarks on leaving as much as possible up to analog systems about my `pseudo-prolog' notation posting? I kinda thought that's what I was doing there, with the notation expressing logical dependencies between the analog goals. E.g. it makes no sense to reach for something that isn't within reach.

Avery Andrews

Date: Mon Jan 27, 1992 3:12 pm PST
Subject: syntax & meaning

Here is a posting from sci.lang, by a sensible person who is quite knowledgeable about language processing:

Newsgroups: sci.lang
Subject: Re: dumping on Schank
Message-ID: <64796@bcsaic.UUCP>
From: rwojcik@bcsaic.UUCP (Rick Wojcik)
Date: 20 Jan 92 23:12:45 GMT
References: <1992Jan17.225647.8616@ils.nwu.edu>
<1992Jan18.030056.1786@athena.cs.uga.edu>
Organization: Boeing Computer Services AI Center, Seattle
Lines: 59

Michael Covington (MC) writes:

MC> To be more precise, viewed in retrospect Schank's work falls under
MC> knowledge representation, not computational linguistics. As such, it has
MC> some value.

I don't agree with Michael on this one. I believe that much of the criticism against Schank has been as unjust as his criticism of linguists (which is another way of saying that *some* of it has been just :-). Most current NLP research does rely more heavily on linguistic theory than Schank might have predicted in his earlier days. But linguistic theory had a major weakness: it was (and still is) dominated by sentence-level analysis. A major impact of Schank's work, besides being among the first to use frame-based knowledge representations, was its emphasis on discourse level interpretation. His books and articles on scripts, plans, and goals are well worth the effort for any linguist who is serious about text understanding. Generative linguistic theory has almost nothing to say about how people understand language. If Schank and his followers are overly critical (or ignorant) of linguistic theory, that is no excuse for linguists to make the same mistake with respect to Schank's work.

MC> My understanding was that Schank's position was that syntax was of little
MC> or no value for understanding natural language. That claim is empirically
MC> false and virtually all the NLP I've seen in the last 10 years assumes the
MC> opposite.

This is only partially true. My understanding is that Schank wanted to see how far he could get with *minimal* syntactic analysis. He had to pay attention to some constructs--e.g. passives. So-called linguistic approaches have grown in popularity because syntactic variation in natural language is far richer than Schank realized. Schank can get away with processing text that is somewhat syntactically predictable, but more robust syntactic knowledge is needed to handle text with moderate to complex syntactic variation. A well-known linguist once referred to Schank's approach (a little inaccurately) as "word-guided mental telepathy", and I have always enjoyed that description.

MC> Do you draw a line between knowledge representation and NLP? Surely
MC> scripts are mainly a means of understanding _events_, regardless of
MC> whether you acquire the information through language or through, say,
MC> vision or something else. So I consider scripts to be a contribution to
MC> knowledge representation rather than directly to NLP.

NLP is perforce interested mainly in linguistic performance. Since generative grammarians have never explained the connection between competence and performance, their theories are not, strictly speaking, relevant to NLP. In particular, generative linguistic theories do not usually touch on such central NLP concerns as disambiguation and ill-formed input. Most people (even bona fide "Chomskyites") would agree that pragmatic and discourse knowledge are essential to such simple activities as picking out the correct word sense, handling unexpected input, or figuring out which syntactic analysis is to be preferred over others. It makes little sense to try to exclude knowledge representation from NLP. It has to be handled somewhere in a performance model of linguistic processing, which is basically what NLP is. Generative theory does not claim to be all of what NLP needs, although linguists themselves, eager to please, often forget this.
--
Rick Wojcik   csnet: rwojcik@atc.boeing.com
              uucp: uw-beaver!bcsaic!rwojcik

Date: Tue Jan 28, 1992 9:49 am PST
Subject: Info needed

Dear Pat & Greg,

I tried to order Powers' "Living Control Systems", but it did not work out. Could you please tell me how I could receive a copy of it (btw, I thought PCT means Perceptual Control Theory, not Powers' Control Theory).

Regards,

Uwe

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Date: Tue Jan 28, 1992 10:43 am PST
Subject: plural and repetition

Various languages of the world express plurality by reduplication --that is, by repeating the singular form twice. I don't have any examples at hand, don't recall e.g. Polynesian examples, but to make one up for illustration, if bot meant stone then botbot would mean stones. This device has been taken from one or more of these languages for plural formation in Pidgin.

Bruce
bn@bbn.com

Date: Tue Jan 28, 1992 10:59 am PST
Subject: some language issues

[From: Bruce Nevin (Mon 920127 11:46:52) (Tue 920128 07:58:38)]

(Bill Powers920122.2030)
>
>The same problem is overlooked by Martin Taylor (920122.1616). He accepts
>
>>   a group consists of a window and a window and a window . . .
>
>as an expansion of "windows." But how do you get from "windows", which is
>what you hear, to "a group ..."? There's a hidden, unspoken, taken-for-
>granted process in there. I'm trying to call attention to that process,
>not to its outcome.

Please be careful. The words "a group" were *overtly* present in the phrase "a group of windows." I was discussing the source of the plural -s on "windows" (not forgetting the disappearance of "a"). You appear to believe that the phrase "a group" is to be reconstructed as a source for the plural. (A word like "group" or "set" or "team" accounts for the difference in meaning between "Offenbach and Lehar wrote operettas" and "Gilbert and Sullivan wrote operettas," i.e. "the team of Gilbert and Sullivan wrote operettas").

>I'm suggesting that the hidden process is a direct nonverbal perception
>of the pluralness of "windows," indicated by the terminal "s", and that
>this sense of pluralness indicates a particular type of nonverbal
>perception of multiplicity -- which I think may be an aspect of the
>category level (although any other hypothesis is welcome and the correct
>identification of the level is immaterial here).

I am suggesting that the direct nonverbal perception is that there is one window and *there* is one window and *there* is one window and so on ("and so on" referring to the perception that I don't have to count them one by one--though some compulsive folks do--but that I could if I chose to--imagination involved here, I think). This perception correlates with "a window and a window and a window . . ." and it also correlates with "windows". Since it does correlate with both, I am suggesting that the act of correlating words with perceptions use the most explicit form of the words.

This is no more than Avery was saying (Tue, 21 Jan 1992 15:47:31)

>I suspect that it might be possible to build a gadget that would detect
>`instances of the same thing', without registering much about that
>that kind of thing is. Then noticing (a) a window (b) that there
>are more of them would add up to noticing that there are windows.

And it is no more than you were saying (920122.2030):

>This mysterious "sense of pluralness" is not basically mysterious. It's
>an easily observed empirical fact. Putting all words aside, and just
>looking at the string of characters . . . TTTTTTTTTTTTTTTTTTTTXTTTTT . . .
>one can immediately perceive the multiplicity of the Ts. This is a set
>of configurations, each individually recognizeable as a familiar object,
>and ALL ALIKE (except one). The "all alikeness" that is perceived is the
>sense of configuration. But in addition to recognizing the
>configuration, we get the immediate impression that there are MANY of
>that configuration. That manyness is the sense of multiplicity to which
>I refer. It is not necessary to say to oneself, "A T and a T and a T and
>a T .." to perceive this multiplicity.

But one must perceive at least one T to get the T perception and one must perceive its likeness to at least one other to get the perception of repetition of the T perception. It is the perception of repetition of the T perception that constitutes what you are calling a "manyness" perception. Recognizing that it is merely a perception of repetition of a configuration perception (signal) seems considerably less mysterious than a "manyness" perception.

The T perception is correlated with the linguistic perception of the letter-name "T". The perception that the T perception is repeated is correlated with the perception that the name "T" is repeated. One does not have to name each T individually, with an accurate count, only "a T and a T and so on". (Of course if they were few enough one could recognize the number as a configuration.) Similarly, the configuration perception and the perception of its repetition are both at a lower level than the category identification as a T. (This supports the performance differences that Avery surmised.)

In all of this I am only saying that words are correlated with perceptions. The repetition of the same word "and so on" is correlated with the repetition "and so on" of the corresponding perception.

Now, is there a perceptual signal that says "the perceptual signal from this configuration recognizer is repeated in different places"? What does "in different places" mean?

Last night I looked across the subway car at a grate on the front panel of the heater outlest under the opposite seat. It was like so:

```
        |  |  |-------|  |  |-------|  |  |-------|  |  |-------
        |  |  |-------|  |  |-------|  |  |-------|  |  |-------
        |  |  |-------|  |  |-------|  |  |-------|  |  |-------
        -------|  |  |-------|  |  |-------|  |  |-------|  |  |
        -------|  |  |-------|  |  |-------|  |  |-------|  |  |
        -------|  |  |-------|  |  |-------|  |  |-------|  |  |
```

A long rectangle, composed of a line of squares, each composed of four smaller squares. Each smallest square was composed of three lines. Each line was a hole in the stainless steel. Each hole was a rectangle with rounded ends, whose length and breadth were roughly in the same proportion to each other as the length and breadth of the whole grate pattern. The order of these perceptions coming to my awareness (or my directing awareness to them) was as I have given them here, except that I probably perceived that there was a pattern to be attended to before I noticed that it filled out a rectangle, due to partial obscuration by the seat. Because of this accident I had to bend down to see all of it.

I think the perception of repetition that we are concerned with for the string of Ts is a function of the configuration recognizer that perceives it as a string.

But for many plurals, there is no configuration of which the "many" things pluralized can be constituents, and to imagine one is I think folly. We know from hypnosis research how willing memory and imagination are to comply with suggestions. In our search for perceptions behind words we tread through a hall of mirrors. If you say all the elm trees in the U.S. died do you have a perception of the aggregate of those trees? It seems to me that I imagine a tree or two (with perhaps more detail than for those of us who don't remember elm trees), and then imagine, less concretely, something that we might *talk* about as "and so on, amounting to all of them". It is to these latter perceptions--the "and so on" perception of repetition and the perception of exhausting the membership of the category--that the words of the unreduced *linguistic* form (AND the words and plural suffix -s of the reduced linguistic form) correspond.

>So the linguistic expansion simply begs the question of how we perceive
>manyness in EITHER the perceptions themselves or in an "unreduced
>linguistic form" that ALSO shows the same apparent characteristic. One
>needs the same kind of perceptual function to see
>   _   _   _
> | | | | | | ...
>   -   -   -
>
>and say it is the meaning of "windows" as one needs to hear
>
>   "a window and a window and a window and a window ..."
>
>and say it is the same as "windows." In the one case we perceive the
>manyness of the window figure; in the other we perceive the manyness of
>the phrases. The sense of manyness is neither the window configuration
>nor a phrase. It is the result of a higher level of perception applied to
>both. It is a nonverbal perception.

When will you notice that I have been agreeing with this?

>By use of a terminal -s we can refer to the nonverbal sense of
>multiplicity directly and economically. We can also create an analogous
>and much more wordy phrase that produces the same sense of multiplicity.

Not only that, but the explicit expansions distinguish ambiguities in the plural -s (such as "the team of Gilbert and Sullivan" and generic plurals), and on the other hand, the reduction is not always to -s. It is *only* in the unreduced form that the words are identifiable for correlation with perceptions (meanings).

>The connection between a configuration name with "s" appended and the

>"expansion" is not between one linguistic form and another linguistic
>form: it is between either form and the underlying perception. We are
>simply paraphrasing a meaning.

And for a different source that reduces to identical -s plural we are "simply paraphrasing" those different meanings. In the reduced forms there may be linguistic ambiguity, in the unreduced sources there is no linguistic ambiguity. Linguistic ambiguity is a multiplicity of mappings from form to meaning within the coarse sieve of linguistic categories. There is still multiplicity and idiosyncratic variation in the associative meanings, but this sort of ambiguity is to linguistic ambiguity as associative meanings are to linguistic information (q.v.).

The motivation for this is not so sharply defined for the plural as it is for reductions whose linguistic ambiguity is more obvious. The proposed source does account for certain restrictions on the plural, as for example the fact that only count nouns (corresponding to perceptions of countable objects) can ordinarily be pluralized. When a mass or aggregate noun like sand is pluralized, it is really another noun which has been zeroed that was plural:

all the sands <-- all the kinds of sand
all the sands <-- +all the sand-places

Here, proposed +sand-places is from something like "places made up of sand." The fact that this usage is now archaic accounts for the archaic "feel" of e.g. "it washed up on the sands of Nantucket."

So you see, if you go directly from nonverbal perceptions to the plural suffix, you have to account for the association with countability anyway. If you take the -s as reduced from a conjunction of count nouns (countable things), you get that association for free. And of course you also have to account for how the same perceptions as are associated with the -s suffix (and its allomorphs, including zero) are also associated with conjunctions under "and" (and plural agreement in the verb), and so on.

>If this is true, then characterizing the relationship between "windows"
>and "a window and a window and a window" as "expansion" or "reduction" is
>erroneous. What is erroneous is the assumption that the one can be
>obtained from the other by some sort of strictly linguistic procedure,
>without crossing the border into semantics.

Of course the language learner and the linguist alike lean on meanings. But the relations among words are always such that reductions that are consistent across the language (not ad hoc or arbitrary) can be undone, revealing a dependency structure of operators and their arguments. The fact that this structure is maintained as the language changes through time, as dialects diverge into different languages, as languages converge (cf. the origins of modern English), is prima facie evidence that language users control for this structure. The fact that these dependency structures do not correspond in a simple way to perceptions one for one, but selectively, and with a selection that differs in detail from one language to another and from one point to another in the history of the "same" language, suggests that it is not merely reflection of dependencies inherent in the world of our perceptions. There must be some truth in this, but it is also true that language imposes categorizations upon the world of our perceptions, that linguistic categories are learned of a piece with the learning of language, and that language users control for conformity to them for the sake of higher goals requiring cooperative action.

You attribute to me an

>assumption that the one can be
>obtained from the other by some sort of strictly linguistic procedure,
>without crossing the border into semantics

This is ambiguous in the way to which I am becoming accustomed. Do you mean that the language user obtains source forms for heard reduced forms and vice versa without reliance on meanings? I have answered that: of course not. Do you mean that the language learner acquires the words, reductions, and word dependencies (sometimes concealed by reductions) without reliance on meanings? I have answered that: of course not. Do you mean the linguist comes up with all this without reliance on meanings? Same answer.

More on linguistic methodology follows. You attribute to Harris a quest for a mechanical procedure that can discover the structure of a language without reference to meanings. This is a misattribution.

(Bill Powers (920123.2200)) --

You respond to Martin's guess that "consists of" might be a reduction of something like "having as a part."

>what connects
>"having as a part" to "consists of?" I am trying to draw attention to
>something that both of these terms point to: the imagined perception that
>gives either one meaning.

>If there were a mechanism or rule that makes "has as a part" a linguistic
>expansion of "consists of," then both input to and output from this rule
>would be nothing but words without meanings. Meanings in a purely
>linguistic analysis supposedly don't enter the argument. As a result,
>there could be only one output, given the input. There would be no need
>to check the perceptual validity of the output, because linguistic rules
>are assumed to be independent of meanings.

Linguists do rely upon meanings--their understandings of the correlation of linguistic expressions with nonverbal perceptions--to arrive at guesses and proposals. They also use all sorts of heuristic rules of thumb and generalizations about the variety of languages, some of them under the brave headings "language typology" and "language universals." But then linguists check that their guess or proposal is consistent with other statements about the language in a theory of language.

For Harris's methodology, all such proposals must be verified by seeing if they *could* have been arrived at on the basis of criteria that do not rely on knowledge of meanings per se. Hence the confusion about his claims.

Two of the reasons for this are the loose correlation of language to meaning and the absence of an external metalanguage.

We have discussed the loose correlation of words and meanings.

Meanings in general are much richer and more variable than the language used to direct the attention of language users to them. If you use meanings as a criterion for analysis of language, you have the problem of determining which meanings are relevant all the time and which are contingent upon the immediate situation and history of the particular language users. You cannot simply say that it is always and only the particular detailed perceptions of the language user that are relevant --these differ in detail even among the participants in the same conversation.

We have also discussed the absence of an external metalanguage. Tom Ryckman's paper goes into this question. UG folks claim that the brain is structured in such a way that language turns out thus and so. They seem not to be interested in the question of how this might have come about through evolution. Nor is it evidently the case that the environing world is so structured that the development of hierarchical perceptual control necessarily eventuates in language in such a way as to dictate the structure of language. To claim this requires one to ignore the structure that is in language, structure that language users impose on their perceptions for the sake of doing things with language and for the sake of goals involving cooperative action. There is no standpoint outside of language from which to account for language.

It is because of this limitation that we tie ourselves in knots trying to account for the correlation of words with meanings. You ask me what perceptions come immediately to my awareness associated with certain words. I attend to my perceptions. Then I describe them to you. Then you take me to task for only giving you more words. But I can do no other. The descriptions of perceptions are not further "expansions," they are descriptions (in words) of perceptions associated with the words you selected. The way out of the "circle of words" is for you to attend to your perceptions associated with the selected words. Then you describe those perceptions. Then you compare your description to mine. The comparison starts out on a linguistic basis. You notice a difference between the words you used and the words I used. You attend the perceptions you associate respectively with the different words. Among all the perceptions associated with word A and all the perceptions associated with word B, you attend particularly to the perceptions that are the most different. (This looks like a familiar process of polarization to me.) You attend to a perception of difference between these perceptions. Word A "becomes" for you primarily those perceptions that most differentiate it from word B, and conversely. You call this "getting closer." I only have to be persuaded a little more to get it "right."

But the way out of the circle of words has turned out to lead right back into the verbal maelstrom again. Why? Because the only way we have of indicating our respective perceptions is words. Words correspond to perceptions only in a loose way. The correspondences have all their corners and sharp edges rounded off, and many of their finer details, like coins which have passed through many hands. That is of course just what they are, coin of the perceptual realm. The analogy breaks down, of course. We constantly refresh the images on the coins--each according to our own perceptual lights.

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

At the end of your response to Gary, Bill:

>Of course if Bruce now tells us that for any reduced sentence, there is
>one and only one valid expansion into an unreduced form (that is, only
>one sequence of specific words that will result), my hypothesis about
>linguistics takes a dive.

No, many reduced forms are ambiguous. Futhermore, most reductions are optional. There is thus a many-many correspondence.

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

(Bill Powers (920124.2100)) --

>Zellig Harris claims that his analysis is not based on meanings, but only
>on empircally-derived linguistic forms. But whenever there is some
>ambiguity or other problem, he doesn't seem to go back to his
>distributional statistics, but to employ meanings as the discriminant
>between alternatives . . .

Harris's analysis does rest on meaning, but in a closely controlled way: is one word combination differently acceptable than another word combination?

It works like so: take a set of sentences that differ only with respect to one word. Rank the sentences in the set as to acceptability, or note any peculiarities of acceptability (various ways to do this), or roughly estimate their likelihood of occurring. Pick two sentences from the set that differ markedly in the judgements made of their acceptability or likelihood. Then take another set of sentences that have the same word selection, such that the sentences of set A appear to differ from those of set B by an operator and its other arguments, or by some zeroing or other reduction. Pick the corresponding two sentences from set B. If they have the same relative acceptability difference or likelihood difference (or whatever the criterion was that you picked), and this is so for any pair you pick, then the differences between the two can be accounted forby one or more steps of operators and reductions. If they are the ranking is scrambled, the proposed grammatical relation fails. (Under some operators, acceptability differences are compressed, but never reversed or scrambled; believe is an example.)

You quote an example of different expansions for an ambiguous sentence. The only way you can imagine determining that relationship is by reference to meanings. You therefore claim that he must have done it that way. That is a mistake. As I noted earlier, an understanding of meanings provides a heuristic guide for guesses and proposals. But the guesses and proposals must be verified by the stated criterion.

A little further on you confuse distributional structure with statistical frequency. Distributional structure in linguistics has nothing to do with frequency. You protested once that I must therefore claim that "the" must be in a cooccurrance class with "and" because I have encountered it at least once. But I did not encounter it in isolation, but only in contexts that provided words of distribution classes with many members. The proposed distribution classes for the cooccurrance of "the and" would have only the single member "the" in one and the single member "and" in the other. Preponderance rests not with frequency, then, but with the productivity of a word class.

Too much for one day. (Well, two days.) I'll be continuing to play catch-up, as a break from unpacking, meetings, and research and writing on internets and IP routers.

Bruce
bn@bbn.com

Date: Tue Jan 28, 1992 12:35 pm PST
Subject: causality

Hello to all,
Well, I haven't been listening in to the net for many many months, but that is not indicative of where my academic pursuits reside. Presently I am working on a Master's thesis on Psychological theory and Phenomenon with causality as the primary emphasis and CT as the secondary--that may or may not reverse as I do along. In any case, CT is central to this thesis, and as a result I expect to be checking in on the net alot more, but only through the news board here--not directly to the net. So if you have any specific responses for me, I'd appreciate it if a copy were sent to my personal mailbox (but its not absolutely necessary).

In my thesis I make a distinction between ontological causes and designated ones. This distinction is important, I think, for in everyday use of the term "cause" we may say that not wearing a seatbelt caused the person to hit the windshield in the accident, but we cannot give ontological status to such a cause since not doing something is not a something to speak of. Not wearing a seatbelt cannot be an ontological cause. Similarly we may speak casually that the loud noise caused the person to jump, but we of course realize that the loud noise has no real (ontological) means of causing a behavior. I would say that pushing the vase off of the table caused (ontologically) it to fall.

Now then, lets say that I want to exit through some door. There is a straight path that I will probably take. But some person gets in my way so I make a little detour in my path out the door. I can say in a colloquial or designated sense that this person caused me to make this detour. But I do not want to say that this obstruction had any ontic causal status.

Now I think of a robot which is designed as a control system. I have it referenced to exit though this same door, and like the person when faced with an obstruction will make a detour, (in exactly the same way because its an identical control system in all relevant respects). But in this scenerio, I want to say that indeed the obstruction caused (ontologically) the robot to take this detour. Now I feel inconsistent. If they are both control systems the descriptions should be identical, logically. Why then does it seem different, intuitively? At first I thought I was biased against nonliving control systems. But now it seems that I find the problem with that nasty environment/organism dotted line. The line seems a little more "real" for the living than the nonliving. When the temperature drop of the room causes (ontologically) the furnace to go on, I can explain it with physics and the line itself becomes a designation without real ontological status. That line seems "more real" with a living system.

So my question is "Is there a difference in relation to causality between a living and a nonliving control system?" If not, help?! If so, is the difference related to the line I spoke of above or is it something else?

Carpe Diem
Mark Olson

Date: Tue Jan 28, 1992 5:54 pm PST
Subject: language issues

Re Bruce Nevin (Mon 11L46:52)

>How would you, Bill, go directly to perceptions without expansions from
>
>B: Am I here?
>A: I think you're over there.

An idea developed by G. Fauconnier (in a book `Mental Spaces;, 1985) is perhaps relevant here. It is that when you have a (contextually determined) function from a domain A to a domain B, you can use a member of A to refer to a member of B (subject to various limitations, as discussed in the book).

Here the function is from people to the places that they are supposed to be working, so `Am I here' unpacks to `Is the place where I am supposed to be working here'. I certainly would want to be thoroughly noncommital about the extent to which the unpacking is specifically `linguistic'.  Language is a good way to describe the unpacking is done, but I am puzzled about the wrangling over the `circle of words' - presumably what we are trying to do is devise models that work, and the verbal decompositions are useful aids to this.

>UG folks claim that the brain
>is structured in such a way that language turns out thus and so. They
>seem not to be interested in the question of how this might have come
>about through evolution.

Not entirely - there is a key article in Brain & Behavioral Sciences (1990) by Stephen Pinker and somebody discussing how UG could have evolved. It is an important point that evolution of UG seemed very problematical in the days of the transformational grammatical monolith, but takes on a rather different aspect w.r.t. contemporary generative theories, which are highly `modular' (as discussed by M. Gopnik in a response in the B&BS issue).

Avery Andrews

Date: Tue Jan 28, 1992 8:01 pm PST
Subject: A Hierarchical Controller

[From: Ray Allis 920128]

(I found this on usenet, thought some of you might enjoy.)

----- Begin Included Message -----

Approved: rick@cs.arizona.edu
Status: R

[Dr. David Kahaner is a numerical analyst on sabbatical to the Office of Naval Research-Asia (ONR Asia) in Tokyo from NIST. The following is the professional opinion of David Kahaner and in no way has the blessing of the US Government or any agency of it. All information is dated and of limited life time. This disclaimer should be noted on ANY attribution.]

[Copies of previous reports written by Kahaner can be obtained from host cs.arizona.edu using anonymous FTP.]

To: Distribution
~From: David K. Kahaner, ONR Asia [kahaner@xroads.cc.u-tokyo.ac.jp]
Re: Unmanned helicopter with fuzzy controller, update
24 Jan 1992
This file is named "helicopt.92"

ABSTRACT. Further details about Tokyo Institute of Technology's unmanned helicopter with fuzzy controller.

Last summer I wrote about helicopter flight control based on a fuzzy logic developed by M. Sugeno at the Tokyo Institute of Technology. See "helicopt", 7 August 1991. Since then Professor Sugeno has sent me some additional written material as well as several striking photos of the model helicopter in operation.

The project is to develop an unmanned helicopter for operation over water which would respond to simple voice controls such as "hover", "land", "fly straight", "turn left", etc.

The current system has thirteen measured inputs, three angles of rotation--roll, pitch, and yaw, three angular velocities, velocities and accelerations along three axes, and altitude. Two additional state variables (horizontal position) cannot be measured. Sensors are made by Tokimec-- angles, angular velocities and accelerations are measured by TMOS-1000, the altitude by an ultrasonic wave sensor, and velocities by a microwave Doppler interference sensor. Global positioning information has not yet been incorporated, but plans are to include this when it becomes available here in Japan.

There are four outputs which are the four control inputs to the helicopter such as elevator adjustment for forward-backward movement, aileron adjustment for left-right movement, throttle adjustment for up-down movement, and rudder adjustment for nose direction.

The helicopter used in Sugeno's experiments is a Yamaha R-50, about 3.5meters long with a 90kg payload and a 98cc engine.

The controller is based on fuzzy logic and is installed in a 16bit microprocessor with fuzzy inference engine built by Omron.

The most interesting aspect of the system is its hierarchical structure. At the top level is a navigator system which receives operator's instructions (hover, land, etc.) along with the present flight states of the helicopter from the sensors. The navigator provides as output both trim information (an equilibrium position of the helicopter's attitude) and the desired values of the control inputs. Both sets of information are input to the lower level, the stabilizer level, which is a servo system with the trim as its reference signal. The stabilizer consists of blocks corresponding to its flight modes, e.g., a sideways flight block, forward flight block, hover flight block, etc. Each block consists of four modules corresponding to the four control inputs (elevator, aileron, rudder, and throttle).

This kind of hierarchical and modular structure simplifies the acquisition of control rules as well as the controller design. It is also natural as pilots recognize that his own ability at control is also hierarchical. To improve stability and the control dynamics, there is also built in feed-forward control action.

The navigator has two subsystems, a major one outputs standard trim and a minor one compensates it. The major subsystem has rules such as "If flight mode is HOVER and flight state is FORWARD then standard trim is sTRIM." The latter is a four-tuple (P,E,R,A) which are the reference signals of the pitch angle, roll angle and the offsets of the elevator and aileron. Since true trim can deviate from the standard, it must be compensated, given by TRIM=sTRIM+cTRIM. cTRIM=pcTRIM+dcTRIM, where pcTRIM is the prior cTRIM and dcTRIM is output of the minor system. The minor subsystem has rules as follows.

```
        If X is + and dX is + then dcTRIM is v1
        If X is + and dx is 0 then dcTRIM is v2
          .
          .
        IF Y is - and dY is - then dcTRIM is v9
```

(Here X, Y are longitudinal and lateral velocity, dX, dY are corresponding accelerations.)

The control rules in the stabilizer were obtained firstly by interviewing pilots and then tested and modified experimentally with a helicopter flight simulator at Kawasaki Heavy Industries. In total 54 rules were developed based on pilots knowledge. Such knowledge was formulated in statements like

  "If the body pitches then control the elevator in reverse"
  "If the body moves sideways, the control the aileron in reverse"

Each developed rule has two inputs and one output. Inputs are linguistic variable (positive, negative, zero). For example some elevator rules are

```
        If EP is + and dP is + then Eo=a1
        IF EP is + and dP is ) then Eo=a2
```

etc. Here EP=Pr-P (previous minus current pitch angle), dP is pitch angular velocity and Eo is elevator output of the stabilizer.

Currently, the helicopter can support the following flight modes.

- Hovering
- Hovering turn
- Forward/rearward flight
- Left/rightward flight
- Stop

Other flight modes are in progress.

- Takeoff
- Acceleration/deceleration
- Left/right turn

Climb/descent
Landing

----- End Included Message -----
9201E CSGnet

Date: Wed Jan 29, 1992 12:57 am PST
Subject: Discrete variables; language

[From Bill Powers (920129.0100)]

Oded Maler (920128) --

>I wonder how (and where) the fact that the signals are analog (dense
>time, dense value) is essential to your claim. That is, wouldn't be
>true that a discrete data-flow architecture will give the same insights
>concerning representation as relations among signals?

No. The discrete data-flow architecture almost invariably leads one to think that the control loop consists of a sequence of processes that take place one at a time: first input, then comparison, then output, then wait for the input to be modified. In the real system there are signals entering and leaving all functions at exactly the same moment, and there are effects in various stages of propagation through the environment between output and input, together with disturbances influencing them, also at the same time.

This applies to the hierarchy, too. There are perceptual signals varying at one level, and at the same time perceptual signals of a higher level that are varying as functions of the lower-level signals. While errors are being corrected at one level, the reference signals from higher levels are being altered, changing the error signals even before they have come to equilibrium. There is no graceful way I know of to represent these relationships in terms of discrete events. Doing it on a digital computer requires constant awareness of the fact that the variables are not continuous in the computer, but jump from one value to another. This royally screws up certain computations, especially the taking of time derivatives.

The discrete data-flow architecture is an artifact of the digital computer revolution, which, 40 or 50 years ago, was erroneously connected to the way neurons operate (giving each impulse the significance of a logical variable). That picture of neural operation has proven inadequate but its implications for models of nervous-system function live on, although disconnected from their justification. The digital computer (von Neumann style) is the ultimate stimulus-response machine: its outputs change only when told to do so, and there is no need for feedback to verify the output because the whole point of digital design is to achieve total reliability of function. There is nothing else in the universe that works the way a digital computer does. Even digital computers, physically, don't work the way digital computers do in the abstract. The basic components of digital computers are analog devices; when a signal in a computer changes from a 1 to a 0, it passes through a continuous series of values starting at a voltage somewhere between 2.4 and 5 volts (TTL logic, anyway) and eventually -- in a few nanoseconds -- reaching something less that 0.8 volts. At the nitty-gritty level of digital design, the actual voltages make a difference, and the transition times and delays are carefully noted because they can create ambiguities and missed signals if not properly compensated. The digital computer was an extremely ingenious and useful invention. But it is not a model of an organism.

Bruce Nevin (920128.0553) --

Don't get too impatient with me. Part of my problem is a mind that works like a steel colander. I do get confused about what we agree on and what we still differ about. And your long discourses on the details of linguistics flatter my ability to hold all the details and relationships in mind.

In your latest example, which I do recall because it's in the alternate window of this word-processor, included

>B: Am I here today?
>A: I think you're over there,

which evolved into.

>B: Am I doing things here today?
>A: I think you're doing things over there today.
>
>Assuming a hierarchy of authority and assignment of tasks in a job
>situation, there is one further step:
>
>B: Am I supposed to be (doing things/working) here today?
>A: I think you're supposed to be (doing things/working) there today.

Then you ask,

>How would you, Bill, go directly to perceptions without expansions from
>
>B: Am I here?
>A: I think you're over there.

I would expand, too, but I would not expand into words first. The first image for "Am I here?" is silly: the guy doesn't recognize where he is. Is he afraid he's hallucinating? No. So I fish around for an embedding circumstance: he's pointing to a place on a map; he's an actor rehearsing on a stage, talking to a director. Then I get it: he's asking about what his goal should be, what his relationship to something should be. Now I know which way to expand verbally. You know another embedding circumstance I didn't think of: he works on a schedule (image of a wall calendar) that takes him back and forth between different locations, and has forgotten which place is today's place.

This doesn't come to me in words, but in pictures and relationships and flittings back and forth. For me, the expansion goes from the initial phrase to its apparent meanings, then to an evaluation of that meaning (and in this case a rejection of it), then to a search for more perceptions that will suggest different ways to hear the phrase, and finally to a perceptual context that makes sense. Then I can describe the perceptual result in many ways that remove the question marks.

I feel a difference between the way you and I operate here -- you seem to be claiming that the sequence goes from the reduced form to the expanded form *of the sentences*, and then finally to the explicit perceptual meanings. I think that I elaborate the perceptions into an acceptable expansion, and then convert to an expanded verbal form that describes the elements I've introduced to make sense of the original meanings.

This IS a difference, isn't it?

RE: groups and plurals.

>Please be careful. The words "a group" were *overtly* present in the
>phrase "a group of windows."

My recollection is that Avery's example was "a wall of windows." Wasn't "group" added as part of the expansion of "windows"?

>I am suggesting that the direct nonverbal perception is that there is
>one window and *there* is one window and *there* is one window and so on
>... This perception correlates with "a window and a window and a window
>. . ." and it also correlates with "windows". Since it does correlate
>with both, I am suggesting that the act of correlating words with
>perceptions use the most explicit form of the words.

We ought to be able to get together on this. Perhaps the problem here lies in what you mean by "most explicit." To me, that means lower levels of perception. The reduced form, which is heard first, elicits only a few explicit perceptions: at the configuration, level, a window and a wall. At a higher level, a sense of multiplicity or (as you say) repetitiveness (just because of the formally plural form). At the relationship level, suggested by the "wall of" part, the window and the wall in relationship. With a different starting point, perhaps the images would be different (as in "sands of Iwo Jima") and the imagined details different.

Then imagination comes into play to fill in the details: not one window, but a window and a window and a window -- the new configuration perceptions being added, then being described. Now the "wall of windows" relationship becomes clearer; we imagine the windows distributed all over the wall (probably arranging them in rows and columns, imagining more than was said). With multiple windows now available at the configuration level, we can scan over them and derive the sense of repetition more explicitly. Or perhaps what we imagine is scanning over the wall while the sense of windowness repeats. I'm just trying to get the levels into this, not to claim my details are right.

This is not far from what you seem to suggest, but it puts the "explicitness" at a lower level, not a higher one. Is that acceptable?

RE: TTTTTTTTTT...

>But one must perceive at least one T to get the T perception and one
>must perceive its likeness to at least one other to get the perception
>of repetition of the T perception.

That would be a LOGICAL way to deduce repetition, but my hunch is that the perception of repetition is more direct than that. When the drum goes boom, boom, boom, I don't think we stop and think that the first boom was just like the second, and the second like the third, and so on, and only then conclude that the booms are repeating. I think this is a dimension of transition perception, or perhaps event perception. If there is a rate-of-repetition perceiver, all it needs is an input signal that repeats. That sort of input function is easy to design: a leaky integrator will serve. It doesn't need any ability to report that the inputs were "alike." It doesn't even have to know what its inputs mean, at a lower level, or even whether each repetition of the input came from the same source.

This brings up another reason I appear to behave stupidly in this discussion. The perceptual functions I try to describe don't work by symbolic reasoning. They work by handling signals in a certain way, so that their outputs have a certain significance in relationship to their inputs: e.g., the output signal indicates "repetition" of the input signals. There are many functions of this sort that we can characterize in words, but they aren't made to work by verbal reasoning. They work because of the kind of neural computation that goes on inside them.

>If you say
>all the elm trees in the U.S. died do you have a perception of the
>aggregate of those trees?

Yes. I imagine dilapidated elms lining street after street, everywhere. They aren't really elms, but just scraggly trees with a sort of elmness about them. The picture is somewhat screwed up because it includes ONLY elms, as if you had said that all the trees in the U.S. are elms, and they all died. It's harder to picture lots of different kinds of trees with only some of them here and there being dead elms, but ALL the elms being dead, all over the map.

If I couldn't imagine something to go with the sentence, I'd probably consider it meaningless. What I do manage to imagine is what the sentence means to me. Unless I put on my logic hat, in which case the elms are just variables in a proposition, and it doesn't matter how they look or what "dead" means. I would probably express doubt about the proposition.

Later ...

>When will you notice that I have been agreeing with this?

Frustrating, isn't it? I'll notice when I understand exactly what you're agreeing with. Doubts arise when you say things like

>When a mass or aggregate noun like sand is pluralized, it is really
>another noun which has been zeroed that was plural,

I can't help wondering what you mean by "really." Are we talking about a process that we can perceive happening? I can understand that

        all the sands <-- all the kinds of sand

satisfies the demands of the theory, but what about the demands of observation? In fact, the above isn't even what I mean, perceptually, by "all the sands." My image isn't of different KINDS of sand. It's just a picture of sand here, sand there, sand everywhere. Same kind of sand. No "kind" in there, or any other zeroed noun that I'm aware of, at any time. Doesn't this count against the theory?

All of which brings up a touchy subject. Your description of Harris' approach sets off alarm bells in me. I hope they're unjustified.

>It works like so: take a set of sentences that differ only with respect
>to one word. Rank the sentences in the set as to acceptability, ...

This tells me immediately that some people will rank the sentences in accord with what majority does, and some will not. This removes "acceptability" from the realm of scientific facts, as far as I'm concerned. I assume that not all persons will rank all sentences in the same order of acceptability. If this ranking is at the basis of language, then we have to have a theory not only of the most usual ranking, but of the deviant rankings as well. Either that, or we have to say that the laws of language are different in different people, in which case there really aren't any laws of language.

Another step is "Pick two sentences from the set that differ markedly in the judgements made of their acceptability or likelihood." (etc)

Now the statistics become even fuzzier, unless by "differ markedly" you mean "ALWAYS differ, regardless of who is doing the judging." We are going to add another statistical discrimination on top of the one that initially established acceptability. The correlations go down some more.

Then we repeat this with a second set of sentences, and add a judgment as to whether the sentences differ by an operator + arguments, a zeroing, and so forth. And finally, test the sentences for whether they have "the same relative acceptability difference ...", so now we are looking at the difference between the means of measures with a statistical spread, and no doubt some overlap.

Do you see my problem? By the time all these successive statistical measures have been combined and compared, there can't be very much precision left in any test of the existence of "operators" and "arguments" and "zeroings" as meaningful units.

The idea of "zeroing" actual words that exist and then are removed seems to me completely untestable. Worse, because you can't verify in any actual example of speech that any particular word was in fact present before "reduction," you're free to hypothecate any kind of word that will preserve the theory, so the theory becomes unfalsifiable. I wouldn't mind if the zeroings were only temporarily unobservable because we haven't thought of a way to observe them yet. But it seems to me that the zeroed terms are IN PRINCIPLE unobservable and will remain so. If this is true, then the zeroed words are simply wild cards that make all hands winners.

I guess I've had these objections in mind for some time, but didn't want to be the bad guy or an ingrate by bringing them up. They seem pretty serious objections to me. I hope they're spurious.

And that finishes me off for the day, too. One last thought. Considering the hundreds of thousands of words that have passed between us, and the difficulties that remain, and a number of other experiences too tedious to enumerate, I am even more firmly convinced that language is overrated as a means of communicating.

Yawn.

Best to all,

Bill P.

Date: Wed Jan 29, 1992 10:53 am PST
Subject: An Appeal to Support Researchers in Flanders

Dear Colleague,

We are spreading this message in order to ask colleagues abroad to help the actions of Flemish researchers against drastically reduced funding. The funding for basic research in Belgium, and especially in its northern region of Flanders, has been getting worse and worse during the last years, and has become especially dramatic lately. Since the group of researchers in Flanders is small and not very well organized as yet, and since it has no political power, we have decided to appeal to friends and colleague researchers abroad to help us in our protest against these measures, by sending us letters of support.

In Belgium the main institution that provides employement and grants for researchers is the "National Fund for Scientific Research" (NFWO/FNRS). There is a wide consensus within the Belgian academic world about the outstanding quality of this institution, as well with respect to its administrative functioning, as to the high scientific level of its researchers. Hence, the recent measures to cut funding for the NFWO cannot be justified by any criticism of the institution.

As you may know, Belgium is gradually evolving towards a federal structure with two main independent regions of about 5 million inhabitants each: the French-speaking Wallonia, and the Flemish-speaking Flanders. This process has recently led to a split-up of the National Fund into a French and a Flemish part. Presently the French part is being strenghtened, financially and structurally. This was to be expected since Belgium as a whole was spending much less money on basic research than comparable countries. According to the OECD, less than 0.5 % of GNP is spent for research in Belgium, compared to around 1 % in the neighbouring France, Holland, Germany and UK (and even more in Japan and the USA). Therefore the effort of Wallonia to increase funding can be seen as an attempt to adapt itself to the European level.

It came as a complete surprise and shock then, when the Flemish government decided to cut funding for the Flemish part of the NFWO, and that to such an extent that its internal function must collapse. In the short term, the measure implies the complete disappearance of long-term research contracts, the diminishing of short term contracts, and a radical reduction of about 80 % in the funding for general working costs and equipment of major research centers. In the long term the only effect can be the factual disappearance of basic research in Flanders.

Practically this means that young researchers, however bright they are, will be unable to continue their career in academic research. The situation in the universities is not much better, and there are no ways of escape there either. In the recent past (10 to 20 years ago) relatively many people have received a permanent research contract because of university expansion. This means that they will stay there until their retirement. In these times of budget cuts, however, there is practically no money left to create new positions, and hence the younger generation simply does not have any outlook for continuing research, unless they emigrate. And this in spite of the fact that the average level of research in Belgium is quite high, as testified by famous research centers such as the one created by the Belgian Nobel-prize winner Ilya Prigogine.

Though the Belgian and Flemish governments do have important budget deficits, that is not a sufficient argument to cut spending on research. First, as outlined above, the funding in Belgium is already much below what could be expected from a highly developed country. Second, what politicians do not understand is that the development of knowledge is the single most important factor determining economical and societal development, and as we are moving towards a post-industrial information society that factor is gaining in importance with every day that passes. Cutting funding on R&D can only keep down future economic growth and, hence, income for the state. Third, in spite of the budget deficit, Belgium (and especially Flanders) is still one of the richest regions in the world, with an economy that has been doing quite well recently and a per capita income higher than those of France or the UK, and only slightly below that of Germany.

The reasons for saving money in research rather than in other domains are to be found in short-sightedness, lack of understanding of what research really means, and the lack of political power of researchers as a group. When train engineers, factory workers, or nurses are unhappy, they go on strike, and everybody is immediately alarmed. If researchers would go one strike, nobody would notice, unless many years later. But then it would be too late to repair the damage. So we are looking for other ways to attract the attention of the public and the politicians to our grievances. (For example, on February 6, for the first time in Belgian history there will be a public demonstration of researchers in Brussels, as a protest against the reduced funding.)

That is the reason we are making an appeal to international solidarity. We ask all our colleague researchers to write a letter in which they express their solidarity with our movement, stressing the importance of an adequate funding for basic research, and protesting against the cutting down of the NFWO. If you have had personal contacts with researchers employed by the NFWO, we would ask you to emphasize the quality of their work, which was made possible by the NFWO. (As some of you know, I am myself working for the NFWO since many years, on short-term contracts that are to be renewed with much difficulty every two years. I had hoped to get a long-term contract, but after the recent measures that has become impossible, and now I can only hope for a last two year renewal).

We would ask you to distribute the present message to all people you know who might be interested, so that an as a large as possible public of researchers is reached. We would also be interested in suggestions on how we might continue our actions, and convince politicians about the importance of research funding.

The letters of support (and possible further reactions) are to be sent to "Focus Research. Belgian Association for the Advancement of Science", a recently founded group representing all researchers, which will distribute them to the press and to politicians. The address is:

Focus Research
Triomflaan 63
B-1160 Brussels
Belgium

Phone: + 32 - 2 - 647 77 13
Fax: + 32 - 2 - 647 31 57

We thank you in advance for your support.

Sincerely,

Francis Heylighen

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Date: Wed Jan 29, 1992 11:42 am PST
Subject: Re: Discrete variables; language

[Martin Taylor 920129 14:00]
(Bill Powers 920129 0100)
>
>
>This doesn't come to me in words, but in pictures and relationships and
>flittings back and forth. For me, the expansion goes from the initial
>phrase to its apparent meanings, then to an evaluation of that meaning
>(and in this case a rejection of it), then to a search for more
>perceptions that will suggest different ways to hear the phrase, and
>finally to a perceptual context that makes sense. Then I can describe the
>perceptual result in many ways that remove the question marks.
>
>I feel a difference between the way you and I operate here -- you seem to
>be claiming that the sequence goes from the reduced form to the expanded
>form *of the sentences*, and then finally to the explicit perceptual
>meanings. I think that I elaborate the perceptions into an acceptable
>expansion, and then convert to an expanded verbal form that describes the
>elements I've introduced to make sense of the original meanings.
>
>This IS a difference, isn't it?
>
In the despised realm of experimental psychology, there IS a difference--
a difference between two kinds of people. There are those who do as Bill
does (and as I do); they get things into imagic mode and work there. And
there are those who work as Bruce seems to; they get things into linguistic
form or something very like it, and work there. The linguistic types
seem to be more numerous than the imagic types, and there are a large
number of people that are hard to classify as either.

I overstate the evidence, deliberately. But both Bruce and Bill should be
aware of the possibility that each is right, but for a different kind of
person. Sometimes it is hard for a linguistic type to conceive of the
possibility that some people think in a quite different way. Personally,
it has taken me a long time to truly believe that there are people who
think in some form that has a lot in common with language, but I have
come to accept it, as an intellectual fact, much as I have come to accept
that there exist people who truly believe in the existence of a God. I

can't feel the truth of it, but as a fact to be used in rationalizing things, I accept it.

Martin

Ref: MacLeod, C. M., Hunt, E.B., and Mathews, N. Individual differences in the verification of sentence-picture relationships. J. Verbal Learning and Verbal Behavior, 1978, 17, 493-507.

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Date: Wed Jan 29, 1992 1:03 pm PST
Subject: language and nonlanguage

[From: Bruce Nevin (Wed 920129 10:41:48)]

Some time ago, I had proposed that the hierarchical control of words qua words (linguistic perceptions) runs in parallel with the hierarchical control of other (nonlinguistic) perceptions, and had wondered about the mechanisms for associating the one with the other:

```
(1)             Non-Language        Language

Program                o                 o       word dependencies, reductions
                       |                 |
Sequence               o                 o       word sequences
                       |                 |
Category               o  <--\           o       word classes
                       |       \         |
Relationship           o         \       o
                       |           \     |
Event                  o            --> o       words, (semi)syllables ...
                       |                 |
Transition             o                 o
                       |                 |
Configuration          o                 o       phonemic segments, ...
                       |                 |
Sensation              o                 o
                       |                 |
Intensity              o                 o
                       |                 |
```

(The arrow here shows words correlated with category perceptions. Semisyllables, syllables, and other phonotactic entities are thought to be on the event level too, but not correlated with category perceptions; that is, they are not meaningful.)

Then I proposed that category perception is not a level but rather a property of the association between the two lines of hierarchical control, verbal and nonverbal:

```
(2)             Non-Language            Language

Program                o   [Categori-     o
                       |      zation]     |
Sequence               o  <--\            o
                       |       \          |
```

```
Relationship           o  <--\   \        o
                       |      \   \       |
Event                  o  <------------>  o       words, (semi)syllables ...
                       |      / / / /     |
Transition             o  <--/  | | |     o
                       |      /  | |      |
Configuration          o  <--/   /  |     o       phonemic segments, ...
                       |      /   /       |
Sensation              o  <--/   /        o
                       |      /           |
Intensity              o  <--/            o
                       |                  |
```

On this view, categorization associates with each categorized perception at least one particular other perception of the same kind or level. This one (not shown above) is another nonverbal perception. It is stripped of detail. (I think this means that lower-level perceptions are not involved, so it is akin to the imagination loop. If the detail continues absent, we may fill it in out of memory. I have some ideas about how this might be done, but don't want to clutter the thread.)

On the view shown in (1), a category perception must also have this likeness to the perception categorized. A sensation category perception like the category perception (not the word) "brown" must be of the same kind as a particular color perception that we might call brown if we chose. The configuration category perception that we call "leg" is of the same kind as the perception of a particular leg. That is to say, the configuration-category "leg" differs from the sensation-category "brown" in just the same way that the configuration perception of that leg differs from the sensation perception of its brown color.

In my experience, each category perception is a simplified, stereotyped, or schematic exemplar of the diverse acceptable inputs. These exemplars differ from one another in just the ways that the particular perceptions differ which they exemplify. The exemplar for "leg" is in fact a representative leg-configuration perception and the exemplar for "brown" is in fact a representative brown-sensation perception.

Perhaps you will say that these distinctions of kind between e.g. sensation categories and configuration categories reside on the lower levels, in the particular sensation perceptions and the particular configuration perceptions that can satisfy the input requirements of their respective category-recognizers. This works only if our various subjective reports of category perceptions (various between e.g. you and me, and at different times for me depending on what I had for breakfast) are illusions manufactured by imagination out of memory of particular perceptions below the category level. Category perceptions themselves are then unintelligible to awareness, and we use these more "concrete" exemplars or surrogates to make them intelligible. The fact that not everyone sees stick figures is suggestive. But this is slippery ground, the same argument could suggest that my stick figures are hidden behind more detailed exemplars flung up by my imagination to cloth their nakedness. Point is: what difference does it make?

I am digressing too much.

You proposed that a category-level ECS accepts EITHER lower-level nonverbal perceptiual signals OR a signal from a word recognizer as input (or both). On this view, categorization brings the two lines of control together in one:

```
(3)             Non-Language        Language

Program                         o
                                |
Sequence                        o
                                |
Category                   __-- o
                          /   / | \
Relationship            o   /   |   \
                        | /     |     \
Event                   o       |       o       (semi)syllables, words
                        |       |       |
Transition              o-------+       o
                        |       |       |
Configuration           o-------+       o       phonemic segments, ...
                        |       |       |
Sensation               o-------+       o
                        |               |
Intensity               o               o
                        |               |
```

This disgram attempts to represent my understanding of the effect of your proposal. Have I misunderstood you?

Given this, I can understand why you would want the processes of arranging the relations of words in utterances to be a mere reflection of the relations and dependencies among the nonverbal perceptions to which they correspond.

I on the contrary claim that there are higher levels of control of words. There are word classes or categories for example, we control for word sequences (surely for idioms and cliches like your "now is the time"), and we control for word dependencies on the program level.

It is confusing that you also speak of program-level control of language in ways that seem to mean syntax as apart from the mirroring of nonverbal perceptions.

These kinds of control are conventional for the language and culture. If I switch to German, or French, or Modern Greek, or Achumawi, with the goal of having another person agree with me about some perceptions (the same goal as I switch languages), more than just vocabulary choice differs. Even between languages relatively quite similar to one another, like English and German, the word order and even the way of designating the perceptions in words (the way of punctuating the perceptual scene) can differ quite markedly. And if I fail to control for conformity to the conventions of the language being used, I increase the difficulty of communication, and decrease the likelihood of reaching agreement. I gave examples of changing the word-order convention of English to that of Achumawi, retaining all other features of the English utterances. No one ever responded to that, but I believe it appeared quite unintelligible. Using English word order in Achumawi would be even worse, given that many things that are separate words for us are reduced to affixes of the (very complex) verb word in Achumawi.

Now consider again:

Am I here? <-- Am I doing things here?

The phrase "doing things" is indefinite. To what sort of category perception does it correlate? If you thought you had trouble with all the elm trees in America, how could you possibly have a perceptual image for "doing things"? In my case, I get an image of moving one's hands and arms. But that might be different after I've gone to a meeting that is coming up.

I don't think there is a category perception there. I think there are words there which do not specify any particular perception. They have the syntactic role of an operator (and its arguments other than the "I" which is present in the sentence), which is also an argument of "here." This syntactic role defines a slot to be filled. I think that we call upon perceptual input and memory and imagination for candidate perceptions and associated with them candidate operators (and their arguments other than "I") to fill in the slot. The structure of the language here provides a scaffold or framework on which to hang and organize these nonverbal perceptions.

"Doing things" provides so little information that you might as well say nothing. By convention in the language (and in any other language that I know of) you can say nothing in place of an indefinite, leaving only the void in the argument requirement of "here" and the void in the requirement that "I" be under some operator (with appropriate argument requirement). Harris calls this low-information zeroing.

In the zeroed form, these unfulfilled word dependencies can specify the indefinite "doing things" and no more. You can't get "working" or "installing cable" or anything else more specific from the language of this conversation alone. In either the expanded "doing things" form or the zeroed form "Am I here?," the hearer is presented with an empty slot to fill. The slot and the particular shape of its emptiness is defined by the language of the utterance. Because of the correspondence of language with nonverbal perceptions, the slot defined linguistically is also defined for nonverbal perception. The nonverbal perceptions with which to fill it must correlate with words that can fill the linguistic slot. Language is more structured than nonverbal experience is. We use the inherited, learned, conventionalized structures of language to help organize our experience, especially but not exclusively for purposes of cooperative action.

>The first
>image for "Am I here?" is silly: the guy doesn't recognize where he is.
>Is he afraid he's hallucinating?

This interpretation was rejected so quickly that it didn't really come to my awareness until I thought about the dialog as an example of reduction. It was waiting in the wings, ready if he had gone on with something like "I've been on the road so long I can't tell where I am any more. Am I really here?"

>I feel a difference between the way you and I operate here -- you seem to
>be claiming that the sequence goes from the reduced form to the expanded
>form *of the sentences*, and then finally to the explicit perceptual
>meanings. I think that I elaborate the perceptions into an acceptable

>expansion, and then convert to an expanded verbal form that describes the
>elements I've introduced to make sense of the original meanings.

I think that both go on in parallel. The dependency requirements for the words in the language provide a structure that the hearer uses to guide imagination and memory in fleshing out the immediate perceptual input.

>This IS a difference, isn't it?

My experience is that there are temperamental differences in how much one attends to language and how much one attends to nonverbal perception in the course of using language. A preference for language may correlate with being literal-minded and punctilious, and a preference against may correlate with being more empathic and reading between the lines. Just as there are preferences for different sensory modalities in communication, choice of imagery for metaphor, etc. But what I suspect is going on here is a studied commitment on your part to getting at the perceptions behind language and seeing language as a deceptive veil. My commitment is to understanding how and why language is as it is. The complementarity of our perspectives will work better when I am more cautious about the capacity of words to suggest what the perceptions ought to be or must be (attending to their suggestions cooked up by an ever-willing memory and imagination rather than to the actual perceptions), and when you are mindful that the depth of the deception goes far beyond the mere correlation of individual words with perceptions.

I believe that you are underestimating the amount of hierarchical control of language perceptions is involved in your use of language. I know that this control is going on in you because I am reading very articulate prose from you that conforms to conventions that are current for educated middle class American English, and not conventions for any other language or any earlier stage in the history of English or any other markedly different dialect of English (such as that of Jamaica or India), and certainly not a lack of convention.

Gack! Look at the time!

Bruce
bn@bbn.com

PS--came back and found this still waiting at the prompt for me to send.
Once more, dear friends, into the mailer queue!

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Date: Wed Jan 29, 1992 2:07 pm PST
Subject: Re: language and nonlanguage

[Martin Taylor 920129 14:30]
(Bruce Nevin 920129 10:41)

To expand on what I just posted in response to Bill:

Bruce says:

>Now consider again:
>
>   Am I here? <-- Am I doing things here?
>
>The phrase "doing things" is indefinite. To what sort of category
>perception does it correlate? If you thought you had trouble with all
>the elm trees in America, how could you possibly have a perceptual image
>for "doing things"?

Speaking personally, it is when you ask me to describe in words the thing/process/percept that I would call "image" that things become difficult. I have no problem at all with the mental operation that I casually think of as imaging "doing things", but when you ask me "what is the picture" I realize that what I see is not a picture that could be painted or animated, but something else. It is not a composite of lots of things being done, though it partakes of that effect (I can imagine that, too). I am clear as to the performance of iamging "doing things" but at a loss as to how to put it into words other than as "imaging 'doing things'", and that doesn't help much. Nevertheless, I can empathise much more closely with Bill's description of the mental processes he goes through than I can with Bruce's.

I don't agree with Bruce's wording when he says:

>My experience is that there are temperamental differences in how much
>one attends to language and how much one attends to nonverbal perception
>in the course of using language. A preference for language may
>correlate with being literal-minded and punctilious, and a preference
>against may correlate with being more empathic and reading between the
>lines.

What I disagree with is the words "attends to". If he substituted "uses internally" or some such, I would have less of a problem. Also, being an imager who has been accused of being "literal-minded and punctilious" I'm not too sure of the correlation. When I work WITH language, as an object of study, I do it an imagic mode. I see the words working with each other, as it were in interacting clouds of force (though as with most of these descriptions of images, the words fail to convey even the flavour of the rich experience). I think I USE images in ATENDING TO language, and I think that is what Bill was also describing.

Martin

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Date: Wed Jan 29, 1992 3:48 pm PST
Subject: ways of thinking

I just meant to open the topic of temperamental differences, which I see you also did, Martin. I did not intend to attribute the particular differences that I said "may correlate" with them to you and me, Bill, and in fact I think it has more to do with commitments about research as I said. I have described my nonverbal thinking processes before. Constraining those processes to communicable form with language is not always a grateful task.
For any of us, I suspect.

Gotta run,   Bruce   bn@bbn.com

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Date: Wed Jan 29, 1992 6:12 pm PST
Subject: language (cont.)

[from Joel Judd]

Avery (920129)

Would you summarize the evolutionary arguments of Pinker's BBS article so I can see if it's worth a return trip to the Biology library? I think I read it a couple of months ago but wasn't impressed. I'm more impressed with Campbell and Bickhard's arguments in their 1986 book to the effect that the same problems which prevent UG from being an _ontogenetic_ account of language also prevent an evolutionary account.

Bruce (920129) and Bill,

I notice that your hierarchy has the language counterpart of RELATIONSHIP still missing. I've had to suppose what that might be for the dissertation, but I'd be interested in seeing what you think it corresponds to. Your 3rd diagram implies that it's still non-linguistic.

Wouldn't some developmental discussion help out in all this? Surely there's language acq. evidence to be reinterpreted. I'm trying to catch up with what I can, but SLA training is still under the impression that children and adults have too many differences for one to study both. One thing I think will be found is that children labor to develop linguistic perceptions for their non-linguistic experience. However, their experience is not as sophisticated as that of adults, so that by the time experience provides for development of higher levels of perception, the language has become THE means of thinking and talking about such levels. In consequence, one would find that the environment appears to influence the language early on, but language appears to influence perceptions more as time passes. How's that for straddling the fence? In addition, Bruce says:

>In my experience, each category perception is a simplified, stereotyped, or
 schematic exemplar of >the diverse acceptable inputs.

Sounds almost like MacWhinney's account of our conceps of things like 'verb,' 'noun' etc. and Martin's comments. Not only are they sort of amorphous as far as not referring to a specific perception, but they require _interaction_ with the environment in order to exist at all.

Misc. from the home front: I got back from teaching last night to find all three children on the top bunk. As I approached the room, our boy who turned 3 on the 23rd said, "Daddy, tickle us." I obliged, then took him away so his sisters could sleep. On the way down the hall he said "I want some jello" or something like that. Then we stopped at the bathroom to bug Mom. He stuck his head in the door and his Mom asked him something. Then I told him, "Tell Mom what you asked me," intending for him to repeat the request for tickles. Immediately I thought "How's he going to know to talk about tickles instead of jello, especially since the latter request was more recent?" But sure enough, he told his Mom, "I said 'Daddy tickle me.'" How did he know what I was expecting him to say? I asked him "Who told you to say that?" (silence) "Huh?" "Me," he answered.

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Date: Wed Jan 29, 1992 7:18 pm PST
Subject: lists

If someone's keeping a list of literature that uses CT themes without doing it properly, F. Adams (1986) "Intention and Intentional Action" Mind & Language 1:281-301, ought to be on it, if it isn't already.

Avery Andrews

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Date: Wed Jan 29, 1992 9:16 pm PST
Subject: Pinker, Evolution of Lg.

Joel Judd:

I won't summarizing the arguments because I don't remember them very well, & actually didn't like it very much myself, in its current form. The point was just that the issue has been thought about, at least a little bit.

But I did like Gopnik's observation that in a modern, `modular' syntactic theory, one doesn't have to say that the whole `language faculty' did or didn't come into existence as an autonomous mental organ. One can think of grammar as a rube-goldberg device cobbled together out of such processing facilities that happened to be available, some of which might have undergone some subsequent optimizations for use in language processing.

I've been looking at Bickhard, but suffer a bit of a culture gap in seeing what he's up to.

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Date: Wed Jan 29, 1992 9:33 pm PST
Subject: the imperfective paradox

Here's a little thought for people with semantic interests. With a `process' type verb, the progressive entails the nonprogressive:

    the water was boiling -> the water boiled

But for `accomplishment' verbs (in the Aristotle-Vendler-Dowty verb classification scheme) this fails, because we can have

     John was driving to New York

without

     John drove to New York

(e.g. `John was driving to New York, but he never got there
   because his car broke down)

This phenomenon is known as the `imperfective paradox'.

Question: can we get the imperfective paradox for uncontrolled accomplishment verbs? My impression is no:

     the water filled the pool in 7 hrs.

    *the water was filling the pool (when Max turned the hose off).

     sediment covered the ship in 5 years

    %sediment was covering the ship when the explorers found it.
      (doesn't mean the ship was getting covered).

If my impression is correct, one might be able to claim that the `imperfective paradox' can only be produced when the state designated by the progressivized verb is the goal being pursued by a person or person-equivalent.

Conventional formal semantics tells very murky and unappealing stories about the imperfective paradox, while in PCT the goal states involved are first-class citizens of the real world, as it were. So it is perhaps as if the semantics of ordinary language were implicitly assuming something like PCT.

Avery Andrews

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Date: Thu Jan 30, 1992 5:25 am PST
Subject: correlating relationships

[From: Bruce Nevin (Thu 920130 07:25:50)]

Joel Judd (Wed, 29 Jan 1992 16:53:40) --

>I notice that your hierarchy has the language counterpart of RELATIONSHIP
>still missing. I've had to suppose what that might be for the dissertation,
>but I'd be interested in seeing what you think it corresponds to. Your 3rd
>diagram implies that it's still non-linguistic.

Well, to amalgamate Blake with a familiar proverb (and two of Bateson's favored quotes), if a fool shall persist in rushing in where angels fear to tread, he shall (eventually) become wise. Such is my brash hope.

I left out a number of things to avoid complexity, and that in particular to avoid what might be a can of worms.

Bill quite rightly took me to task for suggesting that the relation of repetition among words correlated with the repetition of their referents, in the same semiotic way that the words correlate with their referents (nonverbal perceptions). The two correlations are of different types. The second is arbitrary and conventional, the first is iconic. His implicit claim was that the reduction to -s means the repetition of the perception of a window in the same way that "window" means the perception of a window. Now, repetition may not be a relation perception, but I left it out anyway to leave the issue moot at the moment and to simplify the discussion at hand.

I do think that there is something of the kind going on in language, and that the evidence for iconicity of this sort is rather compelling. I refer to the use of overt repetition and reduplication of nouns to signify plurality in many languages, and of operator words to signify an intensification. The latter we have in English: It grew and grew. Alice fell and fell. The Mad Hatter talked and talked. Her face grew redder and redder (more and more red). Curiouser and curiouser. (You can tell what my 4-year-old is asking me to read her these days.)

(Tell your 3-year-old that I think 1/23 is a great birthday, and--in true Aquarian spirit--he is welcome to share it with me. Of course (imagine my saying this archly) mine goes 01/23/45 . . . but you don't have to tell him that.)

My guess is he didn't mention the jello because you had already turned him down and so it was unlikely you would *encourage* him to go around you to mom. Or, if you had already agreed (less likely) then he would have no need to ask her. So it must be that other asking, associated with the same emotional tone of fun as now heard in your voice "Tell Mom what you asked me." I'm of course reading a lot between the lines--as we all do and must.

Your suggestions about acquisition seem like good ones to me. I need to learn more about histories of language acquisition.

Bruce
bn@bbn.com

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Date: Thu Jan 30, 1992 8:13 am PST
Subject: imperfective paradox

[From: Bruce Nevin (Thu 920130 08:00:09)]

(Avery Andrews Thu, 30 Jan 1992 15:04:38 EST) --

(How do you get Eastern Standard Time in Australia?)

It's a little worse than attributing "process" vs. "accomplishment" to classes of verbs. Notice:

> The eggs were boiling, but he took them off too soon so they're not boiled.

Thus it's not a complaint about boiling as a process verb but about boiling with a word denoting liquid as its argument. We don't require the eggs to have been liquified to say "the eggs boiled (in the water)," nor that they be a mass of eggs behaving like a boiling liquid, although it may be that the movement of eggs in boiling water may be perceptually sufficiently like the latter to support the analogic extension.

In operator grammar, -ing replaces the operator-indicator -s (socalled present tense, which really has nothing to do with time) when an operator becomes the argument of a further operator. (This in turn may be replaced by -tion, etc. with zeroing of a noun-form Oo operator --deed, event, fact, state, affair.) The progressive use of -ing is derived in GEMP from reduction of "be on" or "be in" to "be a-" to just "be" plus -ing (which came in under the preposition):

+John is on his going home
+John is on going home
+John is agoing home
John is going home

This is in fact the history of now archaic a- on verbs, as also on nouns: aboard < on board, etc. Compare also:

The house was long on one's building of it
The house was long a-building
The house was long building

while our house was building

the noise of a leaf in its being moved
the noise of a leaf being moved (OED 1596)

Because these uses of on and in are obsolete in most contexts (though not all), they can be replaced today by "in process of," which roughly paraphrases their meaning. Stative "accomplishment" verbs that are thought restricted from the progressive -ing can be used with this:

*He is knowing how
He is in process of knowing how

It is in process of costing $5.

Similarly for other operators indicating process:

When he finishes the payments, he will be owning his house.
He is resembling her more and more all the time.
It has been costing $5 all the time.

The distinction between stative and durative operators is a good instance of how facts of nonverbal perception can correlate with a classification of vocabulary. Other languages mark an animate-inanimate distinction much more thoroughly than we. (We are much more concerned with human or human-like vs. nonhuman, for operators like think, believe, say, and so on.) Many languages classify by shape, texture, consistency (liquid, solid, viscous material, cloth-like, stone-like, plate-like, etc.), type of motion, etc., and require you to specify one or the other by explicit morphology most of the time. This imposed punctuation of the world of perception is of great interest to us. Or is it only a reflection, and not an imposition, and all that differs from one culture to another is the explicit attention given to it?]

Durative operators (process verbs) and stative operators ("accomplishment verbs" plus most adjectives, prepositions, and nouns that are operators) reflect such a punctuation for speakers of English (and many other languages). It appears as though the classification of vocabulary does not impose itself on our perceptions. But maybe it does, and we just don't notice any more.

>Question: can we get the imperfective paradox for uncontrolled
>accomplishment verbs? My impression is no:
>
>   the water filled the pool in 7 hrs.
>
>   *the water was filling the pool (when Max turned the hose off).
>
>   sediment covered the ship in 5 years
>
>   %sediment was covering the ship when the explorers found it.
>   (doesn't mean the ship was getting covered).

I have no great problem with the ones you marked * and %. Try them with
"completely":

The water was completely filling the pool
when Max turned the hose off.
Sediment was completely covering the ship
when the explorers found it.

Presumably "completely" does not contravene the "accomplishment" but
rather emphasises it.

>If my impression is correct, one might be able to claim that the
>`imperfective paradox' can only be produced when the state designated by
>the progressivized verb is the goal being pursued by a person or
>person-equivalent.

(The class of non-human nouns noted above.)

These are examples of the socalled middle voice construction. This
construction can be derived by zeroing a higher operator of the undergo
or deserve type, such as be a case of, be involved in, participate in,
enter into (GEMP:368).

This book sells well. <--
+This book goes well in one's selling of it.

Water filled the pool. <--
Water underwent one's filling the pool with it.

Sediment covered the ship <--
Sediment underwent something covering the ship with it.

In the source, an agency is explicitly indicated as carrying out the
process of filling, covering, selling, etc. The zeroing appears to take
the process word out of its usual selection. This is most obvious for
things like "Frost's poetry reads well," where you would not say that
the poetry was doing the reading. No more would one say that the water
was doing the filling. In terms of perceptions, we say that something
else is the active agent. In terms of linguistic analysis, we can see
that this gives us a single word with a stateable selection (as to which
operator-argument dependencies are judged to be of normal likelihood or
acceptability). It is in this way that the linguistic analysis provides
a handle on the organization of perceptions.

Thus, we might suppose there were two words "boil," one intransitive (the water boiled) and the other transitive (she boiled the water). We can take "boil" as basically intransitive (like roil, probably originally onomatopoeic). The transitive use is causative by a reduction similar to that for the middle:

        The water boiled.
        Someone boiled the water. <-- Someone induced the water to boil.

Interesting issue. Thanks.

                Bruce
                bn@bbn.com

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Date: Thu Jan 30, 1992 9:29 am PST
Subject: Aquarius and universals

[from Joel Judd]

Avery (920130)

Just this morning I put you and Australia together, when trying to figure out the "culture" gap comment. The wonders of e-mail.
Bickhard's worth the trouble, I think. He is concerned with epistemology, which continues to be a problem in recent net discussions of PCT. Specifically, he has put forth some good arguments against "encodingism," or the idea that knowledge is somehow encoded in the brain. Criticisms of this idea have ramifications for just about every well-known idea about knowledge, from Chomsky and Fodor to Piaget. He can be dense, but once you get used to the vocabulary I think it makes sense. The 1986 book is pretty concise (read: short).
Also, I think one way to go about providing evidence for "universals" is to look for cross-linguistic evidence for kinds of perception. The one researcher I've read who might do this (he's not aware of PCT) is Russ Tomlin, who's been gathering evidence for linguistic ordering of episodes in a story or visual event.

Bruce (920130)

Well, Happy (late) birthday! I'll respond later to your comments.

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Date: Thu Jan 30, 1992 9:42 am PST
Subject: learning

[From Rick Marken (920130)]

Some time ago, Martin Taylor (920110) asked about PCT and learning:

>In the year I have been reading this list, I haven't come across anything
>that would qualify as an understanding of learning.

>Would it be possible to have a discussion of what it means to "learn"
>within PCT?

I'm game.

>I think part of my problem, to start a possible debate, is that
>reorganization seems to me to be largely a business of inverting the
>signs of the output from control systems

I think reorganization can involve changes to any of the functions or connections in a control system. Reorganization can involve 1) a change in the characteristics of the sensory function that maps inputs into perceptual signal; such a reorganization changes the "meaning" of the percpetual signal. 2) a change in the output function which maps error signal into lower order outputs (reference signals or physical variables); these changes might consist of sign inversions but they could also be changes in the gain and dynamic characteristics of the function. 3) a change in the connection of the system to other systems; this would include or remove the system from other control organizations. So reorganization is a general term referring to any way you might change the existing organization of a control system and/or its relationship to other control systems. "Who" does he changing is quite a different question. It's the "reorganization system" -- a meta control system that, I suspect, is getting close to dealing with phenomena that we subjectively refer to as "consciousness".

>   But what, in PCT
>theory, really does go on in learning? Is it the construction of new
>elementary control systems?

yes, partly. Also change in existing ones -- and their connections-- see above.

>   If so, where do the new systems get their
>references from, and why?

Don't know yet. My guess is that new systems get their references from other systems that happen to be "nearby" -- neurally. The ultimate "why" for reorganization is chronic, intrinsic error. This means that the current structure of the control hierarchy is not satisfying the basic, UNSPOKEN requirements of the organism. Intrinsic error simply says to the control hierarchy CHANGE. It does not know HOW to change in order to make things better. That is the most fundemental assumption of the PCT theory of learning, namely, that real learning involves getting to a new, better organization WITHOUT KNOWING WHAT THAT BETTER ORGANIZATION IS AND WITHOUT KNOWING HOW TO GET THERE. Thus, reorganization is RANDOM. I must be because it has no way of knowing what to do to make things better -- all it knows is that it must do (change) SOMETHING.

Reorganization works (learning occurs) because there is a criterion for success, namely , reduction of intrinsic error. Random changes that, as a consequence, reduce intrinsic error, are retained; those that do not reduce error are abandomed; This "biased random walk" can be a surprisingly efficient way to get to a goal (in this case, 0 intrinsic error).

The best example of reorganization (real learning) that exists in the psychological literature occurs before psychologists even start to study what they call "learning". Real learning occurs during the "shaping" phase of operant conditioning. Here you have an organism with enormous intrinsic error with no idea how to reduce it. So it does all kinds of things (tries to control all kinds of variables in various ways) -- it roams around, scratches, pushes, etc. Eventually, something produces food and reorganization SLOWS -- the organism stops varying it's behavior quite so much, but there is some variation and eventually the organism hits on the solution (developes systems that control the relevant variables) and reorganization STOPS; you have a bar pressing rat.

One of the few studies of reorganization from a PCT perspective was done by Dick Robertson and his colleagues. It is reported in Psych Reports (I think)-- I'll post the reference if you are interested, or, better yet, maybe Dick Robertson could post it. It is a really excellent piece of research -- with very interesting reults. Dick has some excellent ideas about reorganization -- particularly from a clinical perspective. Perhaps he would be willing to discuss some of them on the net.

Best regards

Rick

Date: Thu Jan 30, 1992 10:56 am PST
Subject: Mail addresses and the server

The biome mail server is having trouble with incomplete addresses. Please make sure that you have a complete Internet address if you want to use it, otherwise the mail will simply get lost.

An Internet address is one like "bill@biome.bio.ns.ca". If you are on BITNET, a standard BITNET address like "bill@system" will not work, but "bill@system.bitnet" will usually get through. But you have to append the "bitnet" so that the mailer knows that this is how you get your mail.

If your address ends in "uucp", like "bill@system.uucp", you will not get mail unless your system is directly known to biome (at present there are only two or three local machines that can use these "uucp" addresses).

Unless you have a reliable Internet or BITNET address and are certain that mail will get through, please do not request lots of material. If you get the HELP file fine, but if you don't get that, please do not request anything else. It just clogs the network.

Thanks, Bill
--
Bill Silvert at the Bedford Institute of Oceanography
P. O. Box 1006, Dartmouth, Nova Scotia, CANADA B2Y 4A2
InterNet Address: bill@biome.bio.ns.ca

Date: Thu Jan 30, 1992 11:54 am PST
Subject: Language and perception and the hierarchy

[From Bill Powers (920130.0900)]

Bruce Nevin (920120.1138) --

>Some time ago, I had proposed that the hierarchical control of words qua
>words (linguistic perceptions) runs in parallel with the hierarchical
control of other (nonlinguistic) perceptions, and had wondered about the
>mechanisms for associating the one with the other: (diagram showing a

single arrow from the category level to "words" in a linguistic chain).

>Then I proposed that category perception is not a level but rather a
>property of the association between the two lines of hierarchical
>control, verbal and nonverbal: (diagram showing all levels of the
perceptual hierarchy (labeled) connected by converging arrows to the
"word" level of a linguistic hierarchy).

>You proposed that a category-level ECS accepts EITHER lower-level
>nonverbal perceptiual signals OR a signal from a word recognizer as
>input (or both). On this view, categorization brings the two lines of
>control together in one: (diagram showing perceptual levels up to
relationship separate from linguistic hierarchy, then convergence to
single line from categories on up).

>This disgram [sic! The third, above] attempts to represent my
understanding of the effect of your proposal. Have I misunderstood you?

Yes, because I have evidently misstated myself. In my proposal there is no separate linguistic chain. It is all one hierarchy. Words are perceptions like any other perceptions: there is absolutely nothing special about them. Higher level perceptions are functions of lower level perceptions regardless of whether those lower perceptions consist of marks on paper, vocal sounds, groups of beeps, squeezes of the hand, expressions on a face, positions of the fingers and hands, smells on a rock, pats on the back, arrangements of sticks, patterns of drumbeats, flashes of light, or twining of tentacles. I recognize only one hierarchy of perception and control, and it must do everything that is done to and with perceptions.

In my discussions, I made it sound as if there were two hierarchies, and I should not have done that. We can partition the hierarchy according to subclasses of perceptions within each major type, but that is not a physical division, only a division of convenience, a way of categorizing. This division is misleading, because it implies that there are perceptual, comparison, and output functions concerned EXCLUSIVELY with words, so that one could tease the two hierarchies apart into physically separate strings of nerves with some undefined sort of connection running between them. This is not my picture of the brain.

What makes a perception into a "word" is not anything special about that perception, but the way it is used, the context within which it is used, the higher-level systems that use it (my version of Wittgenstein's principle). If word sequences are significant, they are significant to the same level of sequence recognizer-controller that deals with all other kinds of sequences -- without it, word-sequences would be unrecognized in the dimension of sequence, and they could not be produced, either.

If you recognize "categories" at all, then categories are a type of perception. You can call categories "a property of association," but what is it that you are asserting to arise from this particular property of association? Categories! If you couldn't perceive categories, then the establishment of categories wouldn't be recognized as resulting from the properties of association. But you do perceive categories; hence you can say that one property of association is to create what we perceive as categories. This leaves categories as a type of perception, however this mode of perception is implemented. If association is the means by which the input function creates a category signal (I'm not convinced of this, but it's possible), then association can create linguistic categories, linguistic-nonlinguistic categories, and nonlinguistic categories, starting with any sorts of perceptions.

The sense of sequence you get out of "bang, tweet, pop" is not different in kind from the sense of sequence you get from "John saw Jean" or "John verb Harry." The sense of category you get from "apples" is not different in kind from the sense of category you get from "nouns." In any case the result is just a signal in a particular place indicating presence of a sequence or of a category. There is no way to tell from looking at a category signal or a sequence signal whether it arose from objects that we classify as words or from objects we classify as things. We can't tell and the brain can't tell. It makes no difference from the level in question upward, except perhaps in terms of which muscles we use to control the perception (and higher systems don't know or care about that).

Only in the totality of consciousness, in which all levels of perception are considered, is the manipulation of language any different from manipulation of any other perceptions. When we are aware of the lower levels as well as the higher, we can see that sequences of words have a different origin from sequences of shapes in general. But neither the words nor the shapes are represented in the perceptual signals of systems higher than the configuration or perhaps event level. The levels of perception in the hierarchy are not cumulative in their significance: they are specialized. At the sequence level, the universe consists of sequentiality and sequentiality alone: not sequences OF somethings, but just sequence. At the logic level, propositions do not exist as words or sentences, but only as variables that have values. If Boolean reasoning is used, the values are TRUE and FALSE. If quantitative reasoning is involved, the variables are just continuously variable magnitudes. They are not magnitudes OF anything. They are just magnitudes, x's and y's in an equation, distinguished in the brain not by labels but by location in a particular path. The logical or other reasoning processes to which these signals are input don't care where the signals came from, any more than an XOR gate cares about the origins of its input voltages or a neuron cares about the source of signals arriving at its synapses.

What we experience consciously as language consists of vertical slices through this hierarchy. Every sentence contains references to many levels of perception, because it is constructed at many levels of perception and is itself an example of many levels of perception. At each level in the hierarchy where some feature of the sentence is perceived, there may be memory associations -- precise or loose, it doesn't matter -- that evoke other kinds of experiences that are perceivable at the same level, thus giving rise to meanings.

But this is not a unique relationship between word-perceptions and non-word perceptions. It exists among all kinds of perceptions. A tall tire on a wheel with wooden spokes means a certain car-body shape; the mechanism of meaning is the same one at work as when words are involved. Meaning is not something associated exclusively with language.

We have been through a long, interesting, frustrating, and I think valuable discussion, Bruce, for many months. It has gradually brought me to see some issues of which I have been only dimly aware. We have differed not about language, but about modeling. I have viewed everything you have said about language as an example of something that a brain can do, and have tried to see beneath the particular activity to the general capabilities needed to do such things. You have been exploring the structure of the activity, looking for the rules that apply to it, whereas to me the important thing has not been not what the rules are, but that there are rules at all. I was not clear in my own mind about this difference -- hence all my attempts at amateur linguistics.

Somehow, your re-presentation of those diagrams has helped me to find the center of my interests again. Now I can see things I should have realized long ago. For example, in your insistence that the structure of language is a social convention you were telling me that the structure of language that you were investigating is NOT a property of the human brain. To the extent that language is conventional, its structure is optional. Only those aspects of it that are exactly the same for every single human being can reflect true properties of and basic functions of the brain. And those are my abiding interest.

Martin Taylor (920129.1400) --

>There are those who do as Bill does (and as I do); they get things into
>imagic mode and work there. And there are those who work as Bruce seems
>to; they get things into linguistic form or something very like it, and
>work there. The linguistic types seem to be more numerous than the
>imagic types, and there are a large number of people that are hard to
>classify as either.

I don't think that anyone whose thought has a connection to reality works exclusively in the linguistic mode. And people like you and me who do think more often than not explicitly in the imagic mode regularly find that mode inadequate, and resort fo formal rule-driven symbol manipulation -- not just language, for communication, but mathematics, for precision, and programming, for demonstration and simulation.

One of my objections to the purely linguistic mode, which Bruce has recognized with mixed feelings, is that the structure of language can become divorced from the structure of experience in general. This is also true of mathematics, or any symbol-manipulation process that can work with abstract symnbols and without detailed reference to the lower levels of experience. It's even true in using language to talk about language.

I believe that theories couched in language or mathematics should contain frequent references to observation at all stages of the reasoning process. Even the intermediate calculations, assumptions, and implications should be checked against nonverbal perceptions, to make sure that the symbols and the reasoning process aren't taking off into worlds of possibility rather than the world in which we live.

This is why I occasionally rail against words that have no direct perceptual referents. Words that are defined only in terms of words that are defined in terms of other words are especially suspect. Bruce and you have remarked that language can help structure perception, and that is certainly true. But the same fact is also a great danger in language -- if we allow language to set our expectations without making sure that the language in question means something, we become prey to supersitution, rumor, demogogery, prejudice, falsehood, politics, and advertising.

Experience, not language, should dominate. When we rely too much on verbal or other reasoning without testing for meaning every step of the way, we let language push us around instead of using it as a tool.

>I think I USE images in ATTENDING TO language, and I think that is what
>Bill was also describing.

It was. Consciousness encompasses the whole hierarchy (at least potentially if not all at the same time), so that images and words seem to occupy the same space. One object of HCT is to decompress this space, to remark on dependencies within it and show that there are separable aspects of it that we normally do not distinguish. Different people are differently aware of these separations; I don't think anyone is totally unaware of them (most people make some distinction between at least sensations and concepts, and between at least some words and some meanings).

Joel Judd (920129.1555) --

>I notice that your hierarchy has the language counterpart of
>RELATIONSHIP still missing. ...

Not as a hard-and-fast rule, I think that generally prepositional phrases refer to relationship: in, of, to-toward, on, inside, before, and so on. Inside the sentence itself, the spatial or temporal attributes of the words -- separations by commas and parenthesis, betweenness, proximity and pairwise relative location in space and time, and so forth -- are significant relationships. To pacify Bruce, I don't claim that a relationship recognized in the physical sentence is connected one-for-one with a nonverbal relationship that is its meaning. The word-relationships indicate something, but perhaps only through association or convention, which are arbitrary.

>Wouldn't some developmental discussion help out in all this?

No doubt. Not from this word-processor, however.

Date: Thu Jan 30, 1992 1:44 pm PST
Subject: correction: wall, not group

[From: Bruce Nevin (Thu 920130 12:35:51)]

(Bill Powers (920129.0100)) --

>RE: groups and plurals.
>
>>Please be careful. The words "a group" were *overtly* present in the
>>phrase "a group of windows."
>
>My recollection is that Avery's example was "a wall of windows." Wasn't
>"group" added as part of the expansion of "windows"?

My fault. I carelessly used "group" in place of "wall" because I carried the derivation over from my (Monday 920120 11:22):

>. . . it is only in a fully
>explicit base form, without reductions, that we can determine what it is
>in language that is correlated, one for one, with perceptions. Without
>undoing the reductions, we have one kind of correlation for the and in
>
>   1. John and Bill
>
>and another kind of correlation for the and in
>
>   2. John came and Bill left.
>
>By undoing the reductions in
>
>   3. John and Bill left.
>
>in two ways, we uncover an ambiguity. This can be either of the
>following:
>
>   4a. John left and Bill left.
>   4b. A group, which includes John and Bill, left.

>. . . The sense of grouping together that we
>associate with and is due to a word like group or set, as in (4b).

Together with a word like "includes".

>There is no such perception associated with 4a, only the perception that
>the speaker is asserting two things together.

What I said in my (Tuesday 920121 0704) post, where I introduced the
confusion, was:

>Re walls full of windows, some words for that perception include
>group and set. The plural on "windows" I would account for as
>in one of my posts this weekend, e.g.

To correct my confusing error of substituting "group" for "wall,
I would amend the example following the "e.g." in that post as follows:

The wall is made of a window and the wall is made of a window . . .
The wall is made of a window and a window . . .
The wall is made of windows
The wall is of windows

The wall--said wall is of windows--is visible
The wall which is of windows is visible
The wall of windows is visible

I said "the wall" instead of "a wall" in this revision to avoid some
complexity about the sameness of the wall (here hidden under the
reduction to "the"), something I had avoided before by simply ignoring
it.

The sense of grouping together that we associate with the plural is due
to a word like "group" or "set" plus a word like "includes"--in this
case, the words "wall" and "made of". This nicely parallels the sense
of grouping together that we get with one sort of "and" but not with the
other, as shown by the contrast of (4b) with (4a).

Bruce
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Date: Thu Jan 30, 1992 2:51 pm PST
Subject: concerns about a theory of language

[From: Bruce Nevin (Thu 920130 13:20:17)]

BTW, I made a little shell hack to put that date stamp up in appropriate format. Anyone on a UNIX host using an editor that lets you dump program output into the file you are editing could use it. It's simple:

```
date|awk '{ print $1,9201$3,$4 }'
```

I keep the other stuff in a file named "from," position my cursor between the parens, and invoke this little "dt" script.

>We ought to be able to get together on this. Perhaps the problem here
>lies in what you mean by "most explicit." To me, that means lower levels
>of perception.

To me, it means saying things that went without saying. Often it is the more "concrete" nouns and operators on nouns that are represented only by indefinites. That's what happens with "abstract" nouns, which are reduced from operators--all those grand words ending in -tion, -ality, and the like. (I touched on this in the discussion of the watchamacallit paradox.) But there are other things that can be inexplicit in the reduced forms, as for example

>At the relationship level,
>suggested by the "wall of" part, the window and the wall in relationship.

This relationship is more explicitly stated in "wall made of". The specific sort of relationship can scarcely inhere in "of" since you get a very different relationship perception for "author of," "son of," "government of," and "destruction of" (for starters). The differences of meaning are not due to this being a different word "of" in each case, with different meaning. Nor is it because "of" only has a fuzzy kind of meaning, a sort of "related in some way" meaning that must be fleshed out by recourse to memory and imagination. Memory and imagination have a lot more help than that from the patterning that is in language. The additional meanings come from zeroed words that are recoverable because of that patterning, that is, because of expectations based upon it.

>This is not far from what you seem to suggest, but it puts the
>"explicitness" at a lower level, not a higher one. Is that acceptable?

Potentially at any level to whose perceptions words can correspond.

>>But one must perceive at least one T to get the T perception and one
>>must perceive its likeness to at least one other to get the perception
>>of repetition of the T perception.
>
>That would be a LOGICAL way to deduce repetition, but my hunch is that
>the perception of repetition is more direct than that. When the drum goes

I agree, in my experience, the larger configuration perception typically
comes first, followed by attending to details within it. You have
suggested a "same-texture" sort of perception that could come to
awareness before perception of detail in the texture. That would give a
perception TTTTTTTTTXTT but not a perception that it was made of Ts.
To become aware that it is made of Ts it seems to me you must first
perceive one T and its likeness to at least one neighbor or its
repetition making up the texture.

>>When a mass or aggregate noun like sand is pluralized, it is really
>>another noun which has been zeroed that was plural,
>
>I can't help wondering what you mean by "really." Are we talking about a
>process that we can perceive happening? I can understand that
>
>   all the sands <-- all the kinds of sand
>
>satisfies the demands of the theory, but what about the demands of
>observation? In fact, the above isn't even what I mean, perceptually, by
>"all the sands." My image isn't of different KINDS of sand. It's just a
>picture of sand here, sand there, sand everywhere. Same kind of sand. No
>"kind" in there, or any other zeroed noun that I'm aware of, at any time.
>Doesn't this count against the theory?

I can think of two kinds of plural "sands":

Which of these sands is best for cement, the coarse or the fine?

The sun sets slowly on the bloody sands of Iwo Jima.

I suggested derivations for both of them. Well there's another

The sands of his life were trickling away

This archaic usage is a reference to the sand in an hourglass. The
sand here may be pluralized to convey the sense of flowing "sand and
sand and sand," which is what seems to be going on with "the waters of
Jericho." These archaisms have the status of frozen expressions or
idioms today.

Give me a natural example of a sentence using "sands" in the sense that
you mean, neither the "kinds of" sense nor the somewhat archaic "we
shall fight them on the sands" (Churchill) sense meaning beaches or
(other sandy places), or the also archaic flowing "sands" of the
hourglass.

>>It works like so: take a set of sentences that differ only with respect
>>to one word. Rank the sentences in the set as to acceptability, ...
>
>This tells me immediately that some people will rank the sentences in
>accord with what majority does, and some will not. This removes
>"acceptability" from the realm of scientific facts, as far as I'm
>concerned.

People may disagree about the absolute acceptability of individual
sentences, or their ranking on some scale of acceptability. But this
does not matter. The relevant datum is the fact that the ranking

(whatever it may be for that individual at that time) is preserved (for that individual at that time) under the assertion of an operator or under a reduction, and is not preserved under other changes. And if a proposed sentence-difference passes this criterion for one speaker of a dialect, it will do so for another speaker of the same dialect, even though the details about absolute acceptabilities or relative rankings may be different for that individual. And this will be so for any speaker of that dialect. (Although there are always marginal constructions, as I have mentioned, at the growing/dying edge of the language, about which people will express uncertainty and disagreement. As Sapir put it, "all grammars leak.")

People speaking different dialects may not agree about these relations. That is what makes their speech different dialects. In Gloucester, my former neighbors'high-school age children would insist that "would've" should be expanded to "would of". I did not investigate--social dialect is a very touchy thing in our judgmental, class-ridden society, and must be approached circumspectly--but I would expect that this use of "of" in place of "have" had additional ramifications in their structure for the language.

Each language learner remakes the language, and agreement is not perfect else language would not change and vary as manifestly it does. However, the principle motivation for diversification appears not to be error, but rather to be control for distinguishing one's group from "those others" and newcomers to a group overshooting the mark as they construct a model of its norms for themselves.

This sort of thing does not make the study of language unscientific. It is rather a defining characteristic of that which a science of language studies.

>Another step is "Pick two sentences from the set that differ markedly in
>the judgements made of their acceptability or likelihood." (etc)
>
>Now the statistics become even fuzzier, unless by "differ markedly" you
>mean "ALWAYS differ, regardless of who is doing the judging." We are
>going to add another statistical discrimination on top of the one that
>initially established acceptability. The correlations go down some more.

Again: we are not concerned with statistics. The criterion uses very crude judgements of relative acceptability, likelihood, restriction as to subject matter in which it is of "normal" acceptability, or the like--various way to go about it. Find two that are clearly distinct in the mind of the language user by this criterion. Match them to the corresponding two sentences with the changed form. Are those two distinguished in the mind of the same language user same way or not?

```
(1)     John hit the ball               It was the ball that John hit.
        The puppy chewed the slipper    It was the slipper that the
                                          puppy chewed
       ?The ball ate the ice cream     ?It was the ice cream that the
                                          ball ate

(2)     John hit the ball               The ball hit John
        The puppy chewed the slipper   ?The slipper chewed the puppy
       ?The ball ate the ice cream     ?The ice cream ate the ball
```

The ranking on the left in (1) is the same as that on the right. The ranking on the left in (2) is different from the ranking on the right. This is born out by additional sentences for (1) and for (2). We conclude that the sentence-differences in (1) involve operators and reductions; those in (2) do not.

Perhaps you disagree about these differences of acceptability. It doesn't matter. I predict that your disagreements on one side of (1) will be parallelled by similar disagreements on the other. This is because I believe that your use of English includes control of operators and reductions that relate the two sides of (1) in the same way that mine does. Disagreements about the two sides of (2) are up for grabs. There is no relation of operator-grammar structure between them.

>The idea of "zeroing" actual words that exist and then are removed seems
>to me completely untestable. Worse, because you can't verify in any
>actual example of speech that any particular word was in fact present
>before "reduction," you're free to hypothecate any kind of word that will
>preserve the theory, so the theory becomes unfalsifiable. I wouldn't mind
>if the zeroings were only temporarily unobservable because we haven't
>thought of a way to observe them yet. But it seems to me that the zeroed
>terms are IN PRINCIPLE unobservable and will remain so. If this is true,
>then the zeroed words are simply wild cards that make all hands winners.

Your fears are unfounded. The conditions for zeroing are constrained. It's not a free-for-all. I won't try to describe them here, no time. Get a copy of _A Grammar of English on Mathematical Principles_ or its companion volume just out _A Theory of Language_ of which I sent you a chapter or two. The discussion in _Language and Information_ which I think you have is pretty sketchy.

I won't be in again until Monday. Have a good weekend, everybody!

Bruce
bn@bbn.com

Date: Thu Jan 30, 1992 4:41 pm PST
Subject: machine readable dictionaries

from Joel Walters

Does anyone know where I could get a machine readable bilingual (English-French) dictionary?

Date: Thu Jan 30, 1992 7:21 pm PST
Subject: language, variation

Re Bruce Nevin (30 Jan 1992).

>To become aware that it is made of Ts it seems to me you must first
>perceive one T and its likeness to at least one neighbor or its
>repetition making up the texture.

Perceive yes, perceive *as a T* (that is, categorize), no. Or such was the speculation I was launching, at any rate.

But Bruce is entirely correct about the inescapable role of variation in linguistics. Even if there is some level in which all people behave exactly alike, by the time you get to do linguistics on them, their histories are too diverse for there to be complete uniformity. So even though we seek uniform underlying principles, there is going to be extensive variation in what is actually observable.

Avery Andrews

PS EST on my postings refers to Australian EST.

Date: Thu Jan 30, 1992 7:37 pm PST
Subject: Re: correction: wall, not group

[Martin Taylor 920130 19:00]

Just to muddy the waters a bit on this question of repetitive structures forming groups, the perceptual side of this is not so clear. The discussion has gone on as if it is incontrovertible that the perception is of a window and a window and a window ... to a countable number. But it ain't necessarily so.

Some aspects of vision seem to be dealt with in the spatial frequency domain (or something like it), and in this domain, any repetitive structure shows up as spikes of high intensity at some frequency and its sub-harmonics. The location of these spikes in the 2-D frequency space shows the orientation and spacing structure of the group shape. Typically, it is very hard to count the members of a repetitive group, although you can see that there are "lots of them". Lots of them is a group, and a group can be signaled by the single (structured) spike in the frequency domain. So it can be a primary percept, not related at all to "a window and a window and a window". The structure of the spike can be reconstituted as the shape of the object that is grouped, so "a group of windows" can be a basic percept that only linguistically and logically is reified into "a window and a window and a window ..."

Chicken and egg, anyone? (Actually, my son answered this one at around the age of 7. He declared that eggs came first, because dinosaurs had them before there were any chickens. He's right.)

Martin

Date: Thu Jan 30, 1992 7:37 pm PST
Subject: Re: Language and perception and the hierarchy

[Martin Taylor 920130 18:30]
(Bill Powers 920130.0900)

In response to Bruce, Bill says:
>
> Now I can see things I should have realized
>long ago. For example, in your insistence that the structure of language

>is a social convention you were telling me that the structure of language
>that you were investigating is NOT a property of the human brain. To the
>extent that language is conventional, its structure is optional.

I think the important point is not the conventional (= optional) nature of language, but the commonality of structure across users of a language-- that they use the same structures for the same purposes, and can therefore make a start at understanding what each other is going on about. The word "conventional" gets in the way here, because although much of the surface structure of language is undoubtedly conventional (= optional), there is all sorts of reason to believe that something about language is built into our basic structure. Maybe that's a false belief, but it really doesn't matter for the arguments that Bruce has been presenting. So you may yet need to incorporate something language-specific in your modelling.

In response to me, Bill says:
>
>I don't think that anyone whose thought has a connection to reality works
>exclusively in the linguistic mode. And people like you and me who do
>think more often than not explicitly in the imagic mode regularly find
>that mode inadequate, and resort fo formal rule-driven symbol
>manipulation -- not just language, for communication, but mathematics,
>for precision, and programming, for demonstration and simulation.
>

Well, introspection is a suspect way of studying what goes on in one's head, but I THINK that when I do symbol manipulation exercises such as mathematical, I use the imagic mode to do it. I image how the symbols and the concepts that underly them behave, and see them fitting together. Something like (literal reinterpretation of a visual pattern here) "integral zero to infinity <something> e to the minus j omega t dt" seems like a single entity rotating the <something> so I can see its frequency shape. I cannot imagine how one simply does symbol manipulation as such, without these images. But I know people who say they cannot imagine how one can conjure up images, and who say that people who claim they do so are only fooling themselves. But it feels real enough!

> Bruce and you
>have remarked that language can help structure perception, and that is
>certainly true. But the same fact is also a great danger in language --
>if we allow language to set our expectations without making sure that the
>language in question means something, we become prey to supersitution,
>rumor, demogogery, prejudice, falsehood, politics, and advertising.
>Experience, not language, should dominate. When we rely too much on
>verbal or other reasoning without testing for meaning every step of the
>way, we let language push us around instead of using it as a tool.
>

How true, and how sad and dangerous. That there is a word for something is often used to buttress the claim that there is an experiencable something to which the word refers. It's sometimes called "reification." Terribly dangerous. I think it is at the heart of our current economic troubles. The word in question is "money" (and "deficit" is another one, as is "debt" when applied to the government). Treating money as an object rather than as a mechanism is surely based on such a misuse of language. The word is a noun, isn't it? There are pieces of metal and paper corresponding to it, aren't there? So of course it is an object, and therefore exists in some quantity. But in the real world, it isn't, and doesn't. And so, people starve in the midst of plenty.

Sorry, you tweaked a hobby horse that awoke, straining at the bit.

Martin

Date: Thu Jan 30, 1992 8:47 pm PST
Subject: Beer's Bug

[from Gary Cziko 920130.2200]

Especially for Pat and Greg Williams:

I've spent so more time reading Randy Beer's book and playing a little bit with your computer bug in preparation for his visit here next Tuesday. I still don't understand his bug (or your program) to my own satisfaction, but I thought I would share some impressions anyway and get your reactions.

I have been getting the impression that Beer's bug (a computer simulation which can wander, follow edges, find and eat food) may look and act to some degree like a network of control systems, but that it is really a complex stimulus-response machine. Take a look at Figure 8.1 on page 143. Here Beer has "mouth tactile," "mouth chemical," "antenna chemical," and "antenna tactile" as "sensory stimuli" and "feeding consummatory," "feeding appetive," "edge-following," and "wandering" as "behaviors. There is only one variable that looks anything like a reference level, "feeding arousal," and even this seems to act more like a stimulus than a reference level.

It may that Beer's bug can do what it can only because it lives in a disturbance-free environment, the imaginary world where S-R or cognitive plans can work as described in Bourbon and Powers resubmitted paper. When the bug puts its leg down and pushes back, it is assumed that this behavior will always have a predictable outcome. The bug uses "fixed action patterns." But give it something sticky or slippery to walk on and it seems to me that it will have lots of problems. As I understand PCT, it says that for behavior to be useful in a real environment, it must be adaptive WHILE IT IS TAKING PLACE. Beer's bug does not seem adaptive in this sense. Any sensory feedback it uses is at best of the TOTE type (sequential, iterative; clunky, not continuous). Perhaps this is why Greg has mentioned to me that Beer could use PCT in his modeling.

I really should have studied this thing more before making these claims, but I'm not sure when I'll get enough time. It will take less time to read your reactions.--Gary

----
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Date: Fri Jan 31, 1992 8:52 am PST
Subject: Perception of groups

:Bill Cunningham 920131 09:30:
(Martin Taylor 920130 19:00)

>... the perceptual side of this is not so clear.
Certainly not to me.

>Some aspects of vision seem to be dealt with in the spatial frequency domain
>(or something like it), and in this domain, any repetitive structure shows
>up as spikes of high intensity at some frequency and its subharmonics.

There are two types of repetition. One is multiple occurances of an object, small with respect to the field of view, and with no particular regularity. Second type of repetition may or may not be present, and deals with the spatial regularity at which the objects occur. I can envision the second form creating directly the pattern of spikes. First seems to require initial distinction, i.e. an irregular sequence of objects, already placed in the same category. Aggregation of more or less same objects within an ill defined region is of extreme interest. Certainly an argument for hierarchy of perception, independent of PCT.

>The location of these spikes in the 2-D frequency space shows the
>orientation and spacing structure of the group shape.

Certainly very strong for the regular sequence. Are you saying brain performs Fourier transform between levels so that next level works with the transformed signal?

>Typically, it is
>very hard to count the members of a repetitive group, although you can
>see that there are "lots of them". Lots of them is a group, and a group
>can be signalled by the single (structured) spike in the frequency domain.
>So it can be a primary percept, not related at all to "a window and a
>window and a window". The structure of the spike can be reconsituted
>as the shape of the object that is grouped, so "a group of windows"
>can be a basic percept that only linguistically and logically is
>reified into "a window and a window and a window ..."

Aren't you saying that periodicity can be a primary percept, and that from this comes direct aggregation into a group? This leaves me stuck with how one aggregates the aperiodic group members and recognizes they are, in fact, a group--and arrive at the same level in the hierarchy. Maybe I'm hung up trying to place both types at the same level.

Bill C.

Date: Fri Jan 31, 1992 9:02 am PST
Subject: RE: BEER'S BUG

From Pat & Greg Williams (920131)

>[from Gary Cziko 920130.2200]

>I have been getting the impression that Beer's bug (a computer simulation
>which can wander, follow edges, find and eat food) may look and act to some
>degree like a network of control systems, but that it is really a complex
>stimulus-response machine. Take a look at Figure 8.1 on page 143. Here
>Beer has "mouth tactile," "mouth chemical," "antenna chemical," and
>"antenna tactile" as "sensory stimuli" and "feeding consummatory," "feeding
>appetive," "edge-following," and "wandering" as "behaviors. There is only
>one variable that looks anything like a reference level, "feeding arousal,"
>and even this seems to act more like a stimulus than a reference level.

Other reference levels besides "feeding arousal" don't get reset by errors in the "feeding arousal" loop (as they would in PCT), making it look like they aren't reference signals -- but there ARE (unchanging) lower-level reference signals in loops which have changing loop gains during adaptive behavior.

>It may that Beer's bug can do what it can only because it lives in a
>disturbance-free environment, the imaginary world where S-R or cognitive
>plans can work as described in Bourbon and Powers resubmitted paper. When
>the bug puts its leg down and pushes back, it is assumed that this behavior
>will always have a predictable outcome. The bug uses "fixed action
>patterns." But give it something sticky or slippery to walk on and it
>seems to me that it will have lots of problems.

Beer's model for the bug's walking is directly based on neurophysiological data. It appears that the leg control circuits of REAL cockroaches are basically FEEDFORWARD with respect to sticky/slippery surfaces. That's what glue-type roach traps and catching bugs in smooth glasses are all about (watch those little feet fly!). The interesting point here is that this is an example of the lack of negative feedback in some nervous circuits of animals. It is to be expected on efficiency grounds (negative-feedback loops are more complicated than feedforward circuits, since the latter can dispense with comparators) that feedforward circuits would evolve when the environment rarely called for them. This is probably especially true in tiny organisms.

Also, you can disturb the bug's environment by adding a block in front of the bug at any time -- and the bug will go around it. You can also add additional food patches.

>As I understand PCT, it says that for behavior to be useful in a real
>environment, it must be adaptive WHILE IT IS TAKING PLACE. Beer's bug does
>not seem adaptive in this sense. Any sensory feedback it uses is at best of
>the TOTE type (sequential, iterative; clunky, not continuous). Perhaps this
>is why Greg has mentioned to me that Beer could use PCT in his modeling.

Yes, to an extent. But Beer SHOULDN'T use PCT if neuroethology says such loops aren't really there. However, the sensory feedback from Beer's bug's antennae orienting the bug toward food patches IS CONTINUOUS (and, we think, NOT "clunky"!).

Best,

Pat & Greg

Date: Fri Jan 31, 1992 10:14 am PST

Subject: RE: BEER'S BUG

[from Gary Cziko 920131.1000]

Pat & Greg Williams (920131):

>Other reference levels besides "feeding arousal" don't get reset by errors in
>the "feeding arousal" loop (as they would in PCT), making it look like they
>aren't reference signals -- but there ARE (unchanging) lower-level reference
>signals in loops which have changing loop gains during adaptive behavior.

I'll take your word for it. When I understand the system better I suppose
I'll find these.

>Beer's model for the bug's walking is directly based on neurophysiological
>data. It appears that the leg control circuits of REAL cockroaches are
>basically FEEDFORWARD with respect to sticky/slippery surfaces. That's what
>glue-type roach traps and catching bugs in smooth glasses are all about (watch
>those little feet fly!).

Now I'm confused. Why would "those little feet fly" if leg movement was
feedforward? If we could show that a cockroach maintains its walking speed
over surfaces of varying slipperyness by varying the temp of its gait then
we have the classic demonstration of a controlled variable. It seems to me
that the little feet should NOT fly if it is feedforward. Also, doesn't
flypaper work because the little critters are just not strong enough to
pull free? Haven't I seen flies and other bugs leave a limb or two behind
when only a limb or two makes contact with the sticky stuff? If so,
wouldn't this indicate that they are pulling a lot harder than normal and
also evidence for negative feedback?

>The interesting point here is that this is an example
>of the lack of negative feedback in some nervous circuits of animals. It is to
>be expected on efficiency grounds (negative-feedback loops are more
>complicated than feedforward circuits, since the latter can dispense with
>comparators) that feedforward circuits would evolve when the environment
>rarely called for them. This is probably especially true in tiny organisms.

I had thought about how weighing so little makes one less prone to gravity
and inertial disturbances. I suppose it doesn't make much difference if
you're walking uphill or down if you weigh next to nothing to begin with.
But aren't there other disturbances that don't depend on weight (we've
already mentioned stickiness and slipperyness)?

>Also, you can disturb the bug's environment by adding a block in front of the
>bug at any time -- and the bug will go around it. You can also add additional
>food patches.

Yes, but the food doesn't try to get away from the cockroach and there is
no ice or glue to walk over. So maybe you're saying that it is negative
feedback at upper levels but feedforward at lower ones.?

>But Beer SHOULDN'T use PCT if neuroethology says such loops
>aren't really there.

If the neurosciences could easily find control loops wouldn't all the
neuroscientists interestd in mammals be on CSGnet by now? It is really so
easy to find this evidence looking at the neuroanatomy? I'd feel better

basing my model on behavioral data. What we need is a Demo1 and Demo2 for cockroaches. Then I'd think twice about squashing them and ask them to be subjects instead!--Gary

P.S. Bill (Powers), you claimed recently that there is no important correlation between efferent nervous activity and behavior, but you didn't specifically mention cockroaches. Did you mean to include cockroaches in this claim?

Gary A. Cziko   Telephone: (217) 333-4382

Date: Fri Jan 31, 1992 11:02 am PST
Subject: Re: Perception of groups

[Martin Taylor 920131 11:20]
(Bill Cunningham 920131 09:30)
>
>There are two types of repetition. One is multiple occurances of an object,
>small with respect to the field of view, and with no particular regularity.
>Second type of repetition may or may not be present, and deals with the
>spatial regularity at which the objects occur. I can envision the second form
>creating directly the pattern of spikes. First seems to require initial
>distinction, i.e. an irregular sequence of objects, already placed in the
>same category. Aggregation of more or less same objects within an ill defined
>region is of extreme interest. Certainly an argument for hierarchy
>of perception, independent of PCT.
>

I was intending to respond to the wall of objects, in which the patterns are repeated with regular spacings. The question of irregularly placed and spaced objects being perceived as a group is a quite different matter. Personally, I don't think Fourier transforms are much used in vision, but there seem to be some circumstances in which they, or something like them, are important. This may be one such condition.

There are quite a few strange perceptual effects associated with repetition, whether regular or otherwise. I'm not well up on that literature now; it was in the early 80s that I was more concerned with it. At that time, it seemed that concepts like Julesz' "textons" were important at low levels (line ends, corners, orientation regularity, etc.), and that the meaning of items was important at high levels. For example, one study I remember showed that if all the background elements conformed to a certain class (say "letter") and the target didn't (say, "digit"), the digit was rapidly picked out in a time independent of how many background distractors there were. Now, if the target was a circle, it could be an "Oh" or a "zero". If it were to be treated as a zero it could be detected like a number among letters, but if it were an "Oh" it would be much harder to detect, and would require a linear-time search, taking longer the more letter there were. The converse would be true if the background distractors were digits.

Groups and regularities are strange in perception, and I would be very wary of taking too strong a stand on anything based on how they are perceived or talked about.

Martin

Date: Fri Jan 31, 1992 11:13 am PST
Subject: Language and meanings

[From Bill Powers (920131.0800)]

Bruce Nevin (920130.0725) --

>Bill quite rightly took me to task for suggesting ...

Not being in academia, save for a lowly and remote adjunct professorship that has not yet become official, and not being in competition for funds, popularity, control, or prestige, I try to refrain from taking people to task. I just say what seems right or wrong to me at the date and time noted, subject to correction by those who know better. If you think you can be any more humble and noncompetitive than that, you're welcome to try.

I find forms such as "He is in process of knowing how" (in one comment to Avery) excruciatingly artificial, not to mention unbelievable as a construction anyone would actually use under any circumstances for thinking or communicating. I would translate that phrase into English as "he is learning how."

Such phrases encourage me in my belief that the actual process of language recognition is

Heard ("reduced") phrase -> sketchy experience -> expanded experience -> description of expanded experience ("unreduced" phrase).

My contention is that "in process of knowing" is an attempt to describe perceptions at several levels, while adhering to some principle of sentence construction that I don't recognize.

For recognition purposes the final step -- turning the expanded experience into a description -- is unnecessary. But if it is turned into a description, why not use ordinary language?

A request: it might be useful if the linguists aboard turned their attention to this term "description." How would we define this word in terms useful to HCT?

In another comment to Avery you say

>It's a little worse than attributing "process" vs. "accomplishment" to
>classes of verbs. Notice:

>   The eggs were boiling, but he took them off too soon so they're
>   not boiled.
>
>Thus it's not a complaint about boiling as a process verb but about
>boiling with a word denoting liquid as its argument.

I think this illustrates my point above. What defines a "class" of verbs in this case seems to be a perceptual criterion, not a linguistic one -- i.e., the perception that the verb "denotes." The verbal form seems to depend on what you imagine the actual (i.e., perceived) circumstances to have been, despite what the words appear to say. Eggs, clearly, do not boil. It is the water they are in that boils. Eggs, once boiled for a while, change their internal consistency. To say that an egg taken out of boiling water isn't boiled is to say that the internal consistency is not what was expected as a result of the usual boiling process, not that the egg was never in boiling water.

Furthermore, eggs do not get into boiling water without intervention of some purposive agency: somebody is boiling the eggs. There is no motor action called "boiling," so if we want to be explicit we have to specify the agency and the means, as well as the perceived consequence: somebody put the eggs in water in a pot and put the pot on the stove and turned on the heat under the pot and the water became heated to the boiling point and continued boiling with the eggs in it. That is what we imagine, more or less, when we encounter a pot containing boiling water and eggs, even if we didn't put it on the burner ourselves.

There are lots of low-level things we may imagine but don't bother to describe, although we could: which hand the person held the pot in, which burner on the stove was used, how full the pot was, how the water looked when we concluded it was boiling, whether the eggs were totally submerged, what their color was, whether the lid was on the pot, whether we knew how long it's been since the boiling started, and so on at length. We may described such a scene with a terse phrase, but if the listener doesn't understand we can examine the scene in more detail and describe it at a lower level.

I seem to have no trouble expanding phrases like "he boiled an egg" into more explicit statements that still sound like simple English and that are more literally correct. I'm sure you have no trouble either, when you're not talking in that funny way. What is added by the use of such expressions as "The house was long on one's building of it?" If your friend said "I thought you'd moved in already," and you replied "The house was long on my building of it," would that comment be understood better than "The house took a long time to build"? I think most of my acquaintances would reply, "Yeah, I suppose it is a long house, but how come you haven't moved in yet? And where did you pick up that accent?"

What the heck does

        Sediment underwent something covering the ship with it.

mean?

I'm pulling your leg a bit, but I'm serious about not understanding what is gained by using such awkward and ambiguous expressions. Can you explain, as if to an idiot? (As if, I said).

Rick Marken (920130.0937) --

Nice tutorial on reorganization. The best part is that I didn't write it.

Date: Fri Jan 31, 1992 11:46 am PST

Subject: RE: BEER'S BUG

[From Rick Marken (920131)]

Gary Cziko, and Pat & Greg Williams are discussing Beer's Bugs. I've just looked through Beer's book and I will give you my opinion about what is going on.

I think what Beer has created is something very similar to the S-R bugs that I discussed over a year ago. For example, there is a fellow (forgot the name) who developed some "flocking birds" based on what he called an S-R model. Actually, the birds were feedback control systems because their outputs (R) influenced the inputs (S) that drove those same outputs (R). Everything worked OK because there were dynamic constraints built into the "Boids" (not a misspelling -- that's what he called them). The dynamics were handled by the software (as we do in many of our control simulations) because the boids operated in a software world -- with no real dynamics. The same is true of Beer's bugs. The disgrams he shows make them look like S-R systems but they are really feedback control systems because their outputs influence their inputs; his differential equations for the neural dynamics take care of the dynamic constrainst that are needed to keep the control system stable.

So Beer's Bugs already are control systems -- controlling input. And to the extent that those inputs are disturbed the disturbances will be resisted.

To see what is going on, check out Fig 4.2 in Beer's book. Outputs (stance, foot, swing) are generated by a pacemaker and a command cell. It loos like the pace maker is modulated by inputs from the sensors (backward leg angle, forward leg angle). But these angles are also influenced by the outputs so there is a loop -- it's just not evident from the way Beer draws the model. It is also not obvious what is controlled (each angle, the sum of the angles?) and what serves as the reference (is it LC or is there just a constant offset?). These things could probably be found out analytically; but that's my beef with Beer -- he conceals the important organizational structure of his model. He makes it look like an S-R model but it's not; and his lack of understanding of how control works leads him to neglect the most important organizational principle underlying the operation of his bugs -- they are organized around the control of INPUT variables. But he has made it very difficult to tell what those variables are. That's because he is most interested in the externally visible appreance of the behavior of the bug -- he likes to see the bug doing things that create interesting perceptions for Beer; but the bug itself can only be controllingf its own inputs.

Everything would be clearer if Beer designed his bug from the bug's per-spective. Then he would be a real PCTer. As it sits, his stuff "works" inasmuch as it entertains observers but it doesn't provide any fundemental understanding of how behavior MUST be organized (around the control of perception) if it is to be ADAPTIVE (disturbance resistant).

Beer's model is in the same category with Brooks' insects -- which are control systems (to the extent that they control -- ie, behave "adaptively") but they don't help us understand what is really going on with adaptive behavior.

So my main complaint is that the Beer (and Brooks) approach obscures what is important. It would be very hard to build a fairly complex control system using their architecture (one that controlled many, complex input variables) though I agree that it could probably be done. But I think it would be like trying to do arithmetic with Roman Numerals.

Best regards

Rick

Date: Fri Jan 31, 1992 3:09 pm PST
Subject: Re: Perception of groups

:Bill Cunningham 920131 13:35:
(Martin Taylor 920131 11:20)

>Groups and regularities are strange in perception, and I would be very
>wary of taking too strong a stand on anything based on how they are
>perceived or talked about.

Interesting comment and good advice. Clearly they are perceived, and at high levels of abstraction. The how and why of that, and what sensory inputs will assist the individual to perform at those higher levels, is at heart of my question. Maybe I was hoping for a magic answer, certainly not taking a stand. As for most of the talk I've heard, it seems to be strong on stand and weak on perception.

Bill C.

Date: Fri Jan 31, 1992 3:55 pm PST
Subject: 'STATS VS CRUISE CONT'L

FROM BOB CLARK

Sorry to be so long in reacting to your suggestion. Things have gotten pretty hectic lately.

Your suggestion about Cruise Control is interesting. It can lead to a discussion of several aspects of control system operation: sensitivity, response time, offset (or "dead zone"), relation to other systems, etc. These are interesting and important in themselves.

However I selected Thermostatic Systems because they are familiar to many people and they include the basic elements of Negative Feedback Control Systems. That is, they include: 1) a means for detecting a variable, 2) a means for affecting that same variable, 3) a means for subtracting the magnitude of that variable from some "preset" value, with a resulting positive difference acting to produce a positive output from the second "means". This is the usual combination of components composing a Negative Feedback Control System.

Such a system need not have a continuous output to achieve its result. It is interesting to observe that "continuity" is, in part, a matter of "viewpoint." Thus, if the Thermal System is observed over a period of several hours, its control approximates continuity. And the Cruise Control, observed in milliseconds, reveals various limitations.

Also, it is true that the Thermal System is a "one-way" system as usually presented with a furnace, etc, that is only one of several limitations it suffers. Another "one-way" system is a living muscle fiber! It can only pull, not push.

The Thermostatic System can also be used to illustrate other aspects of control systems -- and other forms of control system. Thus:

Before Thermostatic Systems were developed, people kept warm in the winter. My father had a coal fired furnace that had a damper that adjusted its operation. Too cold -- open the damper; too warm -- close the damper. And it was a fairly continuous operation.

Where was the Control System? Clearly the situation was livable, although not as convenient as one would like. Obviously, there was a control system in operation where the temperature (where?) was the controlled variable even though the control was accomplished by adjusting the rate of heating.

But where was the Control System? Without a person, the temperature was not controlled -- but also without a damper and a fire box,the temperature also was not controlled. Some person - DECIDES - whether action is needed, and in which direction. He then uses his (lower order) muscle systems to affect his - ENVIRONMENT - according to his - UNDERSTANDING - of his environment.

What about "his - ENVIRONMENT - ?" This usually refers, perhaps vaguely, to his physical surroundings outside his skin.

But someone else might be available and asked to "open the damper," "turn up the furnace," etc.

Where now is the Control System? The person could "do it himself" or "ask someone else." Having made his - DECISION - , he used his lower order systems to get his desired result. Were there two (or more?) levels of control involved: the "other person" and the "furnace."

Thermostatic Systems now take the place of the "other person," much more efficient and convenient.

For those who are familiar with thermostats, most of this is unnecessary. But what about those whose - ENVIRONMENT - does not include - UNDERSTANDING - of control systems? You must have seen people turn the setting up higher and higher when the room doesn't warm up fast enough? The furnace was already at full speed, so raising the setting has no immediate effect. Later, however, the room is too warm and the setting is reduced. This is "over-control" and the system is oscillating!

Notice the importance of the - TIME - scale of the person vs the "response time" of the System.

This illustrates the difference between regarding the assembly of parts as a Control System rather sie≡my a group of connected parts. Such a difference in - VIEWPOINT - can result in a difference in behavior. Often these e·fferences have little effect, but sometimes they are very important!

Both viewpoints are valid and result in the same mathematical representation. However one is more useful for using the system, the other for modifying the system.

Several words have been noted above: DECIDES, UNDERSTANDING, ENVIRONMENT, CHOICE, DECISION, VIEWPOINT, TIME. These words and their associated concepts are used routinely and seem to be readily accepted. However each of them is very important, and merits closer examination.

And how does each relate to a Hierarchy of Control Systems?

Regards, Bob Clark

Date: Fri Jan 31, 1992 5:46 pm PST
Subject: Re: learning

[Martin Taylor 920131 19:30]
(Rick Marken 920130)

Thank you, Rick, for at least presenting something on learning/reorganization. Since Bill P. also thanked you, I guess you represented his concepts as well. But "anything can change, and it does so randomly when intrinsic error is too big" isn't very helpful when you are trying to model a control system that learns. What, for example, is "intrinsic error"? Is it a global measure of the average error per ECS? If so, in a dynamic system that is always shifting in response to shifting goals and environmental disturbances, intrinsic error will always be there, often large. If it is something else, what is it? Is it something an individual ECS can assess within itself, such as absolute error averaged over recent time?

Bill claims that changing gain, if the gain is large enough, doesn't matter much. You say that is one of the aspects of learning. If it doesn't matter much, then changing it won't have much effect. But in a highly redundant hierarchy, maybe the gain of an individual ECS does matter, and it is something to be learned.

Connections can change, new ones being grown according to neural proximity (you don't claim this a authoritative, but it seems plausible). Can these connections link ECSs of the same level? There seems to be ambiguity across various postings as to whether ECSs must link only to thó lext Le!el up/down or whether they can link within a level. Weights can change in the perceptual functions, making the perceptual connection set even more like a powerful MLP.

What are the important aspects?

I ask, partly because it seems a very important issue in PCT, and partly because I have asked a contractor to investigate the possibility of growing a "Little Man". We call it the "Little Baby" project, and I will describe it separately some time -- not now. The idea is to start with sensors and effectors much like Bill's Little Man (Arm Demo), and to use the concepts of distributed coordinated control that I posted a week or two ago to allow the Little Baby to go from a flailing infant to a controlled adult. I don't know if it will work. One aspect of the study is the learning algorithm. Do we change the perceptual weights so that the Little baby knows what patterns to try to perceive? That may be necessary, but I don't think it will be the whole story, because I don't think it will achieve the smooth corrdination I anticipate (but maybe it will). Do we use Genetic Algorithms on ECSs working in imagination mode to organize patterns of reference signs that create cooperation and not much conflict? There are all sorts of possibilities. I had hoped that those who have worked with PCT for many years would by now have a good handle on what works and what doesn't in training the system.

And that word "training" brings up another aspect: teaching. What happens when a hierarchic control system is taught rather than learning from experience? I do not intuit a good place for the teaching to fit. Vaguely I can see setting reference patterns to match the percepts "imposed" by the teacher, but that's not very satisfactory to me as a first cut. Is there a party line on the difference between learning from experience (acquisition), and being taught? Being taught surely cannot involve random restructuring in the face of excessive intrinsic error, can it?

Puzzlement.

Martin

PS. The Little Baby is intended as a way station toward investigating what various posters were talking about on several occasions over the last year--using analysis by synthesis to learn, with specific reference to speech recognition. But that's probably far away.

Date: Fri Jan 31, 1992 7:26 pm PST
Subject: Wow on Closed Loop!

[from Gary Cziko 920131.2025]

Greg Williams and Ed Ford:

I just received the latest issue and of _Closed Loop_ this afternoon and was VERY impressed. Seventy-two lively pages of discussion on "social control," and the new cover makes it look like a "real" journal! Greg, thanks so much for editing this for us. I just hope you can keep it up. Ed must have also put lots of time into the production and distribution and so thanks are due to him as well.

It's so comforting to know that even if I can't always keep up with all the discussion as it occurs "live" on the net that I can always catch up on the major topics via _Closed Loop_.

It's hard to believe that this all started just one and a half years ago when I collected eight e-mail addresses in Indiana, PA at the 1990 CSG meeting. Just goes to show what kind of an effect a powerful idea can have. And for that we have Bill Powers to thank for sharing his insight with us.

It's so comforting to know that even if I can't always keep up with all the discussion as it occurs "live" on the net that I can always catch up on the major topics via _Closed Loop_.

Have you guys thought of single-issue distribution, or would this be too much of a hassle? Given the theme of this issue, I could imagine Tucker or McPhail or McClelland possibly wanting to order a stack for their classes. I also might want a few more copies of this isssue for some of my students and colleagues. Any thoughts about this?--Gary

P.S. The only quibble I have with _Closed Loop_ is the spelling of CSGnet. Greg writes it "CSGNet." I like the small "n" so that the two meaningful units of CSGnet are nicely distinguished. Since I started CSGnet, I want it spelled MY way. What do you think of that?

Gary A. Cziko   Telephone: (217) 333-4382