Subject: Avery, Gary, Ed, Greg

[From: Bruce Nevin (Fri 92041 Fri 92041 12:49:44)]

(Avery Andrews (920430.1053)) --

>What's suspicious about correction is that for the most part language
>seems to be learned without it (though some child language researchers
>I've talked to suspect that correction of children by slightly older ones,
>delivered of course in the most soul-destroying manner attainable,
>may be more significant than orthodoxy would have it). So as far as
>linguistics is concerned, it's a pretty marginal phenomenon.

Suggest looking at Jerome Bruner (1983), _Child's Talk: learning to use language_ (Norton). He shows evidence of a social Language Acquisition Support System (LASS) to complement the hypothesized Language Acquisition Device (LAD). The mothers he worked with provided a lot of overt correction, though this may be in part an artifact of their being middle class members of an occupational category (academia) whose members typically have an especially high index of linguistic insecurity, in a notoriously class-ridden society (UK). However, there is a lot of correction that is unavoidable just in the maintenance of the constrained frames for learning that constitute the LASS. We had some discussion of this last spring, and after I posted a review of the book (thanks again, Chuck) Joel told me that there had been extensive discussion some months before I came on board. I don't have those files, but you could poke the archives for them.

(Gary Cziko 920430.1155) --

The quote about pilots vs engineers reminded me of observations that occurred to me on my return flight from California last summer. As the plane was circling to land at O'Hare, I watched the flaps extend and move up and down to adjust the attitude of the plane. Conventional views of behavior are analogous, I thought, to supposing that the pilot was controlling the angle of the flaps. "Move left flap to -37 degrees. Now -49 degrees . . . -42 . . . now -17 degrees" and so on. (I could only see the left wing, of course.) Looks very complicated, and it is. Until you contrast the perception of these movements with the perceptions the pilot was actually controlling. Turns out all the complication was handled by mechanisms below the level to which the pilot was attending.

I am reminded again of the Balinese dancers, a girl standing in trance on the shoulders of a man. Which way the child in trance leans, how fast and in what degree, determines how he must move in order to maintain the balance and integrity of the dancing unit. Bateson describes this in one of the essays in the first part of _Steps_, but I don't recall which.

(Ed Ford (920430.11:44)) --

>"Me and Eddie, we was rose down to the ridge together."

I guess this is a kind of hypercorrection, where the lower-class speaker knew that some other form was preferred over "raised" and grabbed at "rose" rather than the only seemingly related "reared." In the

courtroom the defendant would want to emulate white, middle-class speech as far as possible, but it is like a foreign language.

>"Them bottles of wine, they was drunk between who?"

I guess this is the lawyer trying to incorporate a phrase used by the witness into a question, unsuccessfully because the lawyer is not fluent in the lower-class dialect used by the witness and/or anyway does not want to *really* speak that way. There's a conflict between wanting to use the words as used by the witness (not leading the witness, putting words in their mouth, etc.) and wanting to speak by norms shared with the judge and other lawyers.

These are just guesses, stories in context of which these utterances make sense to me.

Gee, my grandfather was Phi Beta Kappa, Yale Medical summa cum laude, 1906 I think. Nah, they wouldn't have met.

>The problem for me is that to be properly studied, understood, and
>fully tested, a belief system has to be checked out through experience.

In my experience you have to live more than one alternative belief system fairly authentically before you can begin to distinguish the fish from the water. When I lived in Greece, I went through a period of being very negative about aspects of Greek culture. What I was doing was projecting things I didn't like about my own culture and character, the better to dissociate myself from them. That was a crude application of the effect I mean. Conversely, though in a different domain, something that is problematic for one theory but not for another suggests that some artifact of theory or formalism is at the basis of the apparent difficulty.

(Avery Andrews (920439.1358)) --

>Where grammaticality judgements come from is an interesting
>question. I suspect that they are a side effect of the operation of
>machinery whose purpose is to reduce the range of available meanings
>of an utterance, so that it will be easier to figure out which of the
>remaining possibilities is the intended one. The grammar for example
>tells us to group a demonstrative with the following rather than the
>preceeding nominal material, so we know what `that' is modifying in:
>
> John gave the student that article
>
>but it follows from this principle that
>
> John gave the student article that
>
>gets no meaning

It is specious to say the second example gets no meaning because to say so presupposes that it would occur in native English, and it would not. What might occur that is superficially like it involves error and interrupting correction, for example:

John gave the student <?> article--that! [pointing]

An "an" could be coalesced with the coda of the last syllable of "student," and "the" could POSSIBLY be articulated so poorly as not to be audibly distinguished from the final t of "student." In any case, the intonation represented graphically by the dash, and the timing of body language, make this clearly different from what you intended. Only a system not controlling for the perceptions that speakers of English control for could generate your second example as intended (transpose the "that" after the noun). Irrelevant, immaterial, and (speaking with special precision here) incompetent.

Too many discussions of grammar turn on an argument that goes something like this:

> H²rules that generates structures that we
> find in sentences people actually use.
>
> It also generates these other structures, but for them there are
> no corresponding sentences that people use.

+t?

> There must be a convention or principle that blocks or filters
> out the unwanted ones.
>
> Here's one. And it also works for these other examples that I
> didn't tell you about before (I was saving them for this part of
> the paper). The principle (or convention) is independently
> motivated. Unless someone finds a counterexample, it must be
> true.

I submit that such systems of rules fail to represent (or describe) the perceptions that users of English are controlling in their use of English. And that is why they need all these ancillary principles and conventions, just as Ptolomaean astronomy needed its epicycles. I suggest that they appear to be universal (to the extent that they do) only because they are artifacts of the tools used. When you wear green tinted glasses, you're in the Emerald City of Oz.

(Greg Williams (920430)) --

Yes, ecstatic experiences are very distracting.

Even remembering involves interpretation in terms of other remembered perceptions of other kinds of experience. Rather like the difficulties attendant on dream recall. So it is not all the fault of language. A great deal of interpretation and fitting to categories goes on without any attempt at verbal description.

Sometimes there is no (conscious) memory. It is as though one went unconscious. (From another perspective, I suppose one identified with lower levels, which are unconscious when attention is at a higher level. Perhaps that is only an analogy.)

Sometimes there is memory of a dual experience, of some processes (a convenient if dubious label is "the personality") undergoing all sorts of reactions of fear, joy, calling out for help, vs some other sort of perspective that simply witnesses the former together with that which it appears to be encountering. For example, the latter may appear

to be simple witness of the same sort, and the absolute neutrality of it (perhaps the experience of being objectified) has a lot to do with the reactions of terror, etc.

But I'm sure you have lots of descriptions like that in your library. And I realize you know full well that it's rather another matter in the flesh, so to speak. As the farmer in the old joke put it, you can't get there from here; you have to go there first.

Date: Fri May 01, 1992 10:04 am PST
Subject: Avery, Gary, Ed, Greg

>>
>> John gave the student article that
>>
>>gets no meaning
>a
>It is specious to say the second example gets no meaning because to say
>so presupposes that it would occur in native English, and it would not.

The hypothesis I'm proposing is that it does not occur in native English *because* it gets no meaning by the normal operations of the perceptual system described by the grammar. This could be false, but it isn't specious.

Avery.Andrews@anu.edu.au
(currently andrews@csli.stanford.edu)

Date: Fri May 01, 1992 11:18 am PST
Subject: syntax as meaning

(Avery Andrews (Fri, 1 May 1992 10:45:34 PDT)) --

>The hypothesis I'm proposing is that it does not occur in native English
>*because* it gets no meaning by the normal operations of the perceptual
>system described by the grammar. This could be false, but it isn't specious.

The phrase "by the normal operations of the perceptual system described by the grammar" is a nice hedge, but what does it mean?

I take it you mean to avoid problems due to other parallel operations of the perceptual system imputing meaning to word sequences despite their ungrammaticality. I'll let you have that (for now). But you still have problems with the "Oh, I know what she means" processes by which we correct what we hear, most of the time without noticing that we do so. (BTW, Rick, this is the sort of place to look for control, not in the enforcement of rules learned in school.) A person hearing your unlikely transposition of "that" would reconstruct some grammatical structure-cum-error for it (if she didn't just say "huh?" and ask you to say more). One possibility:

A. John gave the student article that
B. The student article that said what? (Assuming "that <sentence>")

Transposition of determiners to follow the noun in English does not occur in people's speech and is not among the interpretations one would

make of your example, not because it is meaningless, but because it violates conventions of English word order. The equivalent word order in a different language with different conventions is not meaningless. And if I and some friends agree to experiment with an alternative convention, I think someone overhearing us talk would find the following meaningful, if initially a bit disorienting:

> We're experimenting with putting definite article the and other determiners after noun the. Can you understand way this of talking?

Are you dispensing with the distinction between ungrammatical but meaningful utterances like these and the (purportedly) meaningless but grammatical "colorless green ideas sleep furiously."

I want us to be in good agreement as to just what you are proposing to do here. I would caution you, by the way, that it is very difficult to rule out *any* utterance as meaningless, as is shown by the experience of interpretations making sense of the colorless green example and by many linguists' experience presenting starred "can't say" examples to their students (only to have them come up with a context in which it makes sense). The reason this is so hard to do is because people are attending to control of meanings (association with controlled nonverbal perceptions) more strongly (with higher gain) than they are attending to control of word dependencies and word shapes (including reductions). The latter is made to serve the former. That doesn't mean the latter can be reduced to the former. Bill was proposing that when we started our conversations together here, and I think I have persuaded him that there really is control of language apart from control of nonverbal perceptions in experience, memory, and imagination which may be associated with utterances.

Be well, Bruce bn@bbn.com

Date: Fri May 01, 1992 12:19 pm PST
Subject: For the Linguists

For the Linquists

Bruce Nevin's recent reference to Jerome Bruner's wonderful book, Child's Talk, reminded me of a question I have had for the last two or three months. I would appreciate an expert PCT assessment of research reported in the 31 Januaryh 1992 issue of Science by Patricia K. Kuhl et al. It is my understanding that all human infants can make the full range of sounds upon which every known human language is made. Kuhl reports that all human infants can, at birth, recognize all those sounds indiscriminately. However, by hearing only the language spoken by their caretakers (parents?), they gradually come to be less capable of recognizing that full range of sounds and increasingly capable of discriminating the sounds of their parents' language. Kuhl refers to these latter as "phonetic prototypes" and suggests they work like "perceptual magnets" which result in other similar sounds being perceived in the same category as the prototypes. Kuhl et al used "computer-synthesized prototypes of the American English /i/ and Swedish /y/ vowels" plus 32 additional similar but not identical variants. The same computer equipment, speech testing devices, and personnel were used in the USA and in Sweden to examine 32 6

month old infants in each country. Unfortunately there are no "pre" measures, that is, no measures taken at 1 month. But at 6 months the US infants discriminated the English /i/ but not the Swedish /y/); the Swedish infants discriminated the Swedish /y/ but not the English /i/. Am I correct in my PCT interpretation that the infants from birth to 6 months are storing a lot of sound perceptions, that those which are stored most frequently and are more likely to be called up to serve as reference signals against which to judge what is hear in others' speech (and subsequently as reference signals against which to judge what one hears in one's own speech)? I would appreciate an expert's judgment of what I think is relevant research on a matter of some importance to PCT. What say Bruce? Gary? Avery? Joel? Others?

Date: Fri May 01, 1992 12:30 pm PST
Subject: Rvsd For the Linguists

For the Linquists

Bruce Nevin's recent reference to Jerome Bruner's wonderful book, Child's Talk, reminded me of a question I have had for the last two or three months. I would appreciate an expert PCT assessment of research reported in the 31 January 1992 issue of Science by Patricia K. Kuhl et al. It is my understanding that all human infants can make the full range of sounds upon which every known human language is made. Kuhl reports that all human infants can, at birth, also discriminate all those sounds.
However, by hearing only the language spoken by their caretakers (parents?), they gradually come to be less capable of recognizing that full range of sounds and increasingly capable of discriminating the sounds of their parents' language. Kuhl refers to these latter as "phonetic prototypes" and suggests they work like "perceptual magnets" which result in other similar sounds being perceived in the same category as the prototypes. Kuhl et al used "computer-synthesized prototypes of the American English /i/ and Swedish /y/ vowels" plus 32 additional similar but not identical variants. The same computer equipment, speech testing devices, and personnel were used in the USA and in Sweden to examine 32 6-month old infants in each country. Unfortunately there are no "pre" measures, that is, no measures taken at 1 month. But at 6 months the US infants discriminated the English /i/ but not the Swedish /y/); the Swedish infants discriminated the Swedish /y/ but not the English /i/. Am I correct in my PCT interpretation that the infants from birth to 6 months are storing a lot of sound perceptions, that those which are stored most frequently and are more likely to be called up to serve as reference signals against which to judge what is hear in others' speech (and subsequently as reference signals against which to judge what one hears in one's own speech)? I would appreciate an expert's judgment of what I think is relevant research on a matter of some importance to PCT. What say Bruce? Gary? Avery? Joel? Others? The full reference:

Kuhl, Patricia A., Karen A. Williams, Francisco Lacerda, Kenneth N. Stevens, Bjorn Lindblom.1992. "Linguistic experience alters phonetic perception in infants by 6 months of age." Science 255 (January 31, 1992): 606-608.

Date: Fri May 01, 1992 2:52 pm PST
Subject: Language Competence in Isolation

Avery Andrews (920439.1358)

>On the basis of other's and my own experience of the
>relationship between interactive exposure to a language and progress
>in acquisition, I'd tend to conjectyure that they have almost nothing
>to do with each other

Did I miss something? Are you conjecturing that interactive exposure to a language has nothing to do with its acquisition? Does it follow that the offspring of competent language users could be raised in linguistic isolation and would, without interactive language exposure to competent speakers, acquire a language and become competent users? Is this a categorical rejection of what Bruner calls the Language Acquisition Support System?

Date: Fri May 01, 1992 4:07 pm PST
Subject: language

[from Joel Judd]

Apologies for the garbage message that predates this one. The @&!*^$@)_$*& server for students and recently ex-students conveniently goes down every Friday at 5:00pm when there's noone around to complain to, so I tried switching to the faculty server to send.
******
Bill C.--please send me a post so I can copy your address, I just replied without copying it yesterday.
******
Avery, Bruce (920501)

The aside comment to Rick about to where to REALLY look for language control reminded me of something I think about when it comes up in a language class, but never write down.

A big and I think overlooked aspect of native language use is how native speakers know to ask questions about utterances they didn't quite get, or want repeated. As a quick example, suppose I said something like the following:

"Yesterday I walked too close to a construction site and got my ear lopped off."

Now, depending on the context, of course, you might ask something like:

"You got your WHAT lopped off?" or
"You did WHAT?"

or something else which reflects what we know about the relationship of the constituents. Almost unfailingly (and this is why I should keep notes), early learners, middle learners, even some pretty proficient learners will do things like just start to repeat back the sentence:

"Yesterday I walked....."

or just start picking salient words out and repeating them:

"Yesterday....?" "Construction what...?"

Do you know what I'm talking about? I can come up with some other examples if it's not clear. This communicative technique seems to be one which takes a long time to develop, regardless of the proficiency of the learner. Is there a germ of an assessment experiment here?

Date: Fri May 01, 1992 4:07 pm PST
Subject: Language Competence in Isolation

>>On the basis of other's and my own experience of the
>>relationship between interactive exposure to a language and progress
>>in acquisition, I'd tend to conjectyure that they have almost nothing
>>to do with each other

>Did I miss something? ...

Yes - the intended antecedent of `they' is acquisition and correction, not acquisition and interaction. So I'm assuming that interaction matters and correction doesn't, at least not much.

Avery.Andrews@anu.edu.au

Date: Fri May 01, 1992 4:08 pm PST
Subject: Nirvana, mysticism,welcome, language.Wow.

[From Bill Powers (920501.1430)]

Greg Williams (920430) --

> How about this way: DURING ecstasy, one is AWARE OF no reference
>signals, whether they are met or not -- perhaps because awareness is
>focused above all reference signals. Certainly, homeostasis goes on, >even during ecstasy!

I don't know about "ecstasy," or even if I've experienced the much advertised and sought-after states of mind referred to in the mystical literature. All I know about is what happens when you keep going up levels and find, finally, that there's no more. I would describe the experience as observation without judgment or interpretation. But this doesn't mean that you're unaware of reference signals or perceptual interpretations -- they're all laid out in front of you to examine if that's what you do. But there's nothing to conclude about them -- they're just there. The whole system goes right on working. Even your personality goes right on working. You're just not in it at the moment.

The reference signals (and perceptions) you're aware of are at a lower level. They aren't a problem. The "attachment" doesn't have to do with them. Attachment, I think, is the viewpoint(s) you're occuping but are unaware of occupying. The desires are the reference signals that you project into the world of experience as values, not realizing that you're

putting them there and that they're not objective. You don't have to get rid of them; you just have to realize who's creating them.

I feel uncomfortable talking about this because there's an implicit claim that I've stumbled into enlightenment or know the secret of the great mysteries. That's not how it is. Insights are the hierarchy being surprised. The experience I'm talking about, to quote Susan Gulick from an early CSG meeting, isn't a big "AHA!," but just sort of "oh." All the big aha stuff is the lower levels thinking "Goody, now I AM perfect."

Of all the people I met who've claimed or implied attainment of some unusual state of being, the only ones I've believed have been those who seem to treat the whole thing as sort of amusing. The ones who spin fairy tales about the experience and embed it into some elaborate mystical system concept are just commercializing it. I don't think they know what they're talking about. The basic route is extremely simple. Go up a level. Keep going up a level, when you remember to try, until there's nowhere left to go.

I might as well unload what I think of extra-normal experiences. I don't for a second doubt that they happen, but I don't think they're of much importance. Before you consider any happening "unusual," you have to get rid of the desire to make it so. Then you can - unattachedly -- consider how many unusual happenings there must be every day on a planet with 5 billion people living on it 24 hours a day. Certainly, "ordinary" explanations of many happenings would require very unusual circumstances to occur at the same time. If six unusual conditions would have to hold simultaneously, each one having a probability of occurring on a particular day of one in a thousand, the probability of all of them happening at once in one day is one in a billion (10^9). That means that on the average, this coincidence happens five times a day to someone on the earth -- that's over 1800 times a year.

I don't think that people who enthuse over unusual phenomena try very hard to distinguish something truly unusual from a merely unlikely but perfectly ordinary coincidence. Coincidences that can occur and be witnessed on a given day only one time in million must happen 5000 times a day, somewhere. And collectors of stories are very likely to hear of them, because nobody who experiences them believes they could have happend by chance. Why, it's a million to one against such a coincidence! When you're the one who experiences it, it doesn't seem rare at all.

My other point is that few people have any model of the brain, or any conception of what a brain can do to provide itself with imagined experiences. The imagination connection feeds information into the perceptual channels, when it's connected, just as if it were coming from the sensory organs. A person can generate internally any experience and any reaction to any experience that is -- imagineable.

When most people say "You're imagining things," they mean that you're a little crazy. But a control theorist would just say, "Of course I am, all the time. Everybody is." Most of what you think you're experiencing as an objective world is being filled in by your imagination. The imagined parts look just as real as the sensed parts.

It's perfectly possible for a person to experience coherent, repeatable, and extended internal scenarios, and even to learn how to get them started

at will. If the person is unaware of the extent to which a brain can manufacture experiences for itself, the only logical explanation is that this person is in contact with something supernatural, beyond the scope of ordinary experience. Any suggestion that people are internally generating these experiences, of course, leads to denial and even anger, because it seems that one is questioning the balance of their minds, or some such old-fashioned phrase. Not so. One is only questioning their understanding of what a perfectly normal brain is capable of doing.

Nobody can say that we have exhausted all possible knowledge, or that our current concepts of perception cover all that will ever be discovered. But there's a difference between being open-minded to possibilities and being gullible. There is no trait of gullibility, of course, but there is the desire to believe something is true. When one desires to believe something, the brain is admirably equipped to come up with all the evidence (and the means of interpreting it) that is needed to support the belief. When you get used to seeing how your own brain handles reference signals at the higher levels, you will no longer be surprised to find your wishes turning into realities, even if most of the reality has to be constructed from inside.

The only way to handle possible instances of paranormal phenomena is to become acquainted with your own desires before even looking at the evidence. Do you want it to be true? Do you want it not to be true? If it's true or if it's not true, will this damage your relationship with someone? Reinforce or deny something else that matters to you? Support or deviate from some other belief?

I don't think a person can evaluate a paranormal phenomenon unless that person simply doesn't care whether it's real or not. I have met very, very few people who don't care one way or the other. Those whom I have met or whose works I have read who seem unattached (or know how to get there when it's called for) feel just as I do: they don't care, either. They don't think its very important. Most of the stuff that is important is far easier to check up on, and happens not just a few times in a million, but every second of every day. And most of it we don't understand any better than we understand paranormal phenomena. You think ESP is hard to explain? Try explaining how we recognize a friend's face.

Yi P. Huang (920430) --

Welcome to CSGnet. Your combined interests in control engineering and the life sciences will fit in very well. You ask about two ways to go:

1) To continue E.E. studying & try to join control theory with other realms in my research work.
2) To study other subject,such as socialogy,psychology,history,etc., & use control theory to handle.

I would urge you to continue your studies of control engineering, while accumulating knowledge about the other fields any way you can. As you become familiar with the basic theoretical position that's popular on CSGnet, especially as it's used by people in the life sciences, you'll see that most other disciplines in the life sciences will change greatly once control theory beomes widely accepted. You could put in a lot of effort becoming an expert in some existing branch of the life sciences, only to find ten or fifteen years from now that nothing you were taught is believed

any more. This is not likely to happen (at least to the same extent) in control engineering. I don't think that modern control engineering courses give students a very good understanding of control, but the basic methods of quantitative analysis will always be useful and will provide valuable background for anything you do to apply control theory in the life sciences. If you have any choice, I would recommend that you focus on analog methods of control, not so much digital methods (even though they're very popular now). And learn all you can about quantitative simulations, because modeling the brain is going to rest very heavily on testing theories through simulations.

Also -- a very practical matter -- if you study control engineering you will be able to find a good job without much trouble, and will then be able to expand your interests without worrying about how to make a living. If you start enquiring soon, you should be able to find places in the U.S. to continue your education in ways that will include both control engineering and studies of living systems.

I envy your future, young friend. What an exciting time you are going to have!

One last comment, about "correcting."

I think that two aspects of control are being confused here. One is the kind of "correcting" of speech that is done to other people (as Avery Andrews says, in the most soul-destroying way possible), or that one consciously does to oneself. The other is the automatic and necessary kind of error-driven action that is at the basis of all controlled outcomes. The latter kind happens because of the way the nervous system is organized, and works as it does whether or not we are consciously attending to it (or even CAN consciously attend to it).

At a superficial conscious level, we can try to formulate the mechanisms of speech understanding or production as a verbal or symbolic set of rules and algorithms. The sentences we speak or the symbol-manipulations we emit or program into a computer DESCRIBE a way of generating utterances that may or may not be equivalent to the way that actually exists (even computer instructions are merely equivalent to the underlying processes in the computer). But these descriptions are not the neural mechanisms that actually do the work. They are PRODUCTS OF those neural mechanisms. You could study those descriptions as instances of the underlying machinery at work.

If one has studied linguistics for a long time, the descriptions may in fact be closely related to the underlying processes. They will, of course, be wrong in some respects, and in some respects are sure to be wrong (the descriptions are verbal but the mechanisms are neural). If one hasn't studied linguistics, then one may still know some verbal descriptions or rote rules expressed as word structures that are somewhat equivalent to the workings of the actual mechanism. But such casually-acquired verbal descriptions are quite likely to be different from the rules actually in effect, and to miss most of the actual rules. So an uninformed person, as Avery has noted, is probably not a good authority on the organization of that person's verbal systems, just as that person is probably not a good authority on how his or her digestive system works, although that person digests as expertly as anyone else does.

The Test for the Controlled Variable has one nice feature: you don't have to communicate verbally with the system being tested. Applying the test to verbal productions may seem self-contradictory in that regard, but it isn't. What you do is look for actual instances of sentence-correction while sentences are being produced, if necessary inserting difficulties or disturbances that make corrections necessary. What you DON'T do is give the person a questionnaire and ask that person to describe the rules of speech that the person is capable of putting into words (correctly or not). You study the actual process of speech production and error correction while it is going on. This will directly show what is considered an error and what corrected form is considered OK by the person, no matter how the person might describe or misdescribe the rules actually in effect. If you then ask why a person corrected certain errors, the answer in many cases (with non-linguists) will be that it just sounded wrong, and sounded better the other way. That's probably the kind of answer you want, because it shows that the correction was made by the actual machinery and isn't just a guess at a verbal generalization. I suppose the subjects you want for such experiments would know nothing about grammar or syntax, but would still speak well.

Anyway, "correction" at a superficial verbal level is not the same as "error-correction" in an underlying control system. The latter is what will help us model linguistic control systems.

The man from the telephone company who brought his big machine in and finally buried my new telephone line is from Alamosa. Alamosa turned up all winter on the TV weather reports as the coldest spot in the country. The man said, "It's not the worst place to live in the world, but you can see it from there." I hadn't heard that one.

Best to all, Bill P.

Date: Fri May 01, 1992 4:17 pm PST
Subject: child phonology

[from Joel Judd]

Clark (920501)

Re: the Kuhl et al. paper. I don't know how "expert" will be this assessment of the Kuhl paper, but I can tell you what I like and don't like about it.

I like the authors' emphasis on experience and not "meaning" as crucial to phonetic learning, although at the end they say "Infants demonstrate a capacity to learn simply by being exposed to language during the first half year of life..." which smacks of "blank slate" philosophy. The importance of a learned prototype in explaining older learners' difficulties also seems to make sense, and could be carried further to help explain why simply passive listening (the Silent Way techniques such as the Marvin Brown anecdote mentioned awhile back) is also not sufficient in developing a L2 phonology.

What I don't like (perhaps predictably) is their reliance on a cross-sectional design for something that, given a mere six months of time and some $$$$ (which they obviously didn't lack--see bottom middle of page 607 where they make it clear that all experimental equipment and three

people were transported from Washington state to Sweden), they could have looked at a number of infants longitudinally (a test they admit needs to be done in footnote 12). Because of this, they must rely on statistical significance instead of saying something like "every English child went from no reaction to either prototype to reacting to /i/ and its prototypes, and every Swedish...etc." or something like that.

If you look at the percentages for each ring, instead of the averages they printed, the differences don't seem quite so impressive. For example, the English infants equated what looks like 69-70% of first ring variants with the /i/ prototype, the Swedish infants equated what looks like 63-64% of the variants. The differences become greater as the variants become farther from the prototype, but this shouldn't be surprising since they apparently overlap phonetically.

In case anyone reads the summary of the article on page 535 by M. Barinaga, I think she misses the boat by trying to read into the study something the authors are providing evidence against--needing meaning to develop phonological systems--and in addition asking a blatantly encodingism question at the beginning: "How do infants manage to sort through the jumble of spoken sounds bombarding them and tease out the ones that encode meaning?"

The answer, though I'm not the one to elaborate on it, is: they don't!!

I think Clark is probably accurate in saying that children develop [SENSATION/CONFIGURATION] reference signals for those sounds which are heard in the environment, although there must an interactive component to this development, as Molly Mack, a language professor here, has shown that bilingual children develop "compromise" phonologies to use with similar sounds in their two languages--acoustically neither L1 nor L2, but "native-like" in both languages.

There are several interesting issues here but I've said enough.

Date: Fri May 01, 1992 4:23 pm PST
Subject: Riots, language control

[From Rick Marken (920501 17:00)]

Rick here, from riot central.

Spent the day at home today -- work cancelled due to "civic unrest". Boy, are you social psychologists (and sociologists) missing some interesting interactions between living control systems.

I am motivated to begin another thread on social control -- but frankly, I'm a bit shaken now. So let me make some quick comments on a couple of linguistics posts -- and then R&R for a night. Suffice it to say that I want to talk about the fact that people don't think they are controlling other people when they are. For example, I have heard it said that it is not a control strategy to give people the option of working or living in poverty -- it's their choice. I think this is disingenuous; and ultimately hurtful. But it does sound fair and humane -- not

like control. Just like operant conditioning really -- you can press the bar or starve, it's your choice. We even can be nicer and give you many ways not to starve besides pressing the bar; what could be fairer?

Avery Andrews (920439.1358) says:

>> But the fact that it [correction] happens AT ALL
>>is evidence that people do (or, at least, can) control "linguistic"
>>type variables. People must be controlling many variables at the
>>same time when they speak (or type).

>But it doesn't follow that the processes whereby this kind of conscious
>`corrective-control' is achieved have anything much to do with normal
>language-use.
I'm not sure I understand. If you mean that the process of generating language is not a control process, then I heartily disagree. If you mean that people are probably controlling different variables than the ones I suggested when they actually generate language -- then I heartily agree.

Bruce Nevin says:

>(BTW, Rick, this is the sort of place to look for control, not in the
>enforcement of rules learned in school.)

Same kind of problem I had with Avery -- what do you mean? When someone enforces rules learned in school, that IS control (or attempted control). I have tried deperately to use "that" and "which" properly -- I can't do it but I would control it if I could (because I want to write papers that follow accepted grammatical style rules). Control is occurring. So, if you are saying that the enforcement of grammatical rules learned in school (by the speaker or hearer) is not an example of control, then I heartily disagree. If you are saying that when people speak that are probably not controlling for the rules they learned in school then I would say -- yeah, probably a lot of the time what is controlled is not a rule learned in school. But, as we speak, my daughter is typing a paper for school and I am sure (I could test it) that she is busy trying to control, as best as she can, some of those grammatical rules; she will even ask me (a lousy source) or her mother "how to say something". I think she is asking me to articulate a program level reference regarding word order for her. She is controlling.

We control all kinds of stuff about language -- even stuff we learn about it from linguists (if we want to -- ie. have a higher level reference that requires doing so).

Hasta luego and may all people be able to control the variables they need to control.

Rick

Date: Fri May 01, 1992 4:51 pm PST
Subject: Nirvana, mysticism,welcome, language.Wow.

[From Avery Andrews (920501)]
(Bill Powers (920501.1430)

On rules & brains: My inclination would be to say that the linguists grammar is supposed to describe something in the brain, perhaps well, perhaps badly. I don't think we can do better than that until somebody figures out how to get neuron-like elements to perform sentence-processing in a realistic-seeming matter.

control: but how to introduce appropriate disturbances into sentence generation?

Avery.Andrews@anu.edu.au

Date: Sat May 02, 1992 9:19 am PST
Subject: Language

[From Bill Powers (920502.1200)]

Avery Andrews (920501) --

>control: but how to introduce appropriate disturbances into sentence
>generation?

Some kinds occur naturally. "I want the one with green stripes -- I mean the towel, not the washcloth." This suggests that as the meaning of the first part was perceived by the speaker, it applied to too many things in the visual field, requiring a refinement to narrow the choices. If you then asked the person "What?", the person would probably NOT repeat the original utterance, but say "I want the towel with green stripes." So you get a look at the error and the corrected version. Any time a person adds "I mean ..." there is probably an error correction going on.

Other kinds occur in interactions. You say, "Yoo-hoo" and someone looks back at you. You say "Sorry, not you, him -- hey, you with the hat!" meaning not with just any hat, but the hat with the huge purple brim and a stuffed parrakeet on it, of which the wearer is perfectly conscious as is everyone else. This isn't a correction OF language, but a correction of a wrong result from using language.

I think scene descriptions or comments on stories might be a source for seeing corrections occuring during language production. EXPERIMENTER: John and Jane went to Alice's house for dinner. John bought a bottle for a present but Alice doesn't drink. Jane apologizes to John because she knew John was going to bring a present for Alice and she knew Alice doesn't drink but John does. What does Jane say? SUBJECT: I should have told you that Jane didn't drink before you went to the store. I mean doesn't drink. I mean I knew she doesn't drink and I should have told you that before you went to the store.

As a linguist you probably have a lot more examples of things that are somewhat tricky to say or describe. One problem comes up when all you know about someone is an irrelevant tidbit or two and you want to refer to one of the tidbits. EXPERIMENTER: You know of a man who has a green car and a dog, and of another man who has a white car and a dog. How do you state that the first dog has fleas? Unless the person has already solved this

problem you might get "The man with the green car's dog has fleas." On asking "Green car's dog?" you will probably get a correction, or you might get one spontaneously if the person's listening for unwanted meanings.

I don't think you'll find many corrections in sentences like "Jane likes John" unless it's a factual error. About the only syntactical mistake you could make would be to reverse the order (in English). Also, you can't know whether there was a mistake unless the person corrects an utterance. If the person says "He would have done it if he was wise," this isn't an error of production unless the person understands subjunctives and corrects "was" to "were." I think we have to distinguish deviations from social norms in general from deviations from what the person has ACCEPTED as a norm.

For any one person, general rules of grammar and syntax mean nothing unless they've been installed in that person's perceptions and reference signals. This is what's wrong with trying to generalize about "language" as if it were a single thing with an independent existence. When you study language in general you get an average over a large number of informants. But this average usage of language doesn't tell you how a particular person uses it. It's the same old statistical problem I gripe about occasionally. For any given person, language is the way THAT person uses words, not "the way people use words."

If I were a linguist (no remarks, please) I would begin by studying how ONE person uses language with me. Then another. Then another. In each case I'd look for the control processes involved, by learning how that person corrects errors -- misunderstandings, misstatements, unfinishable sentences, violation of formal rules that the person usually uses, and so on: errors of all kinds. With each person I'd get some grasp of the rules that person follows, the phrases that are simply set sequences, and whatever else I could learn. Only after having done this with a lot of people would I start to ask what all these people have been doing that's the same. And by this I don't mean what general concept would cover all the specific things they've done, but what processes have appeared in EVERY person in EXACTLY THE SAME WAY.

Language conventions are things people are SUPPOSED to learn, but they don't all learn all of them and they don't learn them in exactly the same way. The way around these variations isn't to generalize them away, but to learn how people manage to communicate in spite of them. The more I think about it, the more it seems that a study of language as a thing in itself divorced from individual speakers and listeners is misdirected. But I'm used to being a minority of one.

Irrelevant afterthought department:

The Golden Rule can also be stated: what you send around comes around. Or cast your bread upon the waters and you'll have a soggy sandwich for lunch (well, that's really Karma). It isn't that you should treat other people in a certain way to make them treat you that way. That isn't how it works. The way it works is that people will react to you pretty much the way you disturb them. Push not lest ye be pushed back upon. As most people cite it, the Golden Rule is a means of controlling other people.

Best, Bill P.

Date: Sat May 02, 1992 6:14 pm PST
Subject: language, grammar

[Avery Andrews 920502.1856]
(Bruce Nevin 920501.1045)

>Are you dispensing with the distinction between ungrammatical but
>meaningful utterances like these and the (purportedly) meaningless but
>grammatical "colorless green ideas sleep furiously."

No. I'm suggesting that normal grammatical processing is different from the process whereby people might understand syntactically distorted language (before they got used to it, and changed their grammar to suit). I'd assume that unconscious correction of the input is a a consequence of controlling for `perceive a well-formed sentence-structure', which can to some extent be achieved by overriding/overrwriting the input (keeping in mind that the input is frequently unstressed, garbled, etc., so this over-ride/write facility is perhaps very useful even when the input is well-formed, as far as it goes.)

Avery.Andrews@anu.edu.au
(currently andrews@csli.stanford.edu)

Date: Sun May 03, 1992 8:46 am PST
From: g cziko
EMS: INTERNET / MCI ID: 376-5414
MBX: g-cziko@uiuc.edu

TO: * Dag Forssell / MCI ID: 474-2580
Subject: Acknowldgement

Dag (direct):

I just want you to know that I haven't forgotten about the start-up document you drafted for us. It's just that in these final weeks of the semester things have been very busy, not only with coursework but also doctoral students trying to get their dissertations together to defend before faculty start disappearing for the summer.

I also want to acknowledge receipt of paper and cassette and will send you my course syllabus soon. I haven't had time to look over the paper carefully yet; hopefully things will quiet down in a few weeks giving more time to do this.--Gary
Date: Sun May 03, 1992 10:44 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: Standards
Message-Id: 11920504064411/0004742580NA2EM

[From Dag Forssell (920504)]

Bill Powers (920429.0900), Ed Ford (920430.11:44)

>The religious thing seems to be coming up again,....Perhaps what we
>might more profitably do is examine belief as a phenomenon.

Bill is suggesting that we go up a level.

>This phenomenon of belief isn't confined to biology. People arrive at
>firmly fixed belief systems about electron flow, quarks, continental
>drift, natural selection, grammar, etiquette, construction practices,
>and proper forms of music, art, poetry, and dancing. If you challenge
>their beliefs they will defend them. In most cases having to do with
>less material beliefs, the ultimate defense is "I was raised to think
>that ....". And of course that is true, although it doesn't make the
>belief true.

I agree, there is NO difference between BELIEF in what we label religious areas and UNDERSTANDING in what we label secular areas.

I find Bill's post lucid and indisputable. - It hooks nicely into my system of understanding, that is. It is one of those jewels that merits saving. I must get a good indexing system going!

>I can only recommend that others who want to see belief systems as
>objects of study try to see them that way, ... To see them this way
>is not to accept or reject them, or to make them seem less than what
>they are. It is only to see them FOR what they are.

Ed Ford says:

>The problem for me is that to be properly studied, understood, and
>fully tested, a belief system has to be checked out through experience.

Ed, as I interpret your comment, you do not mean to object to Bill's statement as such, but to emphasize the practical difficulty of passing judgement on some specific systems concept.

In your post, you clearly recognize that both PCT and Roman Catholicism are systems concepts. You appear to me to support Bill's post, but you also appear to go beyond it.

You bring up issues of testing and validation of a set of systems concepts. In this you express a point of view that I think is a good subject for discussion. This systems concept debate will not go away, because it is of great interest to many. We are all attached to our individual set of systems concepts. It illustrates the upper reaches of HPCT, which is of great concern to you and me and any others who try to learn from HPCT how to better teach or lead or counsel people.

>I think the standards I've set based on my systems concept, the choices
>I've have made which reflected those standards, and, most important of
>all, the satisfaction that comes from achieving the various things for
>which I have controlled are the real test of a systems of belief.

In my first reading of this I understand you to say that:

Systems Concept =====> Standards

and since the standards work and yield a satisfying life, this validates

the systems concept:

Systems Concept <==== Standards.

However, I believe this last part to be a mistake.

You may not mean the second part the way I interpreted you at first, since you also say:

>...When people are functioning effectively, then what ever they're using
>to drive their system should be given respect.

I do think that the interpretation:

Systems Concept <==== Standards.

or: "My Standards work, therefore my Systems Concepts must be TRUE"

is an unexamined assumption behind most of the Systems Concepts strife we see in the world around us.

I want to focus this post on the standards. Perhaps in that I am "going down a level."

I would argue that the notion of validating or testing the Systems Concept itself is a mistake in the first place. It is not necessary, as you indicate in your last quote above. I respect you as a thoroughly decent human being. I can never study, understand and check out your belief system without living your life from its beginning. (Is this the difficulty you meant)? I do not want to, and it is not necessary.

To think that the standards validate the Systems Concept implies that those STANDARDS that do the validation are UNIQUE to that SYSTEM CONCEPT PACKAGE (read religion). This is the implication I perceive and am debating. Perhaps I am punching a big hole in the air. That's OK too.

I sincerely believe that if there are five billion people on this earth, there are also five billion Systems Concepts (of God and everything else). To a PCT'er it is obvious that the Systems Concepts are individually designed by each person.

Just like we in PCT recognize that a diverse set of objects can with some advantage be categorized as "chairs," so a diverse set of umpteen million individual systems concepts with some common, perhaps even superficial characteristic are called "Roman Catholicism." Other sets are called "Mormonism," "Islam," "Hinduism," "Secular Humanism," "Atheism" etcetera. This is good enough for wars.

It seems impossible to understand another individuals Systems Concepts in anything more than the most cursory categorization, and then we know that we really don't understand very much.

The point I want to make is that many Systems Concepts packages support the SAME standards. Therefore It does NOT follow that your Systems Concept package is validated by the success of your standards.

I would be content to say (I think) that your Systems Concepts are

validated by the simple fact that they are yours. Your Systems Concepts are YOURS and that is ENOUGH.

It DOES make sense to advocate religious freedom, and declare that any religious notion is acceptable, as long as it does not violate important standards that have been agreed upon after more or less public debate over tens of thousands of years (often in the form of wars).

(A personal note: The Thomas Jefferson Research Center in Pasadena, Ca, (818) 792-8130, led me indirectly to CSG. My references in this post and some other neat stuff is available from them).

Here I will insert an excerpt from THE CASE FOR CHARACTER EDUCATION by Frank G. Goble and B. David Brooks. I shall transcribe two pages from

Chapter 7: WHOSE VALUES SHOULD BE TAUGHT?

We sow a thought and reap an act;
We sow an act and reap a habit;
We sow a habit and reap a character;
We sow a character and reap a destiny.

William Makepeace Thackeray

Whose values, people frequently ask, do you propose to teach? Those who ask this question, although they may not realize it, have been influenced by ethical relativism - the idea that there are no enduring ethical values.

When the subject to be taught is chemistry, physics, or astronomy, no one asks whose chemistry? Whose physics? Whose astronomy? It is assumed that the teacher will simply present the available information to the best of his or her ability. Everyone assumes that there is an objective reality about these subjects, in spite of the fact that our understanding of the physical sciences is neither complete nor exact.

The question, whose ethics, implies that there is no objective reality about ethics and this is exactly what the ethical relativists claim.

"Such a position of normalness," writes Professor Philip H. Phenix,

> ....is a denial that there are really any standards of right or wrong, of better or worse, because the whole human endeavor appears to be meaningless and without purpose... If life is essentially meaningless, there is no point in trying to promote or to improve it. An anomic theory of values is fatal to education, as it is to any sustained cultural pursuit. Unfortunately, it is a theory all too widely held, either explicitly or tacitly, and it should be recognized as an enemy of human morale and of educational effectiveness.

The influence of this relativistic, value-free point of view is illustrated by this statement of Dr. Lewis Mayhew in an address given when he became president of the Association of Higher Education: "Colleges are not churches, clinics or even parents. Whether or not a student burns a draft card, participates in a civil rights march, engages in premarital sexual activity, becomes pregnant, attends church, sleeps all day or drinks all night, is not really the concern of an educational

institution."

The problem with this point of view is that it is not realistic and leads to increasing crime and violence and other costly manifestations of social disintegration. There ARE basic ethical principles that are necessary to social progress, and these principles must be identified and taught.

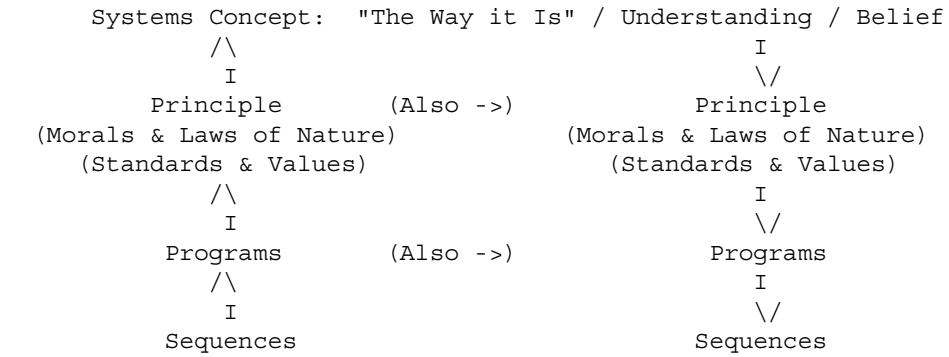
American Viewpoint, whose Good American Program was described in chapter 4, based its program on an empirical code of ethics. The code was developed by writing to hundreds of outstanding citizens and asking their opinions. From this was developed a list of values which had been "hammered out in the anvil of practical experience." The Good American list includes such concepts as conservation, courage, personal health, honesty, initiative, perseverance, reliability, self-mastery, cooperation, courtesy, fairness, respect, tolerance, duty, independence, patriotism, responsibility and understanding.

The American Institute for Character Education, which developed the Character Education Curriculum also described in detail in Chapter 4, based its program on a worldwide study of value systems. This study identified fifteen basic values shared by all major cultures and world religions. These values are courage, conviction, generosity, kindness, helpfulness, honesty, honor, justice, tolerance, the sound use of time and talents, freedom of choice, good citizenship, the right to be an individual, and the right of equal opportunity.

This code of personal values, now taught in thousands of classrooms, has not proved to be controversial.

If indeed the Principles/Standards/Values are what count, and most people on reflection and discussion will arrive at a similar set, it will not be surprising that there is a great uniformity in that area between all religions. In the course of history many creative thinkers / founders of religions have postulated different Systems Concept packages on top of them.

I find it interesting to look at the HPCT hierarchy, which may confirm this suggestion:



Notice that the (SAME) Principles/Standards/Values used to create a particular Systems Concept structure logically could be expected to be derived from it.

It is also possible that a principle taught or experienced "on the way up" is remembered and used "on the way down" without being explicitly

recognized as part of a system of concepts. We experience a lot as we grow up in our families, which stays with us as principles/ values/ standards without deliberate connection with, reflection on, or support by our religious beliefs. The idea that

Systems Concept =====> Standards

does require a deliberate effort to think things through. This should not be taken for granted!

It seems to me that the common inclination (if there is one) to validate your own particular Systems Concept package by the effectiveness of the (common) standards leads to some very unfortunate side effects.

The idea that the Systems Concept package is validated to be (rigid, objective) TRUTH sets the stage for fruitless discussion, fights and wars, since anyone who looks can see that the OTHER GUY'S Systems Concepts package is FALSE. (Heretic is the word, I guess. Death to heretics!!!)

Religions as Systems Concepts packages typically include a whole super structure of baggage in the form of miracles and explanations which at one time probably were designed to sell the package to illiterate, ignorant people and keep them in check. Some of this creates unfortunate standards which prevent people from functioning well.

I have my Systems Concepts which flavor my interpretations. If a God created the Big Bang (today's news), fine with me. I do not recognize a God that can hear me. I think a pastor who tells people from the pulpit that if they pray together in HIS name to put Jello gelatin "salad" to good use in their bodies - and they BELIEVE it - is doing these ignorant people a great disservice. Of course they can pray for healing on Sunday. I have heard enough of this as our family attended church regularly a few years back. We no longer attend.

To me this is part of the baggage that I personally object to as creating misleading and damaging standards. But then, as Ed says:

>It would be hard for my own view or systems of beliefs not to get in the >way of those systems I'm trying to study.

These packages may include some principles/values/standards that are not only misleading but deny people rights we as westerners take for granted. As Ed puts it:

>Does it respect the rights and beliefs of other living control systems?

Consider women's rights under Islam.

Since Islam is TRUE, validated by the satisfaction of Muslim men, how can you question those things?

By going "down a level" and recognize that the Systems Concept is nothing more than a construct in your mind. It is not TRUTH. There is no TRUTH to be had anywhere. It is ALL subjective systems concepts. In a post not too long ago, (which I would like to relocate and re-read, date anyone?) Bill ended a discussion of the levels with the statement: "It is ALL

perception."

I have bared a little of my prejudices here. Everyone has their own. The point is that as I see it, the debate on creation has NOTHING to do with standards; miracles don't matter. A lot of the things we fight over in religion, between religions, against religion and for religion do not matter; they are not essential to justify the PRINCIPLES/VALUES/STANDARDS that DO MATTER.

While I looked at my bookshelf of Thomas Jefferson materials, I was reminded of: THE FIVE THOUSAND YEAR LEAP by W. Cleon Skousen. This book by a constitutional lawyer and scholar spells out the twenty-eight PRINCIPLES which the American founding fathers considered as they formed our government (a Systems Concept!!!). It is very clear from this book that the American constitution is based in large measure on the political writings of Marcus Cicero, which were well known to our founding fathers, NOT on the Judeo-Christian tradition, as we are told often by some religion salesmen.

A nasty thought crosses my mind in regard to some of these salesmen. To paraphrase Hitler's information minister Goebbels: "A point of view repeated often enough becomes the truth." Perhaps Goebbels is another historic figure who clearly anticipated William T. Powers. But then Salomon said: "There is nothing new under the sun."

Happy Principles/Values/Standards everybody!

Dag Forssell
23903 Via Flamenco
Valencia, Ca 91355-2808
Phone (805) 254-1195 Fax (805) 254-7956
Internet: 0004742580@MCIMAIL.COM

Date: Tue May 05, 1992 8:34 am 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: LCS II
Message-Id: 81920505163418/0004742580NA1EM
[From Dag Forssell (920505:0930)

These thoughts came to me this morning during my exercise run in an unusually humid Southern California air. A light drizzle hangs in the air, full of aroma from flowers. What a delight. The net is eerily quiet. Have I been cut off?

LCS II:

Last night, I read the foreword to Living Control Systems II out loud to Christine. I could not get through to the end. I choked and my eyes watered. I am truly blessed to be in touch with CSG and HPCT, a very significant part of my personal systems concepts.

ABOUT systems concepts:

PCT shows us plainly that all our behavior is designed to create or (much more often) re-create perceptions we want. (See Marken's paper: The behavior of perception). From the lowest motor control perceptions to the highest systems concept perceptions.

We perceive that which we want to perceive.

At systems concept level, you can re-phrase that to say: We make come TRUE that which we want to be TRUE.

"Skinners Mistake" was to prove true that which he already perceived to be true.

Skinners mistake is not unique to Skinner. All of us make the same mistake every day. This explains the nature of any discussion of particular beliefs/understandings, academic, religious or otherwise.

Five billion people controlling to confirm that what they already individually KNOW to be TRUE continues to be TRUE.

Progress takes place only when people experience an error signal with regards to a system concept; where it fails to explain or satisfy.

Then, a person is open to consider alternative principles which will adjust the existing system of concepts to a new, revised one.

It has been a few centuries since one person claimed to have and have read all books; to know all knowledge.

Today it is impossible to know it all. Ignorance is the rule. The only question is one of degree and area.

I am comfortable knowing that I am ignorant in vast areas of knowledge. This recognition makes for a sense of wonder and makes it easier to be open to new information in all areas.

A delightful, mind opening, very graphic book that deals with these issues of perception is: INFORMATION ANXIETY by Richard Saul Wurman. 1990, Bantam paperback $12.95. Highly recommended!

The sequel: FOLLOW THE YELLOW BRICK ROAD; Learning to Give, Take & Use Instructions (Bantam hardcover 1992, $24.50) is more specialized. (I have not looked closely at it yet, and have no opinion).

Best 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 May 05, 1992 8:46 am PST
Subject: Model Arms; welcome aboard

[From Bill Powers (920503.1300)]

(to Chris Love, cdlove@ben.dciem.dnd.ca, who works with Martin Taylor)

Hello Chris,

Welcome to CSGnet. That's all there is to it. To sign on, you send a message to this address (from the logon you're going to use):

listserv@vmd.cso.uiuc.edu

The message should look like this:

subscribe csg-l lastname, firstname, location
[total of <= 78 characters]

When you send this message, the listserver will automatically pick up your return address from the electronic header and put you on the subscriber list. You don't have to ask anyone's permission -- just do it. You'll get some introductory messages.

I'm posting this to CSGnet as well as sending it to you; there are other modelers on the net who will be interested in your reply, so we can conduct this conversation in public (if you don't mind). Perhaps your first post to CSG net could be your message to me, followed by your reply to this.

Details: Our custom is to insert by hand the name of the sender and the date/time sent, as in the first line of this message. We use European-style dating: year, month, day. When quoting replies, I copy them into the text and put ">" before each line by hand. Some mail systems do that for you automatically, but I write on a word-processor off-line, and do it by hand. You can do it any way you want.

>Only today though have I been able to make my "little baby" follow the
>target about. He does this very slowly though, but at least he moves
>his finger in the right direction!

Congratulations, proud papa. There's no thrill like seeing your own brain-child start to work properly.

I've gone a long way down this road; my current version simulates muscles with stretch and tendon reflex, a higher level of kinesthetic control, and a visual system that detects target and finger position in three dimensions by optical ray tracing (of a very simple sort, but legitimate) and uses the kinethetic systems to place the fingertip on the target or follow it around (or even draw circles around it). At present the model is running at about 1/5 of real time on my 10 MHz AT-compatible. I'm using a full kinematic model for the arm, with two degrees of freedom at the shoulder and one at the elbow. At this very moment I'm sweating out a system for learning the correspondence between the shoulder-based kinesthetic control actions and the eye-based visual perception of positions (turns out to be a lot harder to do than to describe).

My model, like yours, has the Little Man pinned to a stationary position, with only the arm moving. The head can actually also move to keep the target centered visually, but the head movements turn out to cause trajectories of arm movement that deviate unacceptably (to me and Greg

Williams) from published trajectories of free movements in real arms. I could put in a visual compensation system, and will eventually do so (or Joe Lubin and students at Princeton will), but for now I've frozen the head and solved the problem that way.

>Martin told me before he left on his trip that you have found
>an effective way to move the arm quickly without running into the
>oscillation problems due to overshoot. Do I have my information
>correct, and if so, is it difficult to explain?

Before I start giving you a lot of unnecessary advice, perhaps you'd better fill me in on the organization of your model. I wouldn't want to waste your time telling you a lot of things you already know. Yes, my model is quite stable and can move rapidly (in simulated time); a one-radian movement to a settled position can occur in 0.1 simulated second (dt in the model is 0.01 second). That's at the kinesthetic level. Under visual guidance, the movement takes about 0.3 seconds min. Of course it can move more slowly. With all the parameters trimmed properly, it's a critically-damped movement, with no overshoot. Of course real people do show some overshoot when they move fast, so in simulating real arm movements we wouldn't use the optimum parameters.

Glad to have you aboard.

Best, Bill Powers

Date: Tue May 05, 1992 9:45 am PST
Subject: social control and LA riots

From Ken Hacker

In response to Rick Marten's post of May 1, 92:

This was an excellent catalyst/disturbance for elaborating the linkages of social influence and control (always personal). I was a bit shocked to see that you were referring to "social control." As I recall, earlier on this hotline, social scientists were broadsided for suggesting such a concept. I myself admitted that social control is an oxymoron, that in a technical sense, control is only within individuals, i.e. perceptual. Thus, I suggested with others than "social influence" is what some of us really are getting at. But your statements about LA rioting bring up some political issues about CSG which cannot be swept away with platitudes about individual perception being individual perception. If we tell people to work or live in poverty, what kind of effects to we have on their behaviors. You frame it as giving them options. I think that is a key formulation. The option giver cannot control the receiver, but can strongly influence and reduce the choices of the receiver.

Some key political issues are

a) Is the giving or restraining of options a form of power which affects control processes in such a way as to favor the giver/restrainer?

b) How is the work/live in poverty option different from Powers' famous give money or die example? In both cases the person can CHOOSE.

c) Are we confusing choice of behavioral options with freedom of creating one's paths of action?

I suggest that Powers is totally correct in stating that control is only (technically) in the realm of individual perception, but that control is heavily affected by social influence.

To fully describe and explain control, therefore, it is necessary to explain where the essential variables and references levels of individuals meet the norms, rules, and constraints of culture.

Thank you for an inspiring note, Rick. Ken Hacker

Kenneth L. Hacker, Ph.D.
Dept. of Communication Studies
New Mexico State University
Las Cruces, NM 88003

Date: Tue May 05, 1992 9:48 am PST
Subject: comments on social influence,riots

Ken Hacker [920504]

Those of us who study human communication from a cognitive point of view, are concerned with how cognitive representations diverge or converge during social interaction. Rick's comments about the riots stimulated me to think of how two extremes of explaining human behavior are both false. One says that people are determined by external factors (behaviorism, cultural studies). The other says that people are purely determined by internal factors (cognitivism, some approaches to perception). Could it not be that the study of social interaction as the interplay of control systems and the study of control systems as the perceptions and regulation of social interactions could lead to better theories of both perception and social interaction?

Concerning control and influence, I see clear conceptual distinctions which I have learned on this hotline. Yet the two are related, albeit in ways not yet understood. Control is much more total and personal:

Control	Influence
forcing	persuading, coercing
total perception	shaping, constraining decoding
internal reference	external reference

I know I am speculating, so I will sign off now. Thanks again for some great food for thought regarding the situation in LA.

Ken Hacker

ORIGINAL NOTE RESPONDED TO:
[From Rick Marken (920501 17:00)]

Rick here, from riot central.

Spent the day at home today -- work cancelled due to "civic unrest". Boy, are you social psychologists (and sociologists) missing some interesting interactions between living control systems.

I am motivated to begin another thread on social control -- but frankly, I'm a bit shaken now. So let me make some quick comments on a couple of linguistics posts -- and then R&R for a night. Suffice it to say that I want to talk about the fact that people don't think they are controlling other people when they are. For example, I have heard it said that it is not a control strategy to give people the option of working or living in poverty -- it's their choice. I think this is disingenuous; and ultimately hurtful. But it does sound fair and humane -- not like control. Just like operant conditioning really -- you can press the bar or starve, it's your choice. We even can be nicer and give you many ways not to starve besides pressing the bar; what could be fairer?

Date: Tue May 05, 1992 10:28 am PST
Subject: Test Message

[from Gary Cziko 920504.2200]

Can it really be that no one on CSGnet has had anything to say for two solid days?

I suppose it's possible, but I suspect a network glitch. This test message

Date: Tue May 05, 1992 10:46 am PST
Subject: Psychology and CT

[From Marcos Rodrigues]

It seems that psychology is getting ready for a u-turn. Harnad has accepted for publication in his PSYCHOLOQUY Refereed Electronic Journal a paper with an interesting paragraph. The author is Bruce Bridgeman, Dept. of Psychology, Kerr Hall UCSC, Santa Cruz, Ca. 95064,(408) 459-4005, bruceb@cats.ucsc.edu, "ON THE EVOLUTION OF CONSCIOUSNESS AND LANGUAGE". Many outrageous statements, but the last paragraph reads:

"4.7. As this preliminary analysis shows, the reorganization of psychology around plans will also require a reinterpretation of neurological function. The organization of plans becomes the central business of much of the brain, other regions being concerned with their execution and with providing the sensory information needed to make them successful."

If psychologists accept this paragraph, perhaps Bill, Rick, or anyone else could write an article to PSYCHOLOQUY explaining the exact meaning of "plans" and how the brain executes them.

Best regards, Marcos.

Date: Tue May 05, 1992 10:54 am PST
Subject: test

[From Rick Marken (920505)]

Just a test to see if any mail gets through. I haven't seen things this quiet in a long time. Makes me realize how important it is to have Martin Taylor around.

Have a happy cinco de mayo. Rick

Date: Tue May 05, 1992 10:57 am PST
Subject: language learning

[From: Bruce Nevin (Mon 92044 13:46:51)]

(Clark McPhail (Fri, 1 May 1992 14:56:28)) --

>Am I correct
>in my PCT interpretation that the infants from birth to 6 months are
>storing a lot of sound perceptions, that those which are stored most
>frequently and are more likely to be called up to serve as reference
>signals against which to judge what is hear in others' speech (and
>subsequently as reference signals against which to judge what one hears in
>one's own speech)?

I haven't read the article, but will. In general, the above is what I would expect and consequently is the interpretation I would try to make, however "objective" I might try to be. Joel's comments seem on the mark to me, except the maybe palliative observation that you have to begin a longitudinal study somewhere, and need not wait until it is completed to publish initial results. I gather (hope) that is the intent. Kenneth Stevens is generally pretty careful. Of course, he has his theoretical presuppositions too.

(Joel Judd (Fri, 1 May 1992 18:26:41)) --

>Now, depending on the context, of course, you might [use wh-questions]
>
>early learners, middle learners, even some pretty proficient learners will
>do things like just start to repeat back the sentence . . .
>or just start picking salient words out and repeating them . . .
>This communicative technique seems to be one which takes
>a long time to develop, regardless of the proficiency of the learner.

I take it you mean second language learners.

>Is there a germ of an assessment experiment here?

I expect it would be an effective diagnostic if their attention is on the content and not on the means they use for asking questions.

(Avery Andrews (Fri, 1 May 1992 14:51:12 PDT)) --

>I'm assuming that interaction matters [for language acquisition] and

>correction doesn't, at least not much

There is explicit correction in the mother-child interactions Bruner and his students studied.

There is both explicit and implicit correction in most other language interactions between children and adults. There is a great deal of repetition of words and phrases in any discourse--patterned repetition is a great deal of what constitutes discourse coherence-- and a great deal more of it in adult-child interactions than in other forms of discourse. The repetition of a phrase by an adult provide a model to the child showing how she ought to have said the phrase. The adult is often unaware of doing this.

Accomodation to others' usage is lifelong. Deferring to the usage of one you esteem or who is in authority (just as child to adult) or for other reasons less clear (the lawyer asking who was them bottles shared between). Or not agreeing in usage (e.g. pronouncing "harass" like "heiress" with an h rather than like "her ass," or vice versa). Holding out calls attention to the difference and people tend to become alert to possible judgments associated with the difference ("uneducated," "snob," etc.) Maybe one person conforms to the other's usage because of agreeing with such judgment (yeah, that's the *right* way to say it), or maybe one conforms to the other's usage so as to avoid occasioning such discomfort, to affirm co-membership, etc. (The lawyer's motive?)

Bruce Nevin
bn@bbn.com

Date: Tue May 05, 1992 11:12 am PST
Subject: paradoxim

[From: Bruce Nevin (Tue 92045 13:30:44)]

(Bill Powers (920418.0800)) --

>>Still, there is something satisfying about the notion that the
>>reflexivity of awareness (and attendant paradoxes) is due to the
>>reflexivity of language as used in the stories we tell ourselves.
>
>What's going on here? What paradoxes? If you examine the self, you
>immediately find that what you're examining is NOT the self doing the
>examining. There's no reflexivity of awareness. You're NEVER aware of your
>current point of view -- only of a previous point of view. Language seems
>reflexive only because we can make up sentences like "This sentence is
>about itself." That sounds as if the sentence, all by itself, can be
>"about" something. It can't. We MAKE it be about something, and if we just
>look at it as a sentence, it ceases to be about anything, least of all
>itself. You have to look at the MAKER of the sentence, not the sentence, to
>see what this "aboutness" is about.

Many people have thought there was such reflexivity. I agree with you, of course they are wrong. My claim was only that they probably mistook the apparent reflexivity of language for reflexivity of awareness. You can talk yourself into some pretty amazing perceptions. And yes, the reflexivity of language is only apparent. Harris's debunking of the

paradoxes works like this (or did in 1968): consider "This sentence is false." When the word "this" is uttered it refers to something that does not exist, since the sentence containing it has not yet been completed. There is no way of knowing whether it will even turn out to *be* a sentence or not. Even for a language that allows the deictic "this" to appear as the last word of a sentence (such as Modern Greek) the sentence does not yet exist for reference at the time that the word of reference contained in it is uttered. This explication rests fairly directly on the distinction you are making between the utterance and its maker, but enables him to leave closed the can of worms occasioned by his not having access to a coherent theory of psychology.

Bruce
bn@bbn.com

Date: Tue May 05, 1992 11:13 am PST
Subject: discontinua

[From: Bruce Nevin (Mon 92044 13:48)]

Bill Powers (920501.1430)

Yeah, unusual perceptions are good entertainment. A central problem is that we can't determine how much of what seems to be going on is a tissue of imagining and ignoring. I assume that the most frequent use of the imagination loop is to fill gaps in perceptual input to match expectations. Given the nature of the perceptual hierarchy and perceptual control, unexceptional, routine perceptions seem more suspect than unusual ones. I'll come back to this. I have to establish my bona fides first.

Shifting levels works as a kind of Jacob's ladder. So do some other techniques. What is essential and invariant? At any level of perception, there is always a point of view. Identifying with the point of view rather than that perceived or the process of perceiving has the effect of shifting up a level only because then another level of perception then comes into view, unobscured by what had been under attention. (Better: unobscured now that we no longer take it as our point of view, but the subjective impression is like the first way of saying it.) The experience of shifting levels is the experience of identifying with one level rather than the other. The point of view or witness is something other than either. One can identify with that without the experience of first identifying with a succession of levels of perception as point of view on the next level below. The unattached witness experienced when looking at the highest accessible level is actually available everywhere and one can identify with it even while looking at level n from level n+1. There is much greater freedom in this.

(Bill Powers (920418.0800)) --

Concerning continuity and discontinuity, I have been a bit at a loss to correlate our respective mappings of words to perceptions. I agree that in general perceptual signals at

>The lower levels of ECS are continuous; only the

>higher ones introduce sampling and discrete variables.

Skipping to a later point in your post, where you quote me saying:

>>I would suggest that one of the "priorities" of the brain (at least the
>>mammalian brain) is precisely to "fictionalize," in the sense that
>>higher levels of control in a sense substitute the perception of
>>continuous constructs for discontinuous lower-level perceptions.

You respond:

>And I claim that you're not looking directly at the world of lower-level
>perceptions when you say that, but at DESCRIPTIONS of that world. The
>descriptions are discontinuous. The world isn't. There's no reason why the
>imagination connection can't exist in any brain at any level (but the
>lowest).

This is mostly aimed at somebody I must have sounded like. I hope I can show that I am not confusing map and territory. I agree that categories we project onto the world are discontinuous, and that we use category perceptions to parse or "punctuate" a world comprising evidently only continua into discontinuous elements. But how do you know that the world is continuous? The world itself, apart from our perceptual signals?

No, even that isn't the right question. How do we know that continuities in sensory signals correspond to continuities in the perceived world? We don't, and we can't. We can and do presume that they do. The last sentence quoted above indicates how discontinuities could be masked at any level. I am not clear what the mechanism might be for evoking the imagination loop--perhaps the input function of an ECS can call for a signal to fill a gap when most of its other requisites are met. Clearly, it does happen. And if this process can happen for any ECS, it can fill gaps in perceptual input from all levels.

But I'm getting the explanation before the experience to be explained. That's why it won't come out right.

Also later in that post, you say

>Perhaps we're thinking of different kinds of continuity and discontinuity.

I think so. Consider the case where you are deliberately attending to one perception for an extended period of time, ignoring others as they come to awareness and returning attention to the chosen perception. (Or, easier, attending just to perceptions of a certain type from a restricted set of sensory input mechanisms, such as touch sensation on the rim of the nostrils.) Interesting things happen when you do this.

First thing you notice, of course, is how hard it is to hold one's attention steady against the normal process of letting attention drift among higher levels waiting for some error signal to become prominent.

Then, with some practice, you notice how very much is going on in the periphery of attention. (Except that any of it is liable to become the new focus when you are distracted once again.) A constant up and down

of perceptions, memories, and imaginings. Certainly nothing constant there except change! Or is the appearance of discontinuity perhaps a function of passing a threshold of awareness? But this is all peripheral to your chosen focus of awareness. There seems to be a gradient "threshold" of awareness, if indeed the term is still applicable at all.

As your ability to stay focussed on a chosen perception sharpens, you notice that the perception to which you are attending is itself not constant. Again, is it the perceptual signal or one's attention to it that changes? Who can say? Does it matter? Whatever the basis, when you look persistently for more than a few tens of seconds at a time, impermanence (inconstancy, even discontinuity) appears to be the rule.

Except for the point of view, the witness. Not itself observable, except indirectly in a sense, reflected in the very process of observing. That is continuous. Seemingly.

I said:

>Close attention to perceptions (e.g. the vipassana meditation
>described, taught, and practiced by various Buddhist traditions)
>discloses impermanence and discontinuity at lower levels of perception,
>masked by the continuity of perceptions at higher levels.

I am suggesting that for purposes of higher level control fluctuations and discontinuities in lower level perceptual signals are filled in out of memory and imagination, so that when they reach the comparator of the higher level ECS it is as though they had been continuous and steady at the lower level. I say this because my experience suggests that with prolonged attention to selected perceptions (with no purpose other than attention, and beyond that some motivation for engaging in the practice, usually verbalized as the expectation that such practice will be eventually in some way beneficial)--with prolonged attention, maybe the input functions of higher-level ECSs give up patching over fluctuations and discontinuities out of memory and imagination. Perhaps I am wrong, and these appearances I report all represent flaws in the lens of awareness. Whatever that is. But even when you attend to one intensity sensation (not easy!), that ECS is not disconnected from higher-level control systems just because your attention excludes them. If higher-level control systems call for a filling gaps in the signal out of memory by way of the imagination loop, is there any way to notice? Perhaps just this way: persisting long enough that higher level ECSs give up and their control ceases to be an active factor.

Caveats notwithstanding, I am trying to point at *experience* that anyone may have, as it relates to hierarchical perceptual control theory.

The experience I am trying to point at does have effects on one and in one, effects that don't arise from projecting categories onto the world of perception or changing one's beliefs and disbeliefs (though beliefs and category perceptions may be affected). In this experience, eventually, whatever it is that one may identify with at one time or another as constituting a self or ego turns out to be part of the soup. This has no more to do with philosophizing than experience using the Method of Levels does, though one can talk at length about either.

In any case, "dissolving the ego" is no great shakes as a plum for the Jack Horner Memorial Enlightenment Prize. Just another thing to be noticed. Oh. Right. There never was anything there to be dissolved. Sure looks like there is, even now. And we may still act as though there is. But now we know better.

Greg (private post, I trust it's OK to quote this):

>my own main interest in perennial-philosophers has
>always been as an amateur anthropologist trying to figure out why they keep
>coming back for more (Hindu self-inflicted wounds, Zen hemorrhoids and
>boredom, and so forth) of what doesn't seem (to me) to be very enjoyable in
>itself.

I think holding out ecstasy as a reward is at best a well intended fraud perpetrated on behalf of those who otherwise would not poke into these questions. Similarly for heaven and hell, etc. Students of comparative religion often make the distinction between exoteric and esoteric teachings. Jesus told some things to his disciples that he didn't tell to others, cautioned against casting pearls, etc. My take is that the direct, personal experience is always esoteric, and anything else, certainly anything one could call religion, is more or less exoteric. That doesn't stop me from being a member of a Quaker meeting and participating in the life of the meeting on terms generally congenial to fellow participants (which, like your mileage, varies). But for me this discussion is not about religion.

Bruce
bn@bbn.com

Date: Tue May 05, 1992 11:37 am PST
Subject: Bridgman Article

[from Gary Cziko 920505.1305]

Marcos Rodrigues (920505) writes:

>It seems that psychology is getting ready for a u-turn. Harnad has accepted
>for publication in his PSYCHOLOQUY Refereed Electronic Journal a paper with
>an interesting paragraph. The author is Bruce Bridgeman, Dept. of Psychology,
>Kerr Hall UCSC, Santa Cruz, Ca. 95064,(408) 459-4005, bruceb@cats.ucsc.edu,
>"ON THE EVOLUTION OF CONSCIOUSNESS AND LANGUAGE".

Thanks for bringing this to our attention. I would also like to encourage CSGnetters to respond to this article. Any of the CSG old-timers could have a "field day" with this one.

It is available on Usenet (NetNews) on the group "sci.psychology.digest." I can send it to anyone directly who cannot access it this way (it is 26 kbytes long).

This is not an opportunity to sneeze at since:

>PSYCOLOQUY is a refereed electronic journal (ISSN 1044-0143) sponsored
>on an experimental basis by the American Psychological Association

>and currently estimated to reach a readership of 20,000. PSYCOLOQUY
>publishes brief reports of ideas and findings on which the author
>wishes to solicit rapid peer feedback, international and
>interdisciplinary ("Scholarly Skywriting"), in all areas of psychology
>and its related fields (biobehavioral, cognitive, neural, social, etc.)
>All contributions are refereed by members of PSYCOLOQUY's Editorial Board.

>This target article has been accepted for publication in
>PSYCOLOQUY. Commentary is now invited. Commentaries should not exceed
>200 lines. Each should have a keyword-indexable title and the commentator's
>full name and affiliation. Please submit commentaries to:
>psyc@pucc.bitnet or psyc@pucc.princeton.edu

--Gary

Date: Tue May 05, 1992 11:38 am PST
Subject: Re: test

[from Gary Cziko 920505.1320]

Rick Marken (920505) says:

>Just a test to see if any mail gets through. I haven't seen things
>this quiet in a long time. Makes me realize how important it is to
>have Martin Taylor around.

I don't want do downplay the importance of Martin Taylor, but the listserver on which CSGnet depends was not on-line Saturday and Sunday.

This happens whenever the listserver's machine starts to run out of spool space (where not-yet-read-messages and files are stored) since the listserver sends lots of message, many of which end up on the spooler and take up more spool space. So it shuts down until space clears up (sort of like a control system).

During these times, messages sent to CSGnet are not lost, just delayed. Since this hasn't happened to often, I believe it is a tolerable situation. If it happens with increasing frequency, it may be a good idea to move CSGnet to another site where this wouldn't be a problem (I could still be list manager).--Gary

Date: Tue May 05, 1992 1:29 pm PST
Subject: Bridgman Article

[From Rick Marken (920505 13:00)]

Gary Cziko and Marcos Rodrigues suggest a CSG response to a paper posted in PSYCHOLOQUY (a Refereed Electronic Journal) by Bruce Bridgeman called "ON THE EVOLUTION OF CONSCIOUSNESS AND LANGUAGE".

I can see why; this paper says a lot of things that sound very consistent with PCT. For example, the first sentence of the abstract says: "Psychology can be based on plans, internally held images of achievement that organize the stimulus-response links of traditional psychology".

Sounds like Bridgeman's "plan" is like a PCT "reference signal". Moreover, Bridgeman seems to understand that "the dominant metatheory in psychology has been the stimulus-response link" and that even "modern cognitive psychology retains a similar orientation...". So Bridgmenan (a professor of psychology at UC Santa Cruz) acknowledges what many critics of PCT deny -- that scientific psychology is built on an input-output model of behavior.

Unfortunately, after the excitement of the abstract and first couple of paragraphs, it becomes clear that Bridgeman's model (to the extent that one can understand it -- the descriptions are quite vague) is an "output generation" model. A plan is not a reference for a particular level of an perception, but a recipe for intended actions. In section 2.6 Bridgeman says "As a plan is executed, a single goal state or idea is unpacked into a series of actions". BZZZZT. Wrong. As we (PCTers) know, there is no way to "unpack" an internally specified goal into a particular set of actions and have any hope that these actions will produce the intended (goal) result with any consistency; actions produce their results in a constantly changing environment so actions MUST VARY if they are to produce the same goal result consistently.

Brigeman does make statements that suggest that, somewhere in the back of his mind, he knows that his "plans" must be plans for perceiving, not acting. In section 2.7 he says that plans must be monitored -- which could mean that their sensory consequences are important. But this is never stated clearly or quantitatively. And in section 4.3 he says that motor commands are not commands for specific muscle tensions (at least not the ones originating in the cortex) but they are "an image of intended achievement" -- YES!!! But he never explains the implications of this remarkable statement. If the intended achievement is not a perception then how does this image of intended achievement get transformed into the actual actions that achieve it. The answer to this question is provided by HPCT -- the hierarchical model of perceptual control. I suspect that Bridgeman doesn't even know that this problem (of turning intentions into actions that produce consistent results in a disturbance-prone environment) even exists for his model. Part of the problem is that he has not tried to build a working model -- just a pleasant sounding collection of words.

The basic point of his paper is completely unconvincing to me -- it is simply the assertion that the word "consciousness" refers to the process of carrying out these "plans". I don't see any relevant evidence for this assertion at all. If a plan is really a perceptual control system (giving Bridgeman the benefit of the doubt) then I disagree since there are so many obvious examples of control taking place with no consciousness whatsoever (in fact, we are unconscious of most of our controlling -- such as controlling our balance).

So all in all I rate Bridgeman's paper a good example of the "close, but no cigar" phenomenon.

Dag -- Great post on system concepts and standards (920504 -- I think). Really excellent. I will try to give you some replies ASAP but I think I agree with everything you said -- and said very well, too.

Regards Rick

Date: Tue May 05, 1992 12:23 pm PST

Subject: Re: Test Message

I for one have enjoyed the break. I cannot keep up anyway. Among other things, I have been thinking how, it sems to me, that csg theory is compatible with many notions in psychoanalysis. As I see it that theory is about my perceptions and keeping them constant, especially notions of defense mechanisms and how they operate. But I am still struggling with the ideas. Len Lansky

<< Leonard M. Lansky Internet: Lansky@UCBEH.SAN.UC.EDU or >>
<< Department of Psycholgy Len.Lansky@uc.edu >>
<< U of Cincinnati (ML 32) Bitnet: Lansky@ucbeh.bitnet >>
<< Cincinnati, Ohio 45221 Phone: (513)556-5549/751-0392 >>

Date: Tue May 05, 1992 5:05 pm PST
Subject: Continuous/discontinuous; miscellany

[From Bill Powers (920505.1840)]

Got a adaptive map going to correct the difference between kinesthetic and visual coordinates in the Little Man. So far it seems to work for a while, then goes bonkers. These closed-loop systems are hard to understand. Plod, plod.

Bruce Nevin (920505?) --

In your header you write "[From: Bruce Nevin (Mon 92044 13:48)]". Is that a typo? If not, pls. translate 92044.

We ARE talking about different meanings for "continuous" and "discontinuous." You use these words, apparently, in the sense of "constant" or "variable." I use "continuous" in the sense of having derivatives everywhere. In other words, a continuous (analog) perception gets from one state to another by traversing a continuum of states between the starting and ending states. A discontinuous one goes from the beginning state to the ending state instantaneously, without passing through intermediate states. Thus the perception of a variable like "spatial separation" is continuous in my terms because for the separation between real things to go, say, from large to small, it must shrink through a continuum of intermediate separations. Perception of a variable like "dog," on the other hand, remains the same until the underlying perceptions change enough to cross a category boundary; then the perception changes instantly to "cat" (or whatever -- armadillo) without occupying any intermediate states. This meaning of the terms distinguishes between "analog" and "digital" variables, the meaning I was using.

There can be change in either the analog or the digital world. The difference is whether the changes are smooth or occur in jumps. Clearly, perceptions that change in a continuum don't lend themselves to description in words or symbols (unless the symbols are intended to represent continuous variables). The nearest we can come is to use discrete modifiers to refer to selected points in the continuum: very far apart, somewhat far apart, not very far apart, and not at all far apart. Those terms simply sample a tiny part of the possibilities and don't reflect the quantitative relationship.

I think that perceptions from relationships on down are essentially continuous -- although they are certainly continuously changing! From categories on up, they are discontinuous, either-or, sequential, logical in nature and while they are still changeable they can change only in finite jumps.

> The experience of shifting levels is the experience of
>identifying with one level rather than the other.

Right. I mean, agree.

>The point of view or witness is something other than either.

Agree. Some call it the observer.

>One can identify with that without the experience of first identifying
>with a succession of levels of perception as point of view on the next
>level below.

Agree. But the normal state is for one to be identified with some level, habitually. I think it takes some experience with leaving that habitual level (the hardest one to leave) and seeing it as a level, before the attachment weakens enough to allow detaching at will regardless of the current point of view.

>The unattached witness experienced when looking at the highest >accessible level is actually available everywhere and one can identify >with it even while looking at level n from level n+1.

Agree. It is always the witness (or observer) who is aware; the particular level involved merely provides the content, the interpretations, the desires, and the habitual actions.

>There is much greater freedom in this.

That's why it's worth doing.

>I think holding out ecstasy as a reward is at best a well intended >fraud perpetrated on behalf of those who otherwise would not poke into >these questions.

Yes. Control theory (among other approaches) teaches us that the understanding person is unrewardable, and thus uncontrollable through rewards. It hardly seems appropriate to teach this by holding out rewards, unless the intent is to reverse the desire that tempted the person, sort of like a koan. "You can have anything you want, provided you don't want it."

I've never thought of the Quakers as a religious group. They behave more like support groups I've seen. I like sermons that anyone can stand up and deliver, like here.

Len Lansky (920505) --

>As I see it that theory [psychoanalysis] is about my perceptions and
>keeping them constant, especially notions of defense mechanisms and how
>they operate. But I am still struggling with the ideas.

If you browse through Freud's book on dreams, you'll find his "lens analogy." He said that perception is like one lens looking at the image formed by another lens, which is looking at the image formed by a still lower lens ... . I think he understood about levels of perception. His emphasis on conflict agrees completely with the CT concept. He, however, thought that certain particular conflicts were universal and explained everything, not seeing that it's conflict itself, whatever it's about, that causes the problems.

Gary Cziko et. al.

The thought of replying to the Bridgeman article in PSYCHOLOQUY just makes me want to take a nap. I'll leave that up to you guys.

Rick says "The basic point of his paper is completely unconvincing to me -- it is simply the assertion that the word "consciousness" refers to the process of carrying out these "plans". "

Everybody thinks consciousness is so easy to define. Yawn.

Best to all -- no time for all this jabber. Bill P.

Date: Wed May 06, 1992 5:22 am PST
Subject: discontinua continued

[From: Bruce Nevin (Wed 92046 08:00:23)]

Bill Powers (920505.1840)

Date: Yes, I edited it because of elapsed time since I started the message, or that's what I was intending to do when (I think) someone came into my office at that moment needing something of me. I suppose my finger wandered from 5 to 4, and the 0 finger didn't press hard enough the second time, and I never did put the time in, and I didn't go back and check afterward as I was very late leaving. Interruption is an interesting phenomenon, no? Now what was I doing . . .

>We ARE talking about different meanings for "continuous" and
>"discontinuous." You use these words, apparently, in the sense of
>"constant" or "variable." I use "continuous" in the sense of having
>derivatives everywhere.

I guess I am including both constant/variable and continuous/ discontinuous in the discussion, but I do intend the latter in just the sense you do. I was surprised to find granularity in what seems (in retrospect) to be intensity and sensation perceptions. I didn't pursue the matter at the time (a 10-day vipassana course several years ago) as it was just one aspect of a very rich process. I'm not able just now to verify, don't have protracted time time free for just sitting in awareness of one perception. I am speaking from recollection.

I proposed two alternative explanations.

According to one, it is a discontinuity of attention. Like visiting and remarking how much the child has grown, vs. living continuously with the child and not noticing. It is subjectively similar to a category shift, as you say. Lying gazing at the sky and noticing that a cloud has

changed shape. It has grown imperceptibly darker at my desk and I need to turn on the light. When your attention returns, it is no longer the configuration of a dog's head in the cloud but that of an elephant's foreleg, with the rest of the elephant over there. These are, as you say, category shifts. Perhaps it is the shift of category that draws renewed attention.

According to the other interpretation, there really is some discontinuity or granularity in the perceptual signal that is usually smoothed by the input function of the low-level ECS, using the imagination loop. Usually, expectation fills gaps, that is, reference signals from higher-level ECSs call for supplemental signals out of memory by the imagination loop when input from sensors (perhaps mediated by input functions of lower ECSs) is incomplete. In the unusual situation of prolonged attention to the same sensory signal(s), I speculated, higher-level ECSs stop controlling for that sensory input, and you become aware of it more simply as it is, without "normalization" through the imagination loop.

What argues for the second is the fact that the experience only emerges after prolonged practice at keeping awareness constant, occurs only at times when one seems to be more rather than less successful at keeping awareness constant, and is markedly different from experiences noted when attention wanders, then returns to the perception chosen for meditation. In particular, it is not a category shift. The perception is there or it is not, and when it is there again it is so far as one can tell the "same" perception, subject to the same sorts of continuous variation as before.

It's entirely possible that I'm wrong on both counts. I don't hear anybody else jumping up and saying "yeah, I noticed that too when I was meditating regularly." Nor do I hear anybody saying "I've been meditating an hour a day for years, focussing attention on simple low-level perceptions, and I've never come across anything like that."

Bruce
bn@bbn.com

Date: Wed May 06, 1992 9:21 am PST
Subject: control, blindmen paper

[From Rick Marken (920506)]

Ken Hacker [920504] says:

> Rick's comments about the riots stimulated me to think
>of how two extremes of explaining human behavior are both false. One says
>that people are determined by external factors (behaviorism, cultural
>studies). The other says that people are purely determined by internal
>factors (cognitivism, some approaches to perception).

I have just completed a short paper that speaks to this point -- why there exist such different views of the causes of behavior. The paper is called "The blind men and the elephant: Three different perspectives on the phenomenon of control". It is based on something I posted to CSG-L a couple months ago. It is a very short paper but I

think I should post it to the listserver so people can access it if they want -- and not clog up everyone's mailboxes.

So, Gary (or anyone) -- how do I make my paper available to people who want to read it?

I plan to send it to a journal soon -- but I would appreciate any comments or questions on it before I do so.

Regards Rick

Date: Wed May 06, 1992 11:46 am PST
Subject: left, right, center

[From: Bruce Nevin (Wed 92046 13:25:57)]

(Rick Marken (920506)) --

Someone whose name I am not remembering studied ideology some years ago, and characterized the right as seeing human nature in terms of conformity to external control, the left in terms of internal control. It's sketched in the book _Maps of the Mind_, under the title Left, Right, Center.

Bruce
bn@bbn.com

Date: Wed May 06, 1992 1:04 pm PST
Subject: Blindmen Paper

[from Gary Cziko 920506.1322]

Rick Marken (920506) queries:

>So, Gary (or anyone) -- how do I make my paper available to people
>who want to read it?

Easiest would be to send it to Bill Silvert <silvert@biome.bio.ns.ca> and ask him to put it in the CSG subdirectory. Let me know when you have done this and I will follow up and make an announcement to CSGnet about how to get it after I know that I can get it.

Before you send it though, you might want to look at the format of the Bridgeman paper. Notice how the paper is divided into sections numbered 1.0, 2.0, 3.0, etc. with each paragraph numbered as well, e.g., 1.1, 1.2, 1.3 in section 1.0, etc.

This is a good format to adopt for electronic papers since the numbered paragraphs make up for the lack of page numbers and make it easier for commentators.--Gary

Date: Wed May 06, 1992 1:52 pm PST
Subject: Re: left, right, center

[From Chris Malcolm]

Bruce Nevin writes: (Wed 92046 13:25:57)]

> Someone whose name I am not remembering studied ideology some years ago,
> and characterized the right as seeing human nature in terms of
> conformity to external control, the left in terms of internal control.
> It's sketched in the book _Maps of the Mind_, under the title Left,
> Right, Center.

A fascinating snippet from a 25 year old psychology textbook by "Kretch & Crutchfield" (warning: ancient rusted memory) concerned people's reactions to being placed in a trick room which could rotate on gimbals, while having trick 'solid" water in a glass and trick stiff wire for hanging lamp, so that no matter how the room was tipped, it looked level. The subjects were sat in a chair which they could rotate with little handles, and after the room had been spun around to disorient them, they were asked to level the chair while remaining seated in it.

The only personality variable the experimenters found which correlated with people's performance was left-wing/right-wing. Right wing people tended to line the chair up with the walls of the room, left wing with gravity. In other words, external and internal reference signals.

I've waited for 25 years to find someone who thought this was actually interesting!

Date: Wed May 06, 1992 1:57 pm PST
Subject: left, right, center

[from Avery Andrews (920506.1216)]
So why, given that Chomsky is such a left-winger, is his linguistic terminology so relentlessly fascist in its connotations. E.g. government, binding, control, command, dominate, chain-government ... (this is a frequent observation in the oral tradition, committed to writing by someone in the latest issue of the journal `Natural Language and Linguistic Theory'.

Avery.Andrews@anu.edu.au

Date: Wed May 06, 1992 2:14 pm PST
Subject: One person's continuum is another's discontinuum

From Greg Williams (920506)

>Bill P. and Bruce N. on continuous/discontinuous perceptions

Sometimes, the distinction is a matter of well-developed abilities of fine discrimination, honed via meditation... or practice, which of course can sometimes be a kind of meditation. John Scarne, the premier card manipulator of this century (at least among those who have revealed themselves!) could cut to the aces in a well-shuffled ungimmicked deck after some simple riffle shuffles. In his fabulous autobiography, THE ODDS AGAINST ME (Simon and Schuster, New York, 1963), Scarne wrote: "I always give the deck one or more riffle shuffles and hold the cards in such a manner that I can glimpse the

indices on the cards as they fly past during the shuffle. When I sight an Ace I count by feel the number of cards which fall on top of it. This I do with as many Aces as possible. Then I calculate the number of cards a player cuts to the nearest bottom Ace, and I cut down to this total and there is the Ace.... If you do the same thing for three or four hours a day you'll be able to do it, too, in about twenty years.... I practice ten hours a day."

Greg

Date: Wed May 06, 1992 5:53 pm PST
Subject: Three Blind Men

[Rick Marken (920506 17:30)]

Well I have it on the highest authority (Gary Cziko -- net God) that it would be OK to post my paper since it is within the bounds of decency (20Kbytes). Indeed, I think it's damn near that limit. I am posting it in the hopes of getting 1) helpful comments -- be nice now and 2) suggestions about where it might be worth trying to submit it for publication -- be even nicer.

Or, simply hit the 'n' key now cause here it comes:

The Blind Men and the Elephant:
Three Different Perspectives on the Phenomenon of Control

Richard S. Marken

Abstract - Psychologists have described behavior as 1) a response to stimulation 2) an output controlled by reinforcement contingencies and 3) an observable result of cognitive processes. It seems like they are describing three different phenomena but they could be describing one phenomenon -- control -- from three different perspectives. Control is like the proverbial elephant studied by the three blind men; what one concludes about it depends on where one stands. It is suggested that the best place to stand is where one has a view of the whole phenomenon - be it elephant or control.

The behavior of living organisms (and some artifacts) is characterized by the production of consistent results in an unpredictably changing environment, a phenomenon known as control (Marken, 1988). Control can be as simple as maintaining one's balance on uneven terrain or as complex as maintaining one's self-esteem in a dysfunctional family. Control is a pervasive aspect of all behavior yet it has gone virtually unnoticed in psychology. What has been noticed is that behavior is a response to stimulation, an output controlled by reinforcement contingencies or an observable result of cognitive processes. Each of these ways of describing behavior is what would be expected if people were describing control from different perspectives. The situation is similar to that of the the three

blind men who were asked to describe an elephant; the one near the tail described it as a snake, the one near the leg described it as a tree trunk and the one near the side described it as a wall. These descriptions gave a true picture of some aspects of the elephant, but a false picture of the elephant as a whole. If behavior involves control then psychology, too, has given a true picture of some aspects of behavior but a false picture of behavior as a whole. To see why this might be the case it is necessary to take a close look at what it means to control.

Closed-Loop Control

The basic requirement for control is that an organism exist in a negative feedback situation with respect to its environment. A negative feedback situation exists when an organism's response to sensory input reduces the tendency of that input to elicit further responding. Negative feedback implies a closed-loop relationship between organism and environment; sensory input causes responding that influences the environmental cause of that input. It is hard to imagine an organism that does not exist in such a closed-loop situation because all organisms are built in such a way that what they do affects what they sense. Eyes, for example, are located on heads that move so that what the eyes see depends on what the head does. To the extent that what the head does depends on what the eyes see (and it does, at least sometimes, as when the head turns in response to an attractive passer-by) there is a closed loop; sensory input causes responding (head movement) which affects the cause of responding (sensory input). The feedback in this loop must be negative because behavior is stable; it does not normally exhibit the "run away" behavior that characterizes positive feedback loops (such as the "feedback" from a microphone that amplifies its own output).

The fact that organisms exist in a closed negative feedback loop means that two simultaneous equations are needed to describe their relationship to the environment. The first describes the effect of sensory input on responding (for simplicity we will assume that all functions are linear and that all variables are measured in the same units) so that

$$r = k.o\ (s^*-s) \qquad (1),$$

where r is the response variable and s is the sensory input variable (expressed as deviation from s* which is the value of the input that produces no response -- or no change in response -- from the organism). The multiplier, k.o, is the linear organism function that transforms sensory input into responding. The second equation, too often ignored by psychologists, describes the effect of responding on sensory input. For simplicity it is assumed that responding adds to the effect of the environment so that

$$s = k.f\ (r) + k.e\ (d) \qquad (2),$$

where r and d are the response and environmental variables, respectively. These variables have independent (additive) effects on the sensory input, s. The nature of the environmental effect on sensory input is determined by the environmental function, k.e. The feedback effect of responding on the sensory cause of that responding is determined by the feedback function, k.f.

These two equations must be solved as a simultaneous pair in order to determine the relationship between stimulus and response variables in the closed loop. The result is

$$r = k.o/(1+k.o\ k.f)\ s^* - (k.o\ k.e)/(1+k.o\ k.f)\ d \qquad (3).$$

The organism function, k.o, transforms a small amount of sensory energy into a huge amount of response energy (such as when patterns of light on the retina are transformed into the forces that move the head). In control engineering, k.o is called the "system amplification factor" and it can be quite a large number. With sufficient amplification (such that k.o >> k.f and k.o >> 1) equation (3) simplifies to

$$r = s^* - (k.e/k.f)\ d \qquad (4).$$

Equation (4) describes the relationship between environmental (stimulus) and response variables when on organism is in a closed-loop, negative feedback situation with respect to its environment. The result of being in such a situation is that the organism acts to keep its sensory input equal to s*, which is called the reference value of the input. The organism does this by varying responses to compensate for variations in the environment that would tend to move sensory input away from the reference value; this process is called control. What is remarkable about control is that responses depend on environmental events, d, and not on the sensory inputs, s, that are caused by these events. The sensory inputs are cancelled out of equation (4) by the amplifying effect of the organism on those inputs. Responses also depend on the reference value for sensory inputs, s*, but the value of s* is determined by properties of the organism, not the environment. Thus, variations in s* will appear to be spontaneous because they do not necessarily correlate with other variables in the organism's environment.

Three Views of Control

All variables in equation (4), with the possible exception of s*, are readily observable when an organism is engaged in the process of control. The environmental variable, d, is seen as a stimulus, such as a light or sound. The response variable, r, is any measurable result of an organism's actions, such as a bar press or a spoken word. The reference value for sensory input, s*, is difficult to detect because an observer cannot see what an organism is sensing. The value of s* can be measured in terms of the

environmental variables (it corresponds to that value of d that results in no corrective response by the organism). But it would be hard to imagine why someone would even try to make such a measurement unless he or she knew that the organism was controlling its sensory input. In fact, just the opposite is the typical assumption -- that the organism is controlled by its sensory input.

The reference value for sensory input, s*, is the central feature of control since everything an organism does is aimed at keeping its sensory inputs at their reference values. Because the reference value is difficult to observe it will not be obvious to an observer that an organism is engaged in the process of control. What will be obvious is that certain variables, particularly the environmental and response variables, and the relationship between those variables, will behave as described by equation (4). Thus, equation (4) can be used to show what control might look like if one did not know that it was occurring. It turns out that there are three clearly different ways of looking at control depending on which aspect of the behavior described by equation (4) one attends to.

1. The stimulus - response view. This view of control sees behavior as a direct or indirect result of input stimulation. Equation (4) shows that behavior will look this way when the reference value for stimulus input is a constant; for simplicity assume that it is zero. Then

$$r = - (k.e/k.f)\ d \qquad (5).$$

It looks like variations in an environmental stimulus, d, cause variations in the response, r. This is what we see in so-called "reflex" behavior, such as the pupillary response, where changes in a stimulus variable (such as illumination level) lead to changes in a response variable (such as pupil size). Of course, this relationship between stimulus and response is precisely that which is required to keep a sensory variable (sensed illumination) at a fixed reference value, s*.

One's inclination when looking at an apparent relationship between stimulus and response is to assume that the nature of that relationship depends on characteristics of the organism. Equation (5) shows, however, that when an organism is engaged in control, this relationship depends only on characteristics of the environment (the functions k.e and k.f); the organism function, k.o, that relates sensory input to response output, is rendered completely invisible by the negative feedback loop. This characteristic of the process of control has been called the "behavioral illusion" (Powers, 1978).

2. The reinforcement view. This view of control sees behavior as an output that is shaped by contingencies of reinforcement. A reinforcement contingency is a rule that relates outputs (like bar presses) to inputs (reinforcements); in equation (4) this contingency is represented by the

feedback function, k.f, that relates responses to sensory inputs. Equation (4) shows that it would look like the feedback function controls responses when s*, d and k.e are constants, as they are in the typical operant conditioning experiment. In these experiments, s* is the organism's reference value for the sensory effects of the reinforcement; it is kept constant (and large) by maintaining the test animal at a fixed proportion of its normal body weight. The environmental variable, d, is the reinforcement, which, if it is food, is a constant size and weight. The sensory effect of a reinforcement can be assumed to be directly proportional to its size and weight, making k.e = 1. So, for the operant conditioning experiment, equation (4) can be written as

r = S* - 1/k.f (D) (6)

where S* is the constant (and possibly very large) reference value for sensed reinforcement and D is the constant value of the reinforcement itself.

The only variable in equation (6) is the feedback function, k.f, which defines the contingencies of reinforcement. One simple contingency is called the "ratio schedule" in which the organism receives a reinforceMe¦t only after a certain number of responses. The term "ratio" refers to the number of responses required per reinforcement . So a "ratio 10" schedule is one in which the organism must make ten responses in order to get one reinforcement. It is regularly found that increases in the ratio lead to increases in rates of responding. Such a result is predicted by equation (6) which can be seen by letting k.f equal the ratio value. Increases in the ratio, k.f, lead to increases in responding, r. This relationship exists because the organism is controlling sensed reinforcement; responding varies appropriately to compensate for changes in the reinforcement contingency so that sensed reinforcement is kept at a constant reference value.

3. The cognitive view. This view of control sees behavior as a reflection or result of complex mental plans or programs. This kind of behavior is seen when people produce complex responses (such as spoken sentences, clever chess moves or canny investment decisions) apparently spontaneously; there is often no visible stimulus or reinforcement contingency that can be seen as the cause of this behavior. Cognitive behaviors are most obvious when environmental factors (such as stimulus variables and environmental and feedback functions) are held constant. When this is the case, equation (4) becomes

r = s* + K (7).

where K = (k.e/k.o)D, a constant.

Since s* is typically invisible, equation (7) shows that there will appear to be no obvious environmental correlate of cognitive behavior. An observer is likely to

conclude that variations in r are the result of mental processes -- and, indeed, they are. But it is actually variations in s*, not r, that are caused by these processes; variations in r being the means used to get sensory inputs equal to s*. Thus, chess moves are made to keep some sensed aspect of the game at its reference value. When the environment is constant, r (the moves) may be a fair reflection of changes in the reference value for sensory input. However, under normal circumstances r is only indirectly related to s*, variations in r being mainly used to compensate for variations in the environment that would tend to move sensory input from the reference value, s*.

Looking at the Whole Elephant

The blind men never got a chance to look at the whole elephant but if they had they would have instantly understood why it seemed like a snake to one, a tree trunk to another and a wall to the third. Psychologists, however, can take a look at control and see why behavior looks like different phenomena from different perspectives. What is common to the three views of behavior discussed in this paper is the reference for the value of sensory input, s*. Organisms behave in order to keep sensory inputs at these reference values (Powers, 1973). They respond to stimulation in order to keep the sensory consequences of this stimulation from moving away from the reference value; so it appears that stimuli cause responses. They adjust to changes in reinforcement contingencies by responding as needed in order to keep the sensory consequences of reinforcement at the reference value; so it appears that contingencies control responding. And they change their responding in order to make sensory input track a changing reference value for that input; so it seems like responding is spontaneous.

What appear to be three very different ways of describing behavior suddenly are seen as legitimate ways of describing different aspects of one phenomenon -- control. It also becomes possible to make sense of all aspects of the organism's behavior once one discovers the sensory inputs that are being controlled. Controlled sensory inputs are called controlled variables and s* is the reference value for controlled variables. There are methods, based on control theory, that can be used to determine what sensory variables are being controlled by an organism at any time (Marken, 1989). These methods make it possible to take off the blindfolds and see the whole elephant -- the phenomenon of control.

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Richard S. Marken USMail: 10459 Holman Ave
The Aerospace Corporation Los Angeles, CA 90024
Internet:marken@aerospace.aero.org
(310) 336-6214 (day)
(310) 474-0313 (evening)

Date: Thu May 07, 1992 10:41 am PST
Subject: Blindmen Paper

>Rick Marken (920506) queries:
>
>>So, Gary (or anyone) -- how do I make my paper available to people
>>who want to read it?
>
>Easiest would be to send it to Bill Silvert <silvert@biome.bio.ns.ca> and
>ask him to put it in the CSG subdirectory. Let me know when you have done
>this and I will follow up and make an announcement to CSGnet about how to
>get it after I know that I can get it.

This has been done, and the paper can be retrieved by ftp or mail server
from biome.

Bill
--
Bill Silvert at the Bedford Institute of Oceanography
P. O. Box 1006, Dartmouth, Nova Scotia, CANADA B2Y 4A2
InterNet Address: bill@biome.bio.dfo.ca

Date: Thu May 07, 1992 10:59 am PST
Subject: not just standards alone

from Ed Ford (920507.11:15)

Dag Forssell (920504)

>...many Systems Concepts packages support the SAME standards.
>Therefore it does NOT follow that your Systems Concept package is
>validated by the success of your standards...I would be content to say
>(I think) that your Systems Concepts packages are validated by the
>simple fact that they are yours. Your Systems Concepts are YOURS and
>that is ENOUGH.

I think you are looking at this in a linear way. My Systems Concepts

is my highest level, out of which I create a set of standards, criteria, or principles which form the guidelines for the decisions I make. So far, this is all theoretical.

The real test for anything is
when the rubber hits the road. When I teach, I believe all students should be treated as fairly as I humanly can. At the same time, I have established a standard within that "fairly" framework that limits the time for individual explanation or debate with one student during classroom time, which, if lengthy, would deprive other students of needed instruction or role play experience. The decisions I make and the consequent actions I take with individual students are constantly monitored by me as I compare what I want to the variable I'm trying to control, namely the student/teacher interaction variable.

So it isn't
the standards as such that are or not successful, but rather the entire behavioral process within my system as it evolves during my classroom. So it isn't whether the standards in and of themselves (or as they relate to the systems concepts) are successful, for they can't be measured independent of the entire behavioral structure that is the operational living control system.

Rather, it is our whole system
operating as a continuous process. This involves a whole bunch of things that are all interlaced, interactive, and interrelated, each being a part of the whole process. I might have to adjust my systems concepts (as when I learned PCT), or change a few standards, or alter specific goals or decisions, or change my approach to controlling the variable, perhaps by dealing in a more effective way with the various obvious and sometimes unforeseen disturbances.

Systems Concepts are
validated not because they are mine, but because, over a period of time, I have found satisfaction and fulfillment through controlling and closing perceptual errors using specific systems concepts as a reference signal. This is the real test of any systems concepts, I would think. This is where real success is measured.

Establishing
systems concepts, setting standards, and making decisions is only a part of this process. It also involves being able to control for the right variable, at the right time, dealing with both foreseen and unforeseen distrubances, learning to "listen to and deal with" our reorganization system, while at the same time contending with other conflicting reference signals and principles, both within our own system and in the various systems around us.

>Religions as Systems Concepts packages typically include a whole super
>structure of baggage in the form of miracles and explanations which at
>one time probably were designed to sell the package to illiterate,
>ignorant people and keep them in check. Some of this creates
>unfortunate standards which prevent people from functioning well.

Concerning the use of my own faith as an example. I promise you, I'll not do so again. As a person who, at the tender age of 65, believes in a personal and loving God, in prayer, in miracles (I actually witnessed

one), and in spiritual growth, I can assure you my standards have not prevented this illiterate and ignorant person from functioning well. As to keeping me in check, Hester and my children having been trying to for years but with very little success.

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

Date: Thu May 07, 1992 11:21 am PST
Subject: Papers

cunningB@monroe-emh1.army.mil

Bill Cunningham :920507.1430:

Those lamenting the absence of Martin Taylor can correct for that disturbance by sending for his AGARD paper. It's a gem. Two of its virtues are gentle transition from beginner slope to expert and its wide range of application. As an experiment, I've shotgunned copies to various folks here who have a need to understand, but who would have never considered seeking it. We'll see what happens.

Rick Marken--

I like your paper on blind men, including the metaphor. I'm not a psychologist, but found your explanation of the various "blind men" very clear and very helpful to ME. I wouldn't change the content of the paper, but I do think it needs repackaging. First and foremost, determine the audience and package accordingly. A large part of my job here is translating/explaining technical mysteries to operationally oriented folks.

First thing my "customers" want to know is why any given subject is important to them (where they will apply it), and then the main points to remember. Reinforcing details last. My audiences cringe when they see an equation even if they're totally capable of following the math. They know instinctively that the math just a metaphor for whatever is being discussed. When I have to use math, I do so only in the reinforcing details and introduce it with "I'm going to show you some equations that describe (subject). It's not necessary to memorize the equations, but rather to understand how they reveal the relationship between (xx) and (yy)." That great piece of whizdumb is counterproductive if the audience would prefer a carefully developed mathematical argument--dismissing all else as "soft".

Along same lines, look at the structure of Martin Taylor's paper, particularly his use of graphics to ease readers into any formalism. That's one of the reasons I shotgunned it to folks who might read it without threat of violence.

It's also easy (for me) to see the application of Martin's paper to perhaps a dozen very different customers here. Only one of these, a psychologist dealing with training, would have a "need" to understand your paper. I send him PCT material regularly, with no response to date. So I'm stuck with asking for whom your perfectly clear (to me) explanation has important application. Answer that question, and you

will know where to publish and how to structure the paper.

One last point, hopefully substantive. You might consider the result of adding more blind men. Suppose there were a dozen. At best the "picture" of the elephant would be a mosaic of different parts. What's missing is the relationship between the parts. Control theory provides the relationship.

Bill C.

Date: Thu May 07, 1992 2:24 pm PST
From: William T. Powers
EMS: INTERNET / MCI ID: 376-5414
MBX: POWERS_W%FLC@vaxf.colorado.edu

TO: * Dag Forssell / MCI ID: 474-2580
Subject: Letter Suggestion

Dag:

Rather than waiting for your package to arrive, and thus appearing to reject or criticise it, I've put together a "cold approach" letter that would strike me favorably if I received it. This doesn't mean that a CEO would react the same way, but what other criterion does one have?

--Bill P.

Dear Sir,

This letter asks for an opportunity to tell you about a program based on a new scientific theory of management. If you should become interested, I would like to teach you and your people how to apply it. The point of this program is to identify the structure of your management processes, design ways of curing problems with human interaction, and teach principles that will enable your organization to continue this program in the form that best suits you, without further guidance.

The best way I know of to promote this program is to lay out its logic and its methods in a short presentation, no more than half an hour. "Purposeful Leadership" (one seems to need names for these things) relies on easily communicated principles and common sense. It borrows from the Deming approach, but is based primarily on a relatively new development in the behavioral sciences called "perceptual control theory," which is quite a bit less insulting to the intelligence than most psychological theories have been. The basic principles are teachable to and verifiable by any reasonably attentive person. Most people are able to begin applying it as soon as they understand the underlying model of human nature.

I will not tell you here how wonderful Purposeful Leadership will be for your organization. Given time for a brief presention to you or your key people, I believe I can communicate the meaning and the sense of this new approach well enough for the implications to become obvious. Every organization has problems, many of them considered inevitable, to be lived with but not solved. My presentation will, I believe, show those problems

that result from human interactions in quite a different light.

May I make an appointment for a presentation?

Yours truly,

Dag Forssell

Date: Thu May 07, 1992 5:05 pm PST
Subject: Papers

[From Rick Marken (920507 14:30)]

Bill Cunningham (920507.1430) says:

>Those lamenting the absence of Martin Taylor can correct for that
>disturbance by sending for his AGARD paper. It's a gem.

I got it; looks great.

>I like your paper on blind men, including the metaphor.

Thank you. Thank you.

> determine the audience and package accordingly.

Yes. It's aimed at "head in the air" type theroretical psychologists
like myself.

>It's also easy (for me) to see the application of Martin's paper to
>perhaps a dozen very different customers here. Only one of these,
>a psychologist dealing with training, would have a "need" to understand
>your paper.

If even him. This get's us back to the "hard sell" aspect of PCT. All
that PCT really can offer is understanding. If the model is right,
then this understanding won't help "applied psychologists" do what they
often want to do most -- control people (ie. get them to behave the way
they want). Many scientific models are attractive because they do help
us control things better. But PCT isn't quite so simple.

Incidentally, I count as "controlling" all efforts to get people
to behave the "right" way. This includes the most well-intentioned
efforts to get people to make fewer "errors" (since errors are
defined from the point of view of the observer), be more efficient
(because what is being accomplished "efficiently" is of concern to
the observer, not necessarily the person), etc.

PCT makes us understand that organisms are controlling worlds of
perceptual experience that we don't know about (their own sensory
experience) relative to reference levels that we can influence only
indirectly (by disturbing controlled variables).

PCT might be able to help us understand how to help people

control more effectively -- ie -- how to "empower' them. But
not many people are seriously interested in helping other
people -- unless they want to help other people behave the
way they (the helpers) think they should behave (control again).

> I send him PCT material regularly, with no response to date.
>So I'm stuck with asking for whom your perfectly clear (to me)
>explanation has important application.

My paper is aimed at everyone who is (and/or who deals with)
control systems.

> Answer that question, and you
>will know where to publish and how to structure the paper.

>One last point, hopefully substantive. You might consider the result
>of adding more blind men. Suppose there were a dozen. At best the
>"picture" of the elephant would be a mosaic of different parts.

My paper discussed what I think are the three dominant views of behavior
in scientific psychology (sr,reinforcement and cognitive). It turns
out that the input-output equation of control (equaiton 4 in the paper)
nicely accounts for why students of behavior (unaware of control) might
end up with these three points of view.

> What's
>missing is the relationship between the parts. Control theory provides
>the relationship.

Not the theory -- the phenomenon. There was no theory in the paper -- that
was part of the beauty of it (if I do say so myself). There is a phenomenon
called control that exists when an organism (or anything) is in a
high gain, closed loop, negative feedback SITUATION with respect to its
environment. When you are looking at control (and don't know it) you are
like to see sr, control by contingency or spontaneous (cognitve) behavior.
Control (not control theory) is what psychologists (and most other observers
of behavior) have failed to see.

I acknowledge, by the way, that many of the points I make in the paper
have already been made by others -- particularly, Bill Powers, Wayne
Hershberger and Tom Bourbon. The only claim to fame of the paper is
that it tries to show how THREE major approaches to behavior can be
seen as legitimate views of the phenomenon of control.

Best regards

Rick

Date: Fri May 08, 1992 3:24 am PST
Subject: more clinical example

To: Clifford Gann, Gene Bogess, Bill Powers, interested CSGnet
others
From: David Goldstein
Subject: more on clinical example
Date: 05/08/92

(I have been describing a clinical case I am seeing right now. The purpose of my doing this is to start up some kind of discussion of a clinical topic on CSGnet. I hope Ed Ford, and Dick Robertson will join in. And it would be nice to hear from some CSGnet listeners out there who may have clinical interests but are not card carrying clinicians.)

A summary of reactions(mostly private messages) I have received so far: (a) Bill Powers' first reaction to the initial case description was: don't psychologize, ask the person. By this, he means find out what experiences(perceptions) are being controlled by the person. The method of levels is the primary tool to do this within HPCT., (b) Clifford Gann's initial reaaction was that the man's sexual life with his wife was not satisfying., (c) Gene Bogess' comment was that the man was out of control and having the affair with the babysitter was a way to resestablish control; Gene expressed the opinion that HPCT therapy, or maybe any sort of therapy, was not up to handling something as complex as a marriage relationship. I invite the above named people to correct my summary if it is not quite right. Now on with the case.)

I had my first joint meeting with the man and his wife. This is the first time I met her. Her experience of the relationship is quite different from his. According to her: He ignores her and gives others more time than her. He doesn't value her, treats her like sh__, and has done so from the beginning of their marriage. When asked what she liked about him initially, she mentioned many postive qualities(exciting, intelligent, personable, etc..) but these are not shown to her now. She wants her husband to be with her the way he is with others. What happened didn't surprise her, it is consistent with the way he is. What does surprise and disturb her is the fact that he allowed this woman into their family life. She confided things to this woman. She plans on revenge against this woman.

My impressions of the wife: She was obviously having very strong feelings during the session and her body was shaking. I was struck by her unwillingness to share with him, the kind of activities she does as hobbies. To her, this was an area just for her. Sharing it would result in losing part of herself. She already gives too much to him she thinks. Also, I asked her if she was seeing a counselor for herself. She said that she gets more out of talking with her friends, people she is familiar with. I asked her to describe herself on an introversion/extroversion dimension. She described herself as way over on the introverted side. (He, described himself way over on the extroverted side.) She does not feel as though she has control over anything and uses screaming and yelling to get his attention. (He reacts by becoming quiet and withdrawn). She had a very controlling mother. (He had a very controlling father). In summary, the experiences which are important for her to control seem to be: privacy, being someone he values and respects, revenge at the babysitter.

My impressions of the way they were together: They have radically different perceptions when it comes to each other and their

relationship. They were not able to talk on a topic and resolve it. They jumped all over the place in conversation. They sat on the same sofa but did not show any affection towards each other. He touched her leg which was moving a mile a minute and she pushed his hand away and interpreted it negatively.

Some additional facts which came out: Their son was diagnosed with a serious neurological condition during the past Winter. This really threw her. Her concept of the perfect son had to be changed. She needed her husband then and he needed her then but they couldn't be there for each other.

Treatment interventions so far: I described Ed Ford's Quality Time Program and told them to start it this week. She is willing to give the marriage to the end of summer to see if it can be different. He is willing to give it whatevr time it takes to make it work. I will be seeing him on an individual basis as well as seeing them as a couple.

Date: Fri May 08, 1992 5:58 am PST
Subject: Blind men paper

Bill Cunningham |920508.0925|

Rick Marken--
Very quickly. I'm at home without ability edit and on the run for next week.

Paper aimed at "head in the air" type of theoretical psychologist.
Paper aimed at everyone who deals with control systems.

Those two statements are contradictory. If everyone is a control system, then everyone deals with control systems. Paper is thus for everyone, which brings me back to my original question. Very hard to write paper for both.

I like comment on "empowerment". That gives me another customer.

Not to worry. I'll carry your message to the guy here. I'll try to pose it in terms of a question so he'll be more likely to respond. Make that empowered to respond.

Phenomenon of control. Agree about phenomenon. However, from standpoint of paper, some audiences will not see that.

Bill C.

Date: Fri May 08, 1992 7:03 am PST
Subject: miracles and subjection

[from Joel Judd]

Dag (920507),

As a point of accuracy and not of argument (and because the sentiment is not uncommon), miracles and such were never intended to convince

non-believers or force those who don't believe to "see the light." In spite of this being one of the popular arguments against to organized religion, as well as part of conquering culture folklore (e.g. Cortez's "miracle" fire sticks helping to subject the Aztecs), both Judeo-christian scripture and responsible clergy will tell you that "miracles" serve only to strengthen the faith and commitment of those who already believe. Of course, this is readily interpreted as meaning believers believing what they want to believe, but then that's all the more reason NOT to misinterpret the role of miracles, whether one is a believer or not, right?

Date: Fri May 08, 1992 7:08 am PST
Subject: triple blind
[From: Bruce Nevin (Fri 92048 08:04:11)]

(Rick Marken (920506 17:30)) --

I have read and enjoyed your blind men paper. I have a few comments on substance and presentation. I have quoted sections of your text where I have some ideas for changes. My suggestions are almost entirely in lines not marked with > on the left.

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The existence and ubiquity of control is plain to me but more is needed to get it across to a non-PCT audience. The example that is given (head turning wrt seeing) is weakened by the phrase "at least sometimes," which to an audience who haven't yet grasped control might seem to contradict the claim of universality. So here's the first suggestion:

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> It is hard to
>imagine an organism that does not exist in such a closed-
>loop situation because all organisms are built in such a way
>that what they do affects what they sense. Eyes, for
>example, are located on heads that move so that what the
>eyes see depends on what the head does. To the extent that
>what the head does depends on what the eyes see
(for example,
> when the head turns in response
>to an attractive passer-by) there is a closed loop; sensory
>input causes responding (head movement) which affects the
>cause of responding (sensory input).
(Closed feedback loops concurrently exist for other actions affecting seeing, such as rotating the eyeballs, closing the eyelids, and dilating the pupils.) The fact that behavior is stable shows that the
> feedback in this
>loop must be negative.
Organisms do not
>normally exhibit the "run away" behavior that characterizes
>positive feedback loops (such as the "feedback" from a
>microphone that amplifies its own output).
[Can you provide an example of the rare type of organism behavior resulting from runaway feedback, for clarity?]

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The equations would bear a clearer relationship to text if they stand off as self-contained entities, rather than being embedded in the punctuation of the text. Perhaps the convention in the journals you have in mind is to put equation numbers in the right margin; I find it clearer on the left. Also, a summary of the meanings of terms used in the series of equations would be helpful to the non-initiate, or at least to me. Thus:

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> The fact that organisms exist in a closed negative
>feedback loop means that two simultaneous equations are
>needed to describe their relationship to the environment.
These are given as equation (1) and equation (2), below.
Equation (1)
> describes the effect of sensory input on responding.

(1) r = k.o (s*-s)

>For
>simplicity we will assume that all functions are linear and
>that all variables are measured in the same units.
>Here, r is the response variable and s is the sensory input
>variable (expressed as deviation from s* which is the value
>of the input that produces no response -- or no change in
>response -- from the organism). The multiplier, k.o, is the
>linear organism function that transforms sensory input into
>responding.
It
> transforms a small amount of
>sensory energy into a huge amount of response energy (such
>as when patterns of light on the retina are transformed into
>the forces that move the head). In control engineering, k.o
>is called the "system amplification factor"
or "gain"
> and it can be quite
>a large number.

> The second equation, too often ignored by
>psychologists, describes the effect of responding on sensory
>input. For simplicity it is assumed that responding,
r,
>adds to
>the effect of the environment,
d, as in equation (2):

(2) s = k.f (r)+ k.e (d)

>The variables
r and d
>have independent (additive)
>effects on the sensory input, s. The
effect of the environmental variable, d,
>on sensory input,
s,

>is determined by the
environmental function, k.e.
The
>feedback effect that
the organism's responding has
>on the sensory cause of that responding is
represented in equation (2)
>by the feedback function, k.f.

The terms in these equations are summarized as follows for reference
in the discussion that follows:

s = sensory input
s* = reference value such that s = s* produces no response
r = response
d = environmental variable
k.o = organism multiplier whereby the organism transforms a
small s into a much larger r
k.e = environmental multiplier making d commensurate with s
k.f = feedback multiplier making r commensurate with s

Equation (1) and equation (2)
> must be solved as a
>simultaneous pair in order to determine the relationship
>between stimulus and response variables in the closed loop.
>The result is
equation (3):

>(3) r = k.o/(1+k.o k.f) s* - (k.o k.e)/(1+k.o k.f) d

<Text moved from here to a point after equation (1)>

> With sufficient amplification (such that
>k.o >> k.f and k.o >> 1) equation (3) simplifies to
equation (4):

>(4) r = s* - (k.e/k.f) d

Equation 4 is an input/output equation. It
>describes the relationship between
>environmental (stimulus) and response variables when
an
>organism is in a closed-loop, negative feedback situation
>with respect to its environment. The result of being in such

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I think that around this point it would be helpful to describe modelling
and refer to models that are available and that anyone can examine that
approximate the observed behavioral outputs of organisms with 95-99%
accuracy, contrasting this with the track record of the three views
presented in the next section. This would be an effective place to
motivate the equations, saying that they are the conceptual core of
the programs used in this modelling. Might not hurt to mention that
these programs are essentially surprisingly simple, with
most of the complexity in modelling the physics of the environment
yielding d (and maybe k.e) -- or do I misunderstand some prior posts of

Bill's?

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>would be hard to imagine why someone would even try to
>make such a measurement unless he or she knew that the
>organism was controlling its sensory input. In fact, just the
>opposite is the typical assumption.
Researchers typically assume
>that the organism is
>controlled by its sensory input.

It can be surprisingly difficult to grasp this simple reversal of perspective: the organism is doing whatever it takes to maintain each controlled sensory input s at its respective reference value s* -- it is controlling its sensory inputs.

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>equation (4) can be used to show what control might look
>like if,
like most researchers,
> one did not know that
control was in fact what
>was occurring. It turns out

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Aside from similar dis-embeddings of subsequent equations, those are all the changes I come up with. I think it's a very good paper. I can't evaluate prospective publications.

Be well,

Bruce
bn@bbn.com

Date: Fri May 08, 1992 8:15 am PST
Subject: triple blind, equations, misc

[From Rick Marken (920508 8:30)]

Thanks to all who have commented on the paper so far.
Special thanks to Bruce Nevin (Fri 92048 08:04:11):
I agree with all your suggestions -- they are excellent
and I will incorporate them all (or, at least, those that
I am not to lazy to do, but you already did most of the
work for me -- thank you).

In a personal note, Gary Cziko asked:

>Could you please take me by the hand and show how you got from equation (2)
>to (3)? I know what it means to solve simultaneous equations, but I'm
>having trouble getting the anwer in the form that you have which simplifies
>so nicely when K.o >> K.f and K.o >>1.

>I also suggest that you post this to the net.

OK.

(1) r = k.o(s*-s) and (2) s=k.f(r) + k.e(d)

substituting (2) into (1) for s we get

r = k.o (s* - [k.f(r) + k.e(d)])

multiply through by k.o giving

r = k.o (s*) - k.o k.f(r) - k.o k.e (d)

Now put the terms with r together on the left so:

r + k.o k.f (r) = k.o (s*) - k.o k.e (d)

Now the tricky part. Cancel r out of the separate terms on the left so

r (1 + k.o k.f) = k.o (s*) - k.o k.e (d)

Finally, divide both sides by (1 + k.o k.f) giving equation (3)
in the paper

r = k.o/(1+k.o k.f) s* - (k.o k.e)/ (1 + k.o k.f) d

Now, if k.o is VERY large (say, 100,000) and k.f is relatively small
(say, 100) then the ratio

k.o/(1 + k.o k.f) is approximately 1. (100000/100101)

and the ratio

(k.o k.e)/ (1 + k.o k.f) is approximately k.e/k.f since

(100000*k.e)/(100001*k.f) = approx. k.e/k.f

soooo:

r = s* - (k.e/k.f) d

A quick note on "social control";

I think I have been making the mistake of sounding like I believe
that people CAN control other people -- and shouldn't. What I mean
is that people TRY to get other people to act as they (the would-be
controller) wants. Of course, the controller is not REALLY controlling;
but the controller is acting as though he/she can control (and it looks
enough like control so that people imagine that it can be done). The
fact is, of course, that if you REALLY try to control someone (make
them do behavior X, no matter what) than you are simply placing yourself
in conflict with that other control system. Most of what passes for social
control is just social "influence" (manipulating a side effect of control,
for example, by disturbing a controlled variable). When the controller

becomes implacable (because the controllee fails to continue being influenced) then you get problems.

To Tom Bourbon (if he is around) -- What a wonderful forward to LCS II. The only thing wrong with it is that it's MUCH better than my forward to LCS I.

Oh, and Bill, the book's pretty good too (like, INCREDIBLE!).

Best regards Rick

Date: Fri May 08, 1992 9:35 am PST
Subject: controlling for acting as if controlled

(Rick Marken (920508 8:30)) --

You're most welcome. It's an excellent paper, and I'm glad to do anything I can to help your audience "get it".

I realize there are no footnotes in this paper--one of its charms--but I wonder if it would be appropriate to include your breakdown of the transition from (2) to (3). Depends on what you can assume about your audience. Of course, most anyone would assume that it must be right because you wouldn't risk publishing an error in the algebra deriving one of your formulae. That's what I did. Like Gary, I just don't have the day-to-day familiarity. Thanks for asking him, Gary.

> Of course, the controller is not REALLY controlling;
>but the controller is acting as though he/she can control (and it looks
>enough like control so that people imagine that it can be done). The

This is complicated by the fact that people try to make and maintain social arrangments for cooperative action. This has the effect of people acting as if they were being controlled. The precursors of this are pretty basic in animal behavior, I think. Act in a predictable way around animals and they get used to you. Act unpredictably, and they go on alert and can get quite upset. Social arrangements for cooperative action require predictable behavioral outputs of the participants, as though the participants were being controlled by one another or by the social arrangement itself.

On another tack, I dropped in to the MIT bookstore the other day and saw some books by Georges Bataille. In a pair of books with a title something like "the unbearable share" he (says the cover blub) devlops the notion that the converse of utility is at the root of social arrangements and culture. First, the paradox: on a utilitarian theory, in which X is justified by its utility for the sake of Y, the whole must be ultimately based on something that is useless. This neatly parallels the lack of reference perceptions [I almost typoed "reverence perceptions"] above the highest observable level of the perceptual hierarchy. He builds up his theory on the notion that useless things like potlatch, conspicuous consumption, and eroticism are more fundamental to culture and history than control of the means of production, etc.

Bruce

bn@bbn.com

Date: Fri May 08, 1992 9:52 am PST
Subject: On the utility of miracles

From Greg Williams (920508)

>Joel Judd (920508)

>As a point of accuracy and not of argument (and because the sentiment is
>not uncommon), miracles and such were never intended to convince
>non-believers or force those who don't believe to "see the light."

With regard to accuracy (not argument), I suggest that you read JESUS THE MAGICIAN by New Testament scholar Morton Smith, then see whether (at least in the particular case of Jesus' relations with those he encountered) you still have the same view on this. I can agree with you about what current Christian theologians CLAIM as the utility of miracles (for believers only), but I think they have distorted the historical evidence to support that claim. Smith doesn't have such an ax to grind, and, based upon quite ample documentation, concludes that Jesus employed common conjuring tricks of his time to convince nonbelievers of his miraculous powers. That does NOT mean that his trickery necessarily compromised his message!

Greg

Date: Fri May 08, 1992 11:14 am PST
Subject: language and power

From: Bruce Nevin (920508 14:20)

The following summary posted to the Linguist digest may be of value to some folks here:

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Linguist List: Vol-3-366. Sat 25 Apr 1992. Lines: 122

Date: Tue, 21 Apr 92 16:59:57 MDT
From: southerl@acs.ucalgary.ca (Ron Southerland)
Subject: Language and Power Materials

Several weeks ago I posted a query on Linguist requesting information on texts or syllabi for 'Language and Power' courses. I received a number of responses (from Lloyd Holliday, Sally Thomason, Paul Hopper, Ron Smyth, Marti Hearst, Randy Allen Harris, Ian Smith, Alec Marantz, Michael Newman, Bert Peeters, Lee Hartman, Fiona Mc Laughlin, Niko Besnier, Teun A. van Dijk, Susan Ehrlich and Janet Bing) for all of which I am grateful. My contacts with van Dijk and Ehrlich and their responses were the result of suggestions made by other respondents.

The range of materials which might be covered in a course on 'L&P' has expanded beyond those listed in my original note (e.g., gender, government, business and race) to include 'language and the

disabled', 'power issues relating to speakers of English (or presumably any other dominant language) as a second language', 'language and colonialism', 'issues of linguistic standardization', 'power and discourse in the courts', 'critical linguistics or critical discourse analysis' and 'language and ideology'. The responses included a substantial bibliography from Teun van Dijk and a course outline (and bibliography) from Janet Bing ('Language, Gender and Power' taught Spring 1992 at Old Dominion University). These longer bibliographies will be combined, augmented with items suggested by other respondents and uploaded to the Linguist Listserv at a later date.

With respect to texts for L&P, there seem to be few choices--there are, of course, many sources for individual readings. I have decided to use as my one required text Robin Lakoff's Talking Power: The Politics of Language. (hardbound: 1990, paper: 1992). Sections of Lakoff's readable, accessible and witty book deal with 'the politics of everyday language', 'language and institutions', 'language across cultures' and 'the language of power'. Selection of this text gives the course a particular ideological bent which might, for instance, exclude any substantial use of Deborah Tannen's You Just Don't Understand. In any event, Lakoff must be supplemented by other readings.

Important resource books (for students or instructors) include:

Bolinger, Dwight. 1980. Language - The Loaded Weapon. London/New York: Longman.

Fairclough, N. 1989. Language and Power. London/New York: Longman.

Fowler, R., R. Hodge, G. Kress and A. Trew. 1979. Language and Control. London: Routledge.

Graddol, David, and Joan Swann. 1989. Gender Voices. Oxford: Blackwell.

Hodge, Robert, and Gunther Kress. 1988. Social Semiotics. Ithaca: Cornell University Press.

Joseph, John E. 1987. Eloquence and Power: The Rise of Language Standards and Standard Languages. New York: Blackwell.

Joseph, John E., and Talbot J. Taylor. (eds.) 1990. Ideologies of Language. London: Routledge.

Wilson, John. 1990. Politically Speaking: The Pragmatic Analysis of Political Language. Oxford: Blackwell.

Some other works which may be useful (in part at least) are --

Cameron, Deborah. (ed.) 1990. The Feminist Critique of Language. London/New York: Routledge.

Hughes, Geoffrey. 1991. Swearing: A Social History of Foul Language, Oaths and Profanity in English. Oxford: Blackwell.

O'Barr, W. M. 1982. Linguistic Evidence: Language Power and

Strategy in the Courtroom. New York: Academic Press.

Ricks, Christopher, and Leonard Michaels. (eds.) 1990. The State of the Language. Berkeley: University of California Press.

The following volume contains some examples of language used for purposes of manipulation, persuasion and/or obfuscation. Although the book seems to have a prescriptivist bent and is apparently intended primarily for composition courses, it might be a good source of examples in the absence of anything else.

Eschholz, Paul, Alfred Rosa and Virginia Clark. (eds.) 1990. Language Awareness. 5th edition. New York: St. Martin's Press.

The one journal most relevant to the L&P area is Discourse and Society. Each issue contains at least one article of interest.

The area of advertising is very much under-represented in the materials I have so far gathered. Any suggestions for inclusion in the upcoming *long* bibliography will be appreciated.

Again, thanks to all respondents for their generous contributions.

Ron Southerland
Department of Linguistics
The University of Calgary

Date: Fri May 08, 1992 1:47 pm PST
Subject: Re: Rick's comments on his paper

From Greg Williams (920508)

>Rick Marken (920507 14:30)

>PCT makes us understand that organisms are controlling worlds of
>perceptual experience that we don't know about (their own sensory
>experience) relative to reference levels that we can influence only
>indirectly (by disturbing controlled variables).

>PCT might be able to help us understand how to help people
>control more effectively -- ie -- how to "empower' them.

As I've said before, PCT isn't a single-edged cutting (through the crap) implement. Mapping out others' control structures using PCT techniques (particularly the test for controlled variables) can be preliminary to manipulating the activities of those structures, as well as to "empowering" them. I hope some other PCTers will admit how effective PCT tools could be in the hands of the "predict-and-control" folks, and quit burying their heads in the comforting sands of verbalisms like "there are no social control systems" (true, but not very comforting when you realize that Big Brother might prefer to let you go on controlling as you wish, but with SKEWED premises; and how does Big Brother decide on which premises to skew? one efficient way is to learn about parts of your control structure by applying the Test).

The last time I brought this up, Bill P. suggested that such manipulations in

the light of (partial) knowledge of what others tend to control for are doomed to be "short-term" only. But Bill said that "short-term" could mean many years. Ulp!

>But not many people are seriously interested in helping other people --
>unless they want to help other people behave the way they (the helpers) think
>they should behave (control again).

Exactly why I'm concerned.

New topic:

>There was no theory in the paper -- that was part of the beauty of it (if I
>do say so myself). There is a phenomenon called control that exists when an
>organism (or anything) is in a high gain, closed loop, negative feedback
>SITUATION with respect to its environment.

As soon as you start writing equations which purport to handle such situations IN GENERAL, you are theorizing. You are claiming that the variables change continuously so that linearization works, for example. True, you aren't presenting a DETAILED model (like Bill's hierarchy) for control of particular types of variables, but you are still making a (call it "generic", instead of "generative"?) model. I raise this point because you might be asked about, for instance, delays in controlling (like putting money in a long-term account so you'll get the interest in several years) -- where's the continuity and matching of reference and perception through time THERE, they'll say. If you agree with a recent post from Bill saying that the higher levels of his hierarchy operate DISCONTINUOUSLY, be prepared for battles on the generality of your "generic" CONTINUOUS, LINEARIZABLE model!

Back to the first topic:

>Rick Marken (920508 8:30)

>The fact is, of course, that if you REALLY try to control someone (make them
>do behavior X, no matter what) than you are simply placing yourself in
>conflict with that other control system.

What I was getting at in the beginning of this post is that it is possible (to a degree, and certainly within limits) by using the Test to reduce conflict with another's control structure while manipulating that structure to want what the controller wants and NOT what the structure would have (hypothetically) wanted in the absence of the controller's manipulations.

Greg

Date: Fri May 08, 1992 11:36 pm PST
Subject: clinical example

I am replying to David Goldstein's recent clinical example. I not only suggested that the man who slept with the babysitter was controlling for sexual gratification but for a higher level perception of controlling his own experiences. From memory, this man's wife seems to try to control her husbands experiences by overtly restricting his degrees of freedom. She may be considered aggressive at times. An example is when he was going to but something at the movies and she embarrassed him in front

of their friends so that he would not buy what he intended. She gains control of him by directly and overtly puting restrictions on him and he yields. Until he wants to gain control back. Then he passive-aggressively gains control of his expereinces by staying on the computer all night or sleeping with the babysitter. It was more than sex that he wanted by sleeping with the babysitter; he wanted control that his wife could not have. But when he did this he experienced a big woppin error signal at the priniciple level. He acted against the "don't cheat on your wife!!!" rule and that was very uncomfortable. But he gained control.

I am writing from internet, and I don't have an edit function. But note that the man did not want to but something at the movies but wanted to

buy something at the movies.

Certainly this man's problems seem amenable with PCT. He said he would spend whatever time it took. But she restricted him again buy saying she gave till the end of the summer to works things out. ^by
It seems that this couple is in a never ending battle to gain control by controlling the other whether it be aggressively by her or passive-aggressively
by him. Both need to learn how to communicate their desired perceptions and through quality time they can aid each other in achieving their individual and shared perceptions. But this cooperative action requires communication to direct it so both may attain their intended results. Well good luck David with this couple. I wonder how bad she would resist to you asking her to give up some control?

have a good weekend CSG!

Clifford Gann, Z_GANNCP@CCSVAX.SFASU.EDU

Date: Fri May 08, 1992 11:36 pm PST
Subject: RE: miracles and subjection

AMEN!

Date: Sat May 09, 1992 4:17 am PST
Subject: 3 blind men parable

[from Wayne Hershberger 920509]

Rick,
Your 3-blind-men paper is a gem. I love it. You have employed the Goldilocks principle perfectly! In every way.
I would like to see you submit it to American Psychologist or the new journal being published by the American Psychological Society; I don't recall the exact title, but its something like, "Theoretical Trends." It would also be an excellent chapter for an introductory psychology text. Perhaps you have already begun writing that book you've been talking about.

Warm regards, Wayne

Date: Sun May 10, 1992 6:35 am PST
Subject: group therapy
To: general CSGnet members
From: David Goldstein
Subject: group therapy based on HPCT
Date: 05/10/92

I would appreciate some comments on how to apply HPCT to a group therapy situation with adolescents. I am taking a look at how groups are run at the Residential Treatment Center(RTC) where I have beem Clinical Director for the past almost two years. Each resident is offered at least one session of individual and one session of group therapy per week. Other than insisting that a group therapy session take place each week, I have left this aspect of treatment to the consulting clinical staff to do as they see fit. I am beginning to feel that I need to take a more active role in this area.

Some of the problems we are running into with our groups: The groups are often chaotic, more so on the boys side than the girls side. The residents are sometimes stirred up by the group discussion and act out afterwards. The residential living staff complain of having to deal with this. The residential living staff participate in the group therapy and the clinical staff feel powerless to influence how they participate. The worthwhileness of the group therapy is being called into question.

Some issues which occur to me:

What are the distinctive purposes of group therapy? Some of my own thoughts--

To sharpen observational skills as you see your peers interacting with each other.

The levels of perception can provide some observational categories.

Trying to improve skill at reading other people's intentions.

To improve communication skills as you verbally express yourself in the group.

Expressing your intentions clearly.

Being aware of signs that you may have disturbed someone.

To learn more about yourself from the reactions of other people to you.

Receiving comments about your actions and words.

Picking people in the group who are like you and unlike you.

How should the meeting being organized to reach these goals? Some of my own thoughts--

Some easy-to-understand summary of HPCT should be part of what happens.

Some statement of the group self-image should be made. This involves stating the purpose of the group clearly.

Participation in the group should be voluntary after the resident has participated in the group for a while.

Some development of group norms(principle level perceptions) should take place.

I would really appreciate any comments you have about basing a group therapy on HPCT ideas.

Thanks.

Date: Sun May 10, 1992 7:16 am PST
Subject: Rick's paper

To: Rick Marken, other CSGnet people
From: David Goldstein
Subject: your paper
Date: 05/10/92

I really enjoyed your paper. I agree with all of the postive comments made by the others as well as their suggestions for when you rewrite.

I plan to use it to educate the consulting psychologists and psychiatrists at the place where I work about HPCT and its relationship to other views in psychology.

From a therapy perspective, all three views would likely fall under the cognitive/behavioral umbrella. This leaves out the psychoanalytic and humanistic/existential perpsectives.

One observation: Most of the consultant clinical staff to our center are psychoanalytically oriented. I think that HPCT therapy ideas have something in common with psychoanalytic views. For example: the idea of the importance of conflict, the idea of background perceptions at a higher level which a person is not aware of, the idea of resistance in therapy, the importance of wants.

In terms of your three blind men, I guess I would have to say that the "cognitive" view comes the closest to representing the psychoanalytic position. Would you agree?

Another point: the humanistic/existential view, for example as represented by Alvin Mahrer, is another major frame of reference in therapy which seems to be missing from the story. Can they be

included?

I would appreciate receiving an updated version of the paper when it becomes available.

Thanks.

Date: Sun May 10, 1992 8:47 am PST
Subject: Psychotherapy

[From Rick Marken (920510)]

Now there have been two posters noting the similarity of some aspects of PCT to psychodynamics. I forget who the first was; the second was David Goldstein in a very interesting post that I just read this morning.

David says (re: the three perspectives on behavior described in my paper):

>From a therapy perspective, all three views would likely fall
>under the cognitive/behavioral umbrella. This leaves out the
>psychoanalytic and humanistic/existential perpsectives.

>One observation: Most of the consultant clinical staff to our
>center are psychoanalytically oriented. I think that HPCT therapy
>ideas have something in common with psychoanalytic views. For
>example: the idea of the importance of conflict, the idea of
>background perceptions at a higher level which a person is not
>aware of, the idea of resistance in therapy, the importance of
>wants.

I agree. I think the psychoanalytic idea of "unconscious" causes of conflict is just like the control theory idea that conficts result from incompatible references set by higher (and not consciously accessible) levels. Freud called it the id, we call it the "next level up". Psychotherapy is based on the assumption that conflicts can be solved (somehow) by discovering the unconscious cause of the conflict. This is also consistent with the HPCT idea that once you can adopt the point of view of the higher level systems that are setting the conflicting lower level goals you can "solve the problem" by setting non-conflicting goals -- you become conscious of the cause of the conflict.

The part of psychotherapy (and behavior for that matter) that I don't see in the PCT model is resistance. I don't know that the PCT model currently has any mechanism that explains why people are often so unwilling (resistant) to "going up a level" (or, as Freud would say, see the unconsious cause of their problem). Resistance implies that CONTROL is happening -- in this case, a person is controlling something about the consequences of changing their point of view (their consciousness). I believe this is a real phenomenon; we see it in the discussion of beliefs on CSGnet, for example. I detect some resistance on the part of some people (I will not exempt myself, by the way) to look their beliefs as just phenomena, taking a point of view that is not that of the beliefs themselves.

I think part of this resistence phenomenon may not be a "control of consciousness" phenomenon but just part of the operation of the control hierarchy when it is in imagination mode -- we are controlling what we imagine, and the existing hierarchy of references determine what we want to perceive (whether the cause of those perceptions is "boss reality" or imagination). I think a nightmare is an example of the production of imagined perceptions that were not effectively "resisted" by the ordinary control mechanisms that allow imagined perceptions quite deviant from the existing references for perception (imagined or not).

But the resistence seen in the "methods of levels" or ordinary psychotherapy, could be a different phenomenon -- it's like there is a control system (a defense mechanism?) that is busy keeping you from going "up a level" to the cause of the references that are creating a problem.

So David, as a clinician, do you think there might be such a system? And, if so, what the hell might if be for? Is this the system that zen philosophers (and some motorcycle maintainers) are trying to get past?

Happy mothers day

Oedipus Richard S. Marken

Date: Sun May 10, 1992 10:20 am PST
Subject: CSG Book Publishing Report

From Greg Williams (920510)

Report on Control Systems Group Book Publishing

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Date: Sun May 10, 1992 4:57 pm PST
Subject: re.: psychotherapy

To: Rick Marken, interested CSGnet others
From: David Goldstein
Subject: psychotherapy
Date: 05/10/92

Rick, in comparing HPCT therapy to psychoanalytic therapy asks about the phenomenon of resistence to moving up a level:

"So David, as a clinician, do you think there might be such a system? And, if so, what the hell might if be for? Is this the system that zen philosophers (and some motorcycle maintainers) are trying to get past? "

"But the resistence seen in the "methods of levels" or ordinary psychotherapy, could be a different phenomenon -- it's like there is a control system (a defense mechanism?) that is busy keeping

you from going "up a level" to the cause of the references that are creating a problem. "

I think that people find it hard to move up a level. Moving up a level requires a person to attend to two things at one time. The topic a person is talking about and the person's own reactions to what he/she is talking about. Part of the difficulty is this splitting of awareness. So, what seems like resistance might really be a low ability or skill at splitting attention.

Another thought is that if a person wants to keep on talking about a topic, and is not ready to go to a different subject which seems unrelated or uninteresting, the person will continue talking about the original topic. This will seem like resistence to going up a level but is simply wanting to talk about what we are now talking about.

Connected with this second possible interpretation of "resistance to going up a level" is the issue of power. One person may sense a loss of control over the conversation to another person as if the topic of conversation is a ball in a conversational game of who is the boss of what we talk about.

A third possibility is that people might have preferences for functioning around certain levels of perception. Vallacher & Wegner(1985) in "A theory of action identification" have developed a psychological test called the "Behavior Identification Form" which measures individual differences in the preferred level of perception. The test consists of 25 items like the following: "What does tooth brushing mean to you--(a) preventing tooth decay or (b) moving a brush around in one's mouth. The former choice is at a higher level, while the latter choice is at a lower level. These authors went on to show that higher scores tend to describe their self-concept in more abstract terms. Higher scorers(on the average) believe that they have more control over their lives, tend to be less anxious, and are less sensitive about other people's comment. So, resistence to going up a level, or down a level, might be wanting to experience the world in terms of certain levels of perception.

A fourth possibility is the one you brought up. There might be defense mechanism control systems whose job is to control bad feelings. If going up or down a level results in increasing bad feelings, these control systems would resist such a change.

As Bill Powers says, we should stop psychologizing and ask the person. If we notice that a person seems to not want to go up a level, we can ask the person about it in some way which is understandable and acceptable. Some of the possibilities I mentioned in this post might show up. But I am sure there are lots more. It is much more important to find out for a particular person in a particular conversation. I have sort of come to the conclusion that we have to pay attention to really getting to know a person in detail rather than coming up with generalizations which sound good but only apply a small percentage of the time. Bill's informal study of the way his pet insect moves was a vivid example of this attitude. If it takes

this kind of individualized, detailed study of an insect to find out how it moves, I can only assume that to understand the reasons why a person may resist moving up a level requires the same kind of efforts. I simply pretend my patients are bugs and give them the attention they deserve.

Date: Sun May 10, 1992 9:16 pm PST
Subject: It's ALL perception

[From Dag Forssell (920510.22:00)]

Ed Ford (920507.11:15)

>So it isn't the standards as such ..... for they can't be
>measured independent of the entire behavioral structure that is the
>operational living control system.

I agree with you. The standards certainly fit in a framework. They are at the 10th of 11 levels in the HPCT structure, as presently defined.

>Rather, it is our whole system operating as a continuous process. This
>involves a whole bunch of things that are all interlaced, interactive,
>and interrelated, each being a part of the whole process.

No argument here.

>I might have to adjust my systems concepts (as when I learned PCT), or
>change a few standards, or alter specific goals or decisions, or change
>my approach to controlling the variable, perhaps by dealing in a more
>effective way with the various obvious and sometimes unforeseen
>disturbances.

You are describing the HPCT hierarchy and noting that you carefully consider how it all ties together in order to function well. We are in perfect agreement. The careful consideration is an important point.

>Establishing systems concepts, setting standards, and making decisions
>is only a part of this process.

Yes, only the three highest levels.

>It also involves being able to control for the right variable, at the
>right time, dealing with both foreseen and unforeseen disturbances,
>learning to "listen to and deal with" our reorganization system, while
>at the same time contending with other conflicting reference signals and
>principles, both within our own system and in the various systems around
>us.

As I read you, you are describing the essence of "Behavior of Perception" in a dynamic environment, and noting how reorganization fits into the picture when normal operation is not enough to control the error signals.

As near as I can tell, we are in perfect agreement - in part because I have learned from you. Since each of us have our individual construct of HPCT in our own heads, we will never have quite the same concept of HPCT or anything else, or the same way to explain or think of it.

I still feel that it is more fruitful for human interaction to focus on Principles/Values/Standards AS A SUBJECT OF DISCUSSION and would like to point out that unless I have misunderstood you, this is precisely what you do when you ask an counseling patient: "What are your priorities?"

You don't ask: "What is your understanding about life?" "What are your beliefs?" or "What is the meaning of it all?" The systems concepts are a very large network of understandings. It is unmanageable to question systems concepts directly in therapy. You would get trapped in a labyrinth and never get out. The standards are both more relevant and more accessible.

I grant you that the person will look into his/her systems concepts to answer the question. "What are your priorities?"

But perhaps not; the problem may be that the person has not spent much time to integrate a set of systems concepts, depending instead on fragments of Principles/Values/Standards as taught by and absorbed without deliberation from parents, peers, siblings, teachers, etc.

Perhaps your question about standards requires the patient to think about the systems concepts deliberately for the first time in a long time and create some. You teach PCT, which provides a good framework for that process, without being (or appearing to be) offensive to whatever pre-existing systems concepts the person may have.

I read into your post another aspect of your therapy: If the person does not know how to solve a problem (Program & Sequence level,) even with newly considered (reasonable) standards, the system does not work. It is an integrated whole! Then you have to teach how to solve a problem, starting with one that has a chance of success. Eventually (hopefully) the person learns to function better at all the (integrated) levels.

Many things come together to shape my systems concepts.

Ever since Luther gave Gustavus Vasa an excuse to grab all the Catholic gold in Sweden in 1523, Sweden has had a Lutheran state church.

From 1st grade through junior college in the public school system, I had two lessons a week in "Christianity." In the later years, it amounted to "comparative religion." I was introduced to the basic tenets of all the major world religions. This is conducive to thinking of them ALL as systems concepts, (with malice toward none, with charity for all) and seeing that one of the major purposes of religious teaching down through the ages is character education: Teaching standards, so that people may function well.

In science and engineering, I have understood since high school biology that the ONLY way into the human nervous system is through the nerve endings of the various senses. With this perspective, it is clear to me that it's all perception. I did not need Bill Powers to make that a part of my systems concept. PCT suggests one way to imagine the specifics. Whether it is done on one level in one massive neural network or in 99 levels of hierarchy is immaterial to the basic premise: It's all

perception.

In the past year I have read Thomas S. Kuhn's book: The Structure of Scientific Revolutions. It could just as well also be called: The Structure of Religious Revolutions. Kuhn makes it abundantly clear that to understand a system of concepts, you must internalize that particular set of concepts. When you have done that, you will see and understand the world through the eyes of those rules, that "paradigm." If it works for you (at least reasonably well), you make it your TRUTH and defend it against all comers.

I have a tape with the speaker Marilyn VanDerbur where she quotes Joan of Arc. Joan has been offered her life and liberty if she will only take back what she has said; deny what she believes in. Says Joan: "The world can use these words, I know this now. Every man gives his life for what he believes. Every woman gives her life for what she believes. Sometimes people believe in little or nothing, and yet they give their lives to that little or nothing. One life is all we have. And we live it as we believe in living it, and then it is gone. But to live without belief and purpose, to me is more tragic than dying. Even more tragic than dying young."

A few years ago, I read Bertrand Russell's: "A History of Western Philosophy" and enjoyed the PBS TV series: "The Day the Universe Changed," by James Burke. It is clear to me that MANY systems concepts, explaining the world around us, have been used, lived by and died for down through the ages, and that is only in the west. It is also my perception that many of these still are in use, handed down through different religions, cultures and oral traditions.

I am now reading Living Control Systems, volume II. Marvelous!

There are many pearls of wisdom here. Relevant to this thread on religion is among others: The Good, the True and the Real:

>When we use the creation of realities in the right way, we discover not
>the nature of the objective world and not the nature of human being, but
>the true outcome of being human in a real universe. It is our own nature
>that we find, but at the same time it is the human reflection of a
>different reality, one that we can never know directly.

There is a Boss Reality, no doubt, but all we individually can know of it is a created reality in our own minds. It's all perception.

I think to say that

>...we discover ....the true outcome of being human in a real universe.

is another way of saying that our systems concepts (the creation of realities in the right way) are validated by our ability to function well, which is Ed's point in the first place. If we develop a reasonable set of systems concepts and reasonable standards to go with them, then we will function well in the Boss Reality.

To wit: If we have adopted standards for a good diet, we have a better chance of maintaining health than if we depend on Jello and prayer. Let me mention that I am in no way against prayer. I think, rather, that it

is the atheist who refuses to engage in introspection and quiet dialogue with himself as an anti-religious posture who loses out on that deal. It is the ignorant dependence on Jello that saddens me, and that is a question both of systems concept in regard to your understanding of nutrition and standards in applying the knowledge.

To say that it's ALL perception seems ridiculous to a person eating breakfast. The world is real enough. Indeed, in millions of experiments since we came of age, we typically never fail to touch an object as intended. The reality is palpable. We grab the cup. The coffee is hot.

A few months ago, Gary Cziko posted an experiment, which I have adopted. (Thanks Gary)! Ask a person (while seated) to cover one eye and push on the other while gazing across the room. All that happens is that the image moves sideways a little.

Then ask the person to stand up on one leg. Challenge the person to remain standing. Repeat experiment.

The point is that our senses are so well calibrated that we fail to notice the difference between the actual and the perception of the actual. But the moment we push on the eye - sensing instrument - the difference becomes obvious.

At a higher level, I have adopted Ed Ford's discussion of the concept of wife. It is quite fun to tell the story of how Christine and I met in a whirlwind of fun and after three weeks, I say: "I love you, do you want to be my wife?" She answers: "I love you, I want to be your wife!" My concept of wife is based on seeing my mother slave away in the kitchen, taking care of six kids. Christine's concept of wife is based on seeing her mother shopping in London once a week, with the household handled by six servants. How long is the marriage likely to last?

So far we have shown that it's all perception at the lowest levels and at the intermediate levels in the hierarchy. Why should anything be more than perception at the highest level? How could you POSSIBLY build certain truth on a foundation of uncertain perceptions? No, it's all perception; all the way up.

In my post on standards, I made reference to a post by Bill that said: It's all perception. All of it. Well, I finally read the instructions for a file find program and located the post. Worth a file server address!

Levels of perception: Bill Powers (920324.0300) (to Mark Olsen)

>Behind this exploration of perception lies a fundamental postulate; if
>you don't internalize it, I don't think you can even get started on the
>problem of modeling the brain's perceptual systems, or for that matter,
>in understanding HPCT. The postulate, simply put, is this: it's all
>perception.

>......In short, take nothing about experience for granted, as if some
>aspects of experience were really outside and others were inner
>interpretations. Put the whole thing inside, and see what you come up
>with when you understand that it's all perception. All of it.

Since the dawn of human experience, people have no doubt tried to make

sense of their experience, to suggest systems concepts which can explain.

In the realm of human behavior among those many concepts are 1) that God makes us do what we do; 2) that our Soul makes us do what we do; 3) that impressions of the environment, (accumulated and presently impinging on us) makes us do what we do. 4) Then there is HPCT, which says that our purposes in comparison with the environment makes us do what we do.

Through loud shouting matches on this net, we know quite clearly that HPCT is not compatible with the environmental behaviorism S-R. S-R purely is a machine concept, directly at odds with the notion of God or Soul.

We do not mention that PCT is also not compatible with the idea that any one particular concept of God or Soul (as OBJECTIVE TRUTH or BOSS REALITY). It's all perception. The concept of God or Soul is quite compatible, however, I think, and perfectly respectable as an individual person's personal systems concept. All that is required for compatibility in every direction is for an individual to recognize and acknowledge: It's all perception.

As organisms, we learn ONLY from experience. Our ONLY source of information is the intensity (or energy) signals we experience from our nerve endings. With a head start in the structure our genes have instructed for the biological machinery, we construct an understanding of those experiences in our nervous systems. (It is exciting to know that the Plooij's may have a book in english by early 1993 that spells out 10 phases of reorganization observed in human infants during the first 18 months of life). One advantage we as humans have for this growth process is the spoken and written language.

By way of language we can share the experiences of others and thus accelerate and multiply our individual experiences. Still, this all has to enter through nerve endings.

Ed, I do not mean to pick on you, but by way of your own example: If Ed has read or been told about a miracle, that is a perceived experience. If Ed has personally witnessed a miracle, this is a perceived experience just the same, subject to Ed's perceptual capability and interpretation. Ed does the perceiving through nerve endings and construction of an understanding in Ed's mind in either case, and both are subjectively real to Ed. No-one has any business questioning Ed's reality. It is his. As I said in my post, I think it obvious that there have to be 5 billion individually constructed systems concepts among 5 billion people.

The strength of existing understanding was the subject of Bill's post on momentum.

From Bill Powers (920224.0800)

>I'm more or less resigned to the fact that when people from other
>disciplines get interested in control theory, they already have built
>up a lot of scientific momentum. They are not starting from scratch.
>They have built up a complex structure of understandings in the course
>of trying to make sense of whatever aspects of life seem most
>interesting. They generally think they've been getting somewhere,
>although an interest in control theory shows that they see some unsolved
>problems. What takes a long time to realize -- years -- is that if

>control theory is a correct description of how people work, then way
>back in the mists of the past, all the ideas they have built upon for
>all these years contain some fatally wrong assumptions. Way back down
>there in the foundations.

In my interpretation, "from other disciplines" applies to all systems concepts, scientific, religious or whatever.

PCT requires a lot of reorganization and takes a long time to grasp because it does provide a complete perspective which is not really compatible with many of the systems concepts people have used with various success since time began.

Things will be much easier 50 years from now, when PCT is taught in elementary school and all the way up. (Unless fundamentalists catch on and object, of course). When that happens, the world will be a better place for our grandchildren. That is worth living, working and dying for!

I have begun to notice that in post after post, Bill persistently and patiently says: It's all perception. All of it. Bill does not always use the same words, but the understanding is always there.

Last August I saw the closed loop handshake for the first time.
May I suggest a PCT greeting to go with it:

Greeting: It's all perception!
Answer: All of it!

This way the greeting will illustrate the two (and only two) fundamental concepts of PCT: 1) The phenomenon of control. 2) It's all perception.

When all PCT'ers have internalized this, discussions of epistemology will go away and discussion of religion will be reduced to a discussion ABOUT religion as a systems concept phenomenon.

In the meantime, I believe that discussion of particular systems concept elements as TRUTHS is pointless, but that it can be very fruitful to focus on the standards which have a much greater universality and direct impact on the functioning of an individual control system. (They are after all one level closer to where the rubber hits the road).

It's all perception!

Dag Forssell

Date: Mon May 11, 1992 9:39 am PST
Subject: epistemology

[From Wayne Hershberger 920511]

Dag Forssell (920510.22:00)
>Our ONLY source of information is the intensity (or energy)
>signals we experience from our nerve endings.

>When all PCT'ers have internalized this, discussions of
>epistemology will go away and discussion of religion will be

>reduced to a discussion ABOUT religion as a systems concept
>phenomenon.

Dag, this is where the epistemological questions BEGIN, not end.

For instance, reflect upon the two concepts, energy and information, used in your first sentence?. Do we experience the energy or the information, or neither? Do we, instead, experience phenomena (the constituents of time and space), as you imply in your second sentence. How are the three concepts related? Energy, information, and phenomena. To each other? To an ecological control system?

What about this greeting? "All that is empirical, is phenomenal."

Warm regards, Wayne

Date: Mon May 11, 1992 9:46 am PST
Subject: Collingwood on Grammarians

From Greg Williams (920511)

I'm posting the following quote not because I know anything about linguistics, but because it is virtually the only thing I think might be worth passing on to netters from Israel Rosenfield's new book, THE STRANGE, FAMILIAR, AND FORGOTTEN: AN ANATOMY OF CONSCIOUSNESS. So this constitutes a sort of review of a terribly vague account of neurology and perception which could use a good dose of PCT.

On pages 118-119 of SFS, Rosenfield quotes from R.C. Collingwood's PRINCIPLES OF ART (1958, page 257) as follows:

"We vaguely suppose it [grammar] to be a science; we think that the grammarian, when he takes a discourse and divides it into parts, is finding out the truth about it, and that when he lays down rules for the relations between these parts he is telling us how people's minds work when they speak. This is very far from being the truth. A grammarian is not a kind of scientist studying the actual structure of language; he is a kind of butcher, converting it from organic tissue into marketable and edible joints. Language as it lives and grows no more consists of verbs, nouns, and so forth than animals as they live and grow consist of forehands, gammons, rump steaks, and other joints."

(Note, Bruce N., that Collingwood was a major influence on Gregory Bateson.)

Greg Williams

Date: Mon May 11, 1992 10:40 am PST
Subject: patients as bugs

[From: Bruce Nevin (Mon 920411 10:12:28)]

(David Goldstein (05/10/92)) --

> I simply pretend my patients are bugs and

>give them the attention they deserve.

David, I sincerely hope this remark doesn't get bandied about out of context !-) (Even a programmer might take exception.)

:-)

Bruce
bn@bbn.com

Date: Mon May 11, 1992 10:54 am PST
From: g cziko
Subject: CSGnet DOWN

[from Gary Cziko 920511.1100]

Due to a major hardware problem (involving smoke, I am told), the machine on which CSGnet depends (called VMD here) is currently not functioning. A team from IBM is reportedly winging its way to Urbana as I type and things should be running again by tomorrow (Tuesday, hopefully).

During this time, messages sent to CSGnet will be saved and sent out when VMD is functioning again. So you may continue to post to CSGnet, but don't expect any CSGnet mail for a day or so.--Gary

P.S. In case you are wondering how I am able to post to CSGnet, it is because I have a backup list which uses another machine.

Date: Mon May 11, 1992 11:12 am PST
Subject: Re: clinical example

[From: Bruce Nevin (Mon 920411 08:36:38)]

I had written some comments on David's clinical example but David said he had not seen them, and I can't turn it up either in my files, so I guess I never posted that message. I will reconstruct what I can.

A key question for me was, how did the wife know he had to be seduced, and how did the babysitter know he had to be seduced, with that peculiar threat "you'd better!"

It's useful to distinguish between sanctioned, overt communication and other concurrent communication which is covert and not consciously acknowledged. It seems to me that there is a lot of back-channel communication that has the effect of people advertising and trying out for parts in one another's psychodramas. The patterned transactions that Eric Berne and his students so cleverly describe I think have this function. (Berne thought they served only to "structure time" and alleviate anxiety about being caught purposeless, so to speak.)

Another question is, why are so many of us anxious when we have freedom, so anxious that we rush to construe ourselves as victims or products of circumstance as quickly as possible? Rather than as authors and products of our own purposes. Could it be that we learn about this kind of fear

and apparent safety as children in our families, schools, and other social institutions?

Is this not at the core of being passive-aggressive?

Is he seeking control that he generally lacks in his life? Or is he seeking to control his perceptions (as all control systems must) without acknowledging that he is doing so? Without revealing any clue as to his inner reference perceptions which might make him subject to manipulation.

The last post from David is more revealing of the wife. Instead of being a cardboard cutout character in the husband's account of the drama, representing a figure of some ominous power who robs him of his freedom, she reveals herself as a woman who feels herself to be the loser in a relationship that she is nonetheless fearful of losing.

The social role of being a woman requires following the dance partner's lead. Reading inner reference perceptions from outer signs and controlling for one's own only while accomodating those of others. This can work well only with a partner who knows what he wants and communicates it well. Lacking either of these requisites, the feminine partner can suffer awkward collapse and embarrassment, or help her partner to conduct the pair of them in a purposeful way. It is hard for this not to be manipulative at best. When she has purposes of her own that she wants to further, then almost anyone would so construe it. The husband here seems to lack both requisites, he doesn't communicate his purposes well, not even to himself, with the effect of not knowing his own wants.

An acquaintance many years ago put it to me this way, speaking out of her experience as a new mother. The baby expresses some desire in an inarticulate way. The parent does what she can to figure it out. Even the best parent can't always. In bad situations the net outcome is a decision by the child: if I express a desire, I won't get what I want, I'll get punished for fussing. At best I'll get a pacifier or some other substitute palliative. If I don't fuss, I might get what I want. (When the parent is ready. Maybe the diaper smells too ripe.) So I'd better not even know what I want, lest I start complaining about not having it. That's an extreme portrayal, but I think there's a germ of truth in it.

It seems to me that it would be helpful to get down from higher levels to the immediacy of lower levels of perception. It's suggested that we resolve intrapersonal conflicts by going up a level. I think we can undermine intrapersonal conflicts by going down a level as well. In most communication problems I know about, something is being ignored. Very often something is being imagined, too, but always something is being ignored, and it is easier to get acknowledgement of perceptions that are there than it is to get relinquishment of perceptions that are not there.

The stair which they both traverse many times every day accidentally and unexpectedly made a noise. She called his name in the dark hallway. A key communication transaction. For each, certain perceptions constituted input for comparison with higher-level references, and other perceptions were ignored. They live in very different construals of the same

events, construals that are in many ways complementary to each other.

Where one ignores some particular perception that is a key ingredient for the other's construal of events, perhaps ways can be found for the other to acknowledge having that perception too, though it may be irrelevant to the second one's higher-level perceptions or (more challenging) though it may be inconsistent with them.

Where memories disagree perhaps ways can be found for experiencing some lower-level perception just as itself, rather than in light of the higher-level construal; perhaps its ambiguity at the higher level can emerge; perhaps behind that its essential simplicity, prior to interpretation in accord with higher-level expectations. Maybe this can happen where one remembers some lower-level perception and the other does not. The same lower-level perception can have different significance for each. Lack of memory on one side can be the limiting case, where "significance" on one side drops to zero. Or it can be that the perception on one side was supplied by the imagination loop. ("Screen memories" are a more extreme example.) Or it can be that lack of memory is due to denial, because the perception is inconsistent with and implicitly challenges the higher-level construal. In this case there should be other indicators of a conflict to which the particular "blanked out" perception is a clue.

Was his heart beating fast as he was sneaking down the stairs? Can he acknowledge such a perception?

Have you interviewed the babysitter? The wife revealed a bit about the relationship of the two women. If she's a live-in babysitter who challenges the wife at her role of seducing the husband, she's very much a member of this family system.

Hope this is suggestive. Good luck! I have the greatest respect for you guys who are applying this stuff with troubled people in real time.

Bruce
bn@bbn.com

Date: Mon May 11, 1992 12:31 pm PST
Subject: re.: patient as bugs

From: David Goldstein

For people who do not know my strange sense of humor, I just want to make clear that I was joking in the last sentence of my post.

When I wrote this, I had a background thought that maybe it could be misunderstood. Then I dismissed this with the thought that this was unlikely.

I will try to suppress my strange sense of humor in future posts or at least make it clear when I am joking. Sorry, if this caused some concern.

I do not treat my patients like bugs, really.

David Goldstein

Date: Mon May 11, 1992 1:55 pm PST
Subject: psychotherapy

[From Rick Marken (920511)]

David Goldstein -- I was working on a reply to your last post and I managed to overwrite the file. I just wanted to say that I agree with your suggestion that "resistance" in the method of levels may be more of a skill thing than an active avoidance of going up a level.

In fact, I think one of the skills of psychotherapy may be the ability to help someone (by knowing when to ask the right quesions) go "up a level". It might even be something one could practice on their own; I'd like to learn how to navigate more skillfully through my own hierarchy. Maybe the method of levels could be the basis for a nice "self help" system based on PCT? A book of exercises might be nice. My computer demo for the "Behavior of perception" paper might be a way to start on the lower levels. Are there any nice exercises one might suggest for looking at different levels of one's own perceptual experience -- especially the top four or five?

Best regards Rick

Date: Mon May 11, 1992 2:33 pm PST
Subject: Premature Warning

[from Gary Cziko 920511.1500]

Sorry about the warning that CSGnet would be done for a day or so. As soon as I posted the message, it seemed to fix itself. Things seem fine now.

At least I got the chance to try out my backup communication system.--Gary

Date: Mon May 11, 1992 3:52 pm PST
Subject: It's ALL perception

[From Rick Marken (920511 14:30)]

Dag Forssell (920510.22:00) says:

>Ed Ford (920507.11:15)

>>So it isn't the standards as such ..... for they can't be
>>measured independent of the entire behavioral structure that is the
>>operational living control system.

>I agree with you. The standards certainly fit in a framework. They are
>at the 10th of 11 levels in the HPCT structure, as presently defined.

I think this discussion could be cleared up for me a bit if someone (Ed or Dag) could tell me what the word "standards" means in this context. I think of standards as specifications -- so for me "standard" is a synonym for reference level for perceptual variables. You guys seem to be using the word "standard" to refer to a type of perceptual variable (like a principle or system concept). What do you mean by "standards"???

> one of the major purposes of religious teaching down through
>the ages is character education: Teaching standards, so that people may
>function well.

Are they teaching you how to perceive "standards" -- like "thou shalt not X and thou shalt not y are examples of standards, kiddies". Or are they teaching you where to set your references (standards) for certain variables that the church already assumes that you can perceive -- like "I know you can perceive many different gods but you better set your reference for perceiving YHWH as numermo uno -- or fry, bubby". I think that you meant that religions teach standards in the second sense -- "set your reference for these perceptions here -- or else". Is this correct?

I would suggest that religions do try to teach people where to set their reference for certain perceptions. I think this is not a good way to help people to function well -- in fact, its just about the worst thing you can do to many people. It would only help if 1) everybody perceives the work in the same way 2) uses words exactly the same way in desribing those perceptions (so that everybody knows an "abonination" when they see it) and 3) lives in a world that produces exactly the same disturbances for everyone so that certain reference settings are always the right way to correct for disturbances of higher level perceptual variables. I think its safe to say that the propobaility of any one of these conditions being met is close to 0. The probability of all three being met is thus 0 * 0 * 0 = 0.0000. This is my estimate of the probability of religion being a reasonable to solution to the real life problems any individual living control system.

But it's worth a try.

>There is a Boss Reality, no doubt, but all we individually can know of
>it is a created reality in our own minds. It's all perception.

Yes indeed.

> If we develop a reasonable
>set of systems concepts and reasonable standards to go with them, then
>we will function well in the Boss Reality.

I would rather say that, what we develop to function well in boss reality are CONTROL SYSTEMS. We develop means of perceiveing and of influencing those perceptions such that they are controllable. There are unquestionably ways to perceive and act that make control impossible; the solutions we develop for controlling our perceptions are constrained by boss reality. I must, for example, learn to exert forces on the steering wheel that bear a particular relationship to my perception of the angle between my car and the center line in order to control that angle. But there is not a "right" way to set the references for that force -- since the amount I exert depends on continuously varying disturbances acting on the car.

Your statement above implies that there are "reasonable" ways to set references (if standards mean references) for perceptual variables If this is what you meant then I must disagree. Reference settings depend on the goals of higher level systems AND disturbances to the variables controlled by those systems -- there is no one " reasonable" setting for references at ANY level of the hierarchy. Their can't be -- and imagining

that this is so can lead to internal conflict, interpersonal conflict or self-destruction (I think that's what happened to Joan of Arc in your example -- lack of willingness to adjust a reference to control another variable; she imagined that there are absolute references. That's her choice, of course, but for, as for me, give me liberty or let me outta here).

>To wit: If we have adopted standards for a good diet, we have a better >chance of maintaining health than if we depend on Jello and prayer.

Maybe, maybe not. As you say in the post, "it's all perception" and all you do is control perception. If you can control the perceptions you need to control with jello consumption then it's fine -- chance has nothing to do with control. You either control the perception or you don't -- and you reorganize. If prayer works to control the perception you are trying to control, then great -- if not, not. No chance involved.

If one eats vegetables to increase their chance of living longer then I think they are controlling an imagined perception. If one eats vegetables to feel better -- and they feel better when they do eat vegies and worse when they don't -- then they are controlling some perception or other and its fine. Some people eat steaks and wash it down with a whiskey to successfully control the same perception. There are many ways that can (and, because of boss reality, sometimes must) be used to control the SAME perception. I think it's just important to be sure one is controlling perceptions and not just imaginations because the perceptions could be getting out of control behind one's back.

>It's all perception!

Yes, it is all perception. But we have to live with the fact that we want some of those perceptions to be a certain way -- we want to control them. And to do that we have to be able to develop systems that will take into account the constraints of our oun nature (the fact that we are controlling many perceptions at the same time) and the constraints of boss reality. And a control system only works (controls) if it can vary its output to compensate for disturbances to the controlled perceptual variable. These outputs are often references for lower level percpetual variables; so the last thing you need in an effective control system is a "pegged" output -- one that does not vary. So a control system that believes that there is only one reasonable output (reference) value for another control system is, to my way of thinking, nothing but a big problem -- whether that control system exists within our own hierarchy or in someone else's hierarchy. Control systems that think that there is just one "right" reference value for a perceptual variable are the control systems that really need to learn PCT!!!

Regards Rick

Date: Mon May 11, 1992 7:37 pm PST
Subject: clinical example--4

To: Clifford Gann, Gene Boggess, interested CSGnet others
From: David Goldstein

Subject: clinical example continued
Date: 05/11/92

I have received some comments on the clinical example from the above named people. I have also had an additional couple and individual session.

Clifford corrected my summary of his position: The man was not only controlling for sexual satisfaction but for the sense of not being controlled by his wife. I have previously said that I didn't think it was sexual satisfaction per se (described by the man as equal to the experience of masturbation) but other things this man obtains from the babysitter.

I have not guite pinned down what these other experiences are which he receives from the babysitter but not from his wife. In the most recent individual session with him, he described the things about his wife he doesn't like, namely, she is bossy, demanding and moody. I have used the method of levels to explore the meaning of these experiences for him but need to do some more exploring of them.

Gene Bogess (in a private message) stressed the importance of working on communication with this couple and the importance of having common experiecnes. He supported the idea of using Ed Ford's Quality Time Program as one way of beginning to work on these areas. Gene expressed the idea that love between two people is based on understanding which is based on communication which is based on common experiences. One novel intervention was to bring the babysitter into a session. Gene, as you can see, I am doing this in imagination. If I did this in vivo, I think I would have to later arrange for blood, and possibly a dead body, to be removed from the floor.

Gene also expressed some doubt that HPCT could contribute to the understanding which is the basis of two people loving each other. On this point, I think I disagree. If I am sucessful in helping to identify some of the more important wants (desired experiences) of this man and woman, I think they will understand each other better. Once they know what the other wants and doesn't want more clearly, each is in a better position to make a choice of how, or whether, to meet the want. HPCT helps by telling me that I should be focusing on discovering the wants (and don't wants).

The most recent couples session I had with them was very emotional. The body shaking I mentioned last time turned into 100% rage. She hit and kicked him a few times during the session as she verbally expressed her rage and then moved to a different chair. She was very powerful as she expressed her rage and hurt. The rage made reference to the following kind of thoughts--I wanted you to protect me and the children. You allowed this woman to come into our life. You didn't care about me or the children. This woman was telling me about an affair she was having with someone. We were talking about it together as friends do. Now I find out it was you. She said that throughout her marriage she was having thoughts that her husband was having an affair but she

kept on dismissing these thoughts. In addition to being angry at her husband and the babysitter, she is angry at herself for being so stupid and gullible. I am afraid that this experience has only confirmed her sense of not letting people get close to her.

During the couple's session, the woman expressed that she was closer to her father than her mother. Her mother has suffered some brain injury related to a medical condition and needs a lot of care. The mother, therefore, is in no position to offer emotional support. The woman expressed wanting her father back. He was the only one who nurtured her.

The husband, in the session, was going over the story of how horrible he was during this whole sordid episode. He was publicly flogging himself with words so that I would really understand how terrible he was. He was trying to stay calm while his wife was emoting. He kept on expressing that he wanted to work it out and that he loved her very much. The husband allowed his wife to hit him without moving or saying anything. He sat there and took it.

On her own, the wife sent the husband of the babysitter a package which included every present the babysitter every gave to anyone in the family. The presents were smashed and torn into little pieces. The note revealed to the husband what has been happening. The wife said she did feel good about doing this. The revenge against the babysitter is not yet over I am told.

In the most recent individual session with the man, we discussed his parents and siblings. He is the oldest child. He has a younger brother and sister. The father was very controlling on the one hand and very permissive on the other hand. As long as he performed in school, there were no other expectations around the house. The father used to beat him when he was a child. The father had an awful temper and was very impulsive. His mother was more a friend. She used to talk with him quite a bit. He considered her almost as a friend. In college, she came into his bedroom and smoked pot with him. From the conversation about his parents, I pictured him as having been spoiled which he agreed with. I also pictured him as having no respect for woman which he agreed with.

He described an incident at home this week which sounded like a turning point to me. His wife had just told him she wanted him out of the house. He called me and we spoke. Then he went up to his room and cried very strongly for a long time. His wife came to the room and comforted him verbally and held him. He described the experience as being very sad, thinking that he had lost his wife and children, feeling empty inside.

Date: Mon May 11, 1992 8:12 pm PST
Subject: grammar, control

[Avery Andrews (920511.2048)]
(Bill Powers 920502.1200)

Observations of grammatical errors and subsequent corrections are

useful (people do this sort of thing, and I'm pretty sure that both Bruce Nevin and Penni Sibun know more about it than I do), but I don't think they are a substitute for ordinary grammatical observations, for the reason that you can't apply the Test, and systematically create disturbances for them to neutralize. All you see is them changing their mind about what to say and backtracking a bit, and there's really only an inference that control is what is involved, and you don't have much of an angle on what they're controlling for either (sounding cultured? like a real person? or what?)

Extended work on an individual's language is also problematic from the point of view of both practicality and ethics--to record really large samples of someone's ordinary use of language would pretty well trash their privacy, and those of the people they were talking to. I suspect that it wouldn't be worth it, either, on the basis that an individuals `internalized grammar' is a selection from what's floating around in the speech community, & there wouldn't be much significance to which bits happened to fetch up together and any particular person's brain.

Of course the standard generative grammarians `can you say this: ...' method also has pretty strong limitations. Avery.Andrews@anu.edu.au

Date: Mon May 11, 1992 8:18 pm PST
Subject: Collingwood on Grammarians

The Collingwood quote is probably closer to the truth than grammarians would like to admit, except that at least some of us see ourselves as trying to find the natural joints to carve the thing up along.

Avery.Andrews@anu.edu.au

Date: Mon May 11, 1992 9:43 pm PST
From: Dag Forssell / MCI ID: 474-2580
Subject: replies

[From Dag Forssell (920511.22:45)]

Rick Marken (920511 14:30)

>What do you mean by "standards"???

In my posts I have tried very hard to make the connection to the Principle level (as detailed in Powers' and Robertson's book). The word value is in there too. To Ed, it is at the level of Understanding and Belief, if I understand him correctly. I think it belongs at the principle/standards level, if it belongs at all. It signifies a judgement as to what is important among the things you understand/believe.

Please refer to my original post on standards for a graphic representation of my interpretation.

>I would suggest that religions do try to teach people where to set their
>reference for certain perceptions. I think this is not a good way to
>help people to function well-- in fact, its just about the worst thing

>you can do to many people.

As I have talked to you and read your posts for a long time, I get the impression that you think that "people will do what they will do" regardless, and that as a fellow human being you have no business influencing them. You did admit to me once, that you just might have influenced your kids along the way. How? Did you per chance teach them where they might profitably set their reference perceptions, so that they might function better?

Dr. Spock told a generation of parents to leave their kids alone, and let them do whatever they pleased. I suppose those kids earn the highest incomes and have the happiest marriages now. Surely they must function well, since no-one tried to "control" them when they were little.

I think that just about the only thing that separates humans from animals is the ability to suggest reference perceptions which the young can adopt because they choose to.

Greg Williams (920508) recently commented on the tendency of PCT debaters to bury their heads in the sand, when it comes to "social control." Influence is a form of social control, for sure. Why be afraid of it?

Influence is for real, and it is important. The world is not populated only by well behaved, adult PCT academics, who object to being "controlled" by others. To pretend that positive influence through teaching "standards" or "principles" is A) impossible or B) bad is a cop out. Parenting, management, teaching, leadership and counseling are about that.

When you make an earnest effort to help people manage themselves better, (because they have hired you for that or because they are your kids), you are faced with the real question of how to influence them positively and effectively. You cannot duck and talk theory alone, but it sure helps to have a good one. You cannot afford the time and confusion of dealing with everything all at once. You have to figure out a good place to start. I know of no better application of PCT and set of suggestions on that subject than Ed's book: Freedom From Stress. Have you read it? Ed shows how to question people so that they will reason with themselves, but he also suggests and teaches. Ed is a master of positive influence.

I have wanted to try on the net my thought that the level of principles is key, and the suggestion that there are some well defined, universally acceptable reference perceptions or "standards" that have worked well for a lot of people over time. Character education is, I think, a very useful form of "social control" that is vitally important, no matter where it comes from. Of course, it is also important that this same character education is not misused, as historically has been the case in many times, religions, places and cultures. Greg might call it a double-edged sword. But the total absence is a disaster, for sure. That is why I think it makes a good subject for discussion.

Wayne Hershberger 920511

>.....reflect upon the two concepts, energy and information, used in
>your first sentence?. Do we experience the energy or the information,
>or neither?

I recognize that my nervous system exists as part of that "Boss Reality" we talk about. But the only thing I will ever know about it is by way of the intensity signals. (Ed uses the word "sensing energy," which is offered as a synonym. Clinicians take liberties with the terminology when dealing with their customers). I cannot know what causes those intensity signals. The term intensity signal is already a perception. My nerves construct information from the intensity signals. As I study the nerves, I form perceptions of them. I experience the information, and that experience is a perception. It's ALL perception.

>Do we, instead, experience phenomena (the constituents of time and >space), as you imply in your second sentence. How are the three >concepts related? Energy, information, and phenomena.

You are using the word phenomenon in some meaning or perception of yours which is far from what I had in mind. You can strike the word from my sentence. I was just talking about Systems concepts. A level of perception.

Anyhow, "phenomena (the constituents of time and space)" are perceptions. You perceive time. You perceive space. The phenomenon of control is a perceptual construct.

It's all perception!

Dag Forssell
23903 Via Flamenco
Valencia, Ca 91355-2808
Phone (805) 254-1195 Fax (805) 254-7956
Internet: 0004742580@MCIMAIL.COM

Date: Tue May 12, 1992 4:52 am PST
Subject: bugging David

Ah, now it's my turn to apologize. I was only mock-aghast, and responding to the humor in it, David.

There is a serious thread under this, however. Email is notoriously treacherous because there is no body language to flesh out the message. Humor is especially apt to be lost. Hence the smiley :-) and other CRT icons. I should have included one ;-(

Bruce

Date: Tue May 12, 1992 4:52 am PST
Subject: Re: It's ALL perception

[From: Bruce Nevin (5/12/92 08:12)]

(Rick Marken (920511 14:30)) --

I think Dag is using the term "Standard" to refer to perceptions on (current) level 10, Principles. I saw this fly by in one of your long posts, Dag, but don't recall the date. Did I get it right?

Bruce

Date: Tue May 12, 1992 5:48 am PST
Subject: Re: clinical example

David Goldstein (05/11/92)

>The rage made reference to the following kind of thoughts--I
>wanted you to protect me and the children. You allowed this woman
>to come into our life. You didn't care about me or the children.

>she is angry at herself for being so stupid and gullible.

Discovering oneself stupid and vulnerable can be amusing when there is no fear. What is she afraid of? What does she want a man to protect her from? It sounds like the babysitter is a threat only insofar as loss of her husband would make her vulnerable to something else.

Ignoring perceptions is hard work. Puts one under a lot of stress.

Waving semaphor flags can be hard work too. "This is what I need, but I can't risk saying it explicitly." The threat of abandonment or eviction is a big flag to wave. Responding to the need of one who denies having it, and for whom exposure of the need (vulnerability) is a threat, is itself a risk.

What kinds of perceptions are these needs and responses and threats and semaphor flags? Imagined? At least partly, I think.

I think you undermine the imagination loop by focussing attention on perceptions at a level below the loop. Gendlin's approach depends on sensory anchoring. So does NLP. Probably many others. What were the physical sensations in the body when she was enraged? When he was grieving? When she was comforting him? How does it feel to recall those physical sensations now? Do you find that anchoring in lower-level perceptions is helpful in your therapy sessions?

Bruce
bn@bbn.com

Date: Tue May 12, 1992 8:45 am PST
Subject: Greetings!

[From Chris Love (920512.1200)]
[To CSG-l]

Hello everyone,
I have finally gained access to csg-l after a few difficulties at our base. I suppose some intros. are in order so I'll get these over with first. My name is Chris Love and I'm a contractor working with Martin Taylor. My background is in electrical engineering (from University of Manitoba). Martin has had me start work on a somewhat parallel system to Bill Powers "Little Man."

This work is being done on the Macintosh computer and developed using a

language called Prograph. For those of you who are not familiar with language I will briefly describe it, since it is very interesting. It is an object orientated language. The beauty of it is that, in the Macitnosh philosophy, there is no line-by-line programming. To build a program, one simply draws symbols on a window (a symbol may be a addition square,etc.) and then connects the data lines (really lines) to the input terminals and the sum is produced from the root. You can think of this methodology as drawing flowcharts that actually work in the same manner as line code. And, if you have a color monitor, it look great too!

Ok, so what have I done with this??? Well, as I mentioned, I am in the process of developing a similar application to that of Bill's "little man". Initially, I spent my efforts developing the cool 3D environment and the routines to move the target around as well as rotate the box and my "little baby" about the 3 axes.

This little baby I speak of is somewhat like Bill's (I hope most of you are familiar with this model/software - if not ask me to provide more details and I will). The little baby has one arm (only basics now), which includes an upper arm connected to a shoulder and a lower arm connected to the elbow. The baby is pinned to one of the walls of the box. He has a head and a neck and two eyes. The objective of the baby is to dynamically move his finger to the location of the target in the box space. Why do we need to rotate this entire problem about three axes one may wonder? Well, because then you, the observer,
may view this experiment from any perspective!

The next question may be how does the baby know how to move his finger toward
the target. Oh yeah, the target has six buttons to move the target along each axis. This is what you are supposed to do. Ok, back to the movement problem...
The baby uses "retina projections" to provide the percepts. First off, I'm no vision expert (only 4 weeks experience here, ok) so go easy! Basically, the baby receives the X,Y,Z of both the target and finger tip and translates these back to each eye and then divides by the depth coordinate. This gives the baby 8 percepts to feed to his "brain"; 2 coordinates from the target and 2 for the finger tip, and this applies to both eyes. His highest level percept is to minimize these, i.e., put the finger on the target.

One really neat thing I've done (based on a suggestion from a fellow contractor) is to "draw" two retinas on the screen and project *what* the baby sees on to them. So, as the baby moves his finger you see the target and finger tip move on his retina. Quite exciting, especially when the distance between the two of them gets smaller, i.e., it's working!

Well this phase was only to scratch the surface since the actual "control", if one could call it that, was derived from 3 rules. The shoulder, which has 2 degrees of freedom, use two of the rules. The first was to rotate the arm across the body, and the second was to rotate it beside (up/down) the body.
The third rule was to control the elbow movement.

Ok how did the baby resolve depth??? It's a good question. Well since this was a VERY simple model, and since he only had 2-D coordinates to work with, he used this third rule to resolve this issue. What happens is that, "if the sum of the squares of the target is smaller than the sum of the squares for

the finger then the target is further out than the finger, therefore increase the elbow angle, thereby moving the fingertip out further." This is, I suppose, what vision experts call the binocular vision alogorithm??? Anyways, it seemed reasonable and it worked (most of the time!!!). There are a few cases, as one may find after thinking a bit, in which this rule fails miserably. But, to summarize at this point, this phase was to get "things" (the environment) up and running.

The next phase, which I am currently deep into thought and development over is to build some *control* into this control system. The system will be composed of about seven elementary control modules (ECM). Each ECM has the percepts/reference signals, which are normally associated with it as well as the comparator and a few shifted sigmoids to smooth things out. So the highest level reference signals are zero inputs (math finger to target) and
the lowest levels ones are the 8 2-D retina values. The outputs to the external environment are the 3 angle adjustments (shoulder - Phi,Theta and elbow - Alpha). These are delta values which are then added to the existing angles. The baby automatically recieves these changes regularly and automati-lly so the experimenter simply has the job of moving the target away from his
finger tip. What a nice person the experimenter is - always taking the ball away from the baby!!! Well, once in while I let the baby have it while I check
some calculations!

So, how does these ECSs' learn anything?? I intend on using a basic Hebbian learning rule to train the weighted connections in the system to respond to the the two way signals, thereby learning the space (I hope). I haven't gotten to this point yet so any suggestions are graciously invited.

Well that's all for now. I have another 15 min. for lunch so I'm off!
Take care,
Chris.

Date: Tue May 12, 1992 10:09 am PST
Subject: reference standards

[from Joel Judd]

First, who is Z_GANNCP@CCSVAX.SFASU.EDU ???

The standards discussion appears to want to continue, soooo...Rick (920511) said, regarding religions telling people where to set their reference for certain perceptions:

>I think this is not a good was to help people function well...it would only help if 1) everybody >perceives the work (SIC?) in the same way 2) uses words in exactly the same way...3)lives in a >world that produces exactly the same disturbances...

I think it functions very well if a religion has a "Do all you can for others but be responsible for yourself" ethic at its roots. In this way, you try to point out to someone what kinds of things have worked for you and others, but you do *not* force them to act in your image.

The thing about principles that I think gets confusing sometimes is the distinction between how we label the principle and what we *DO* that we interpret as reflecting it. I don't think there is anything wrong with telling someone: Don't lie! It's bad. But there is always someone (invariably someone older and "wiser") who asks: But what about if the Gestapo is knocking at my door asking if there are any Jews in my basement? Here we have a particular experience, not a common one by the way, where I have no problem telling the officer "No." But that doesn't make lying good! And my 3-year old certainly doesn't understand when I tell her "Look, mistruths are generally not good, and telling them will contribute to a type of character most people don't appreciate, so you should always tell the truth, except when your mother asks you if her green hair is beautiful, or your friend asks you if her dying Mom is going to get better, or the Gestapo knocks at your door...Besides proposing standards for people to follow, religions also usually provide guidelines against which to check your personal interpretation of the standards.

We ALL teach standards to others, whether we consciously recognize it or not. Being grown-ups and knowing so much about everything, it's sometimes tempting to let the benevolence in us make us reluctant to teach the things that really do bring happiness to people's lives, in the name of not infringing on their "rights" or "freedoms" or "autonomy" or whatever.

There's actually one more piece to this picture for me, but I'll wait and see if this topic continues before using up more space.

Date: Tue May 12, 1992 10:09 am PST
Subject: Influence and control

[From Bill Powers (920512.0930)]

Rick Marken (920511) --

>Control systems that think that there is just one "right"
>reference value for a perceptual variable are the control systems that
>really need to learn PCT!!!

Dag Forssell (920511.2245) --

>Greg Williams (920508) recently commented on the tendency of PCT >debaters to bury their heads in the sand, when it comes to "social >control."

>Influence is a form of social control, for sure. Why be afraid of it?
>Influence is for real, and it is important.

Influence is not control unless you (a) insist that your influence have a particular effect on the other person, and if it does not, (b) apply whatever means is necessary to make sure it does have that effect.

Influences should be thought of as disturbances. That is, you can perform an act that by itself would alter the other's perceptual world if it were the only influence. But you realize that you can't determine the OUTCOME of that act in the other person. We tend to use the same word, influence, for the act we perform and for its effect, just as we do with the word "disturbance." Setting an example is an influence in that it presents a

situation to another's perceptions. But it doesn't necessarily HAVE an influence, in the sense of altering the other's way of behaving. Even if it does alter the other's behavior, that change may be simply a way of counteracting the influence, and will disappear as soon as the influencing act ceases. Of course what we hope for is a more or less permanent change in the other's way of doing things -- but that result comes from the other person's way of dealing with and understanding the influence. We can't make it happen from outside that person. So it's important in using the term influence to distinguish between the act we perform that's intended to have an effect, and the effect that actually results, or doesn't result.

Parents influence their children by (for example) advice, commands, example, demonstration, and story-telling. Children generally being eager for new experiences and not being very sure of themselves in situations beyond their capacities, they normally latch on to these influences and adopt from them whatever fits their growing organizations.

If, however, they don't adopt some of them, or reject some of them, the parents may then resort to punishments and withholdings as a way of trying to make their influences have the desired (by the parent) effect. Then we get all the ills that result from concerted attempts to control other control systems. The children learn, in protecting themselves from direct external control, how to satisfy the parents' reference levels and thus remove the pressure. They learn to lie, dissemble, conceal, misrepresent, pretend, and otherwise give the impression of compliance while internally isolating themselves from their parents. They become, in short, alienated from the adult world.

Of course a lot of the children simply buy into the system and save themselves all that trouble.

Social influence is not social control. But it's hard to learn how to influence (act on) other people while accepting completely that they will not be influenced (be changed) if that is their choice. When we exert influences on other people, hoping for some change in their behavior that's to our own liking, it often happens that there's no visible result. What do we do then? If we just try harder, we're falling into controlling another person, or trying to. If we give up, we haven't achieved what we want. It's hard to find the middle ground, where we give it a good try but on detecting serious resistance give a higher priority to respecting the other's will as much as our own.

I'm not saying that one should never try to control other people. If a kid runs out in the middle of the street, we whisk the kid to safety by whatever physical means is required. If we're being mugged, we do whatever is required to protect ourselves or those we care about. Not everyone goes around respecting other people's wills. We can't just pretend that everyone in the world subscribes to the same system concepts. Well, we can, but it's not always wise.

What really counts is our understanding of human nature. If we understand that all people are basically as autonomous as we are, then we wouldn't want to encourage a system in which autonomy is ignored or overridden by force as a matter of policy (the present most popular system). With that understanding, we try to deal with others in a way that encourages them to understand things the same way, and to realize that if they want to continue being autonomous, they have to support a system in which autonomy

is generally accepted as a fact. Once you see that basic concept, you understand the problem we're trying to solve in our social interactions. There's always a conflict between what we want other people to do and what they want to do. If we begin by respecting the will of others as much as our own, there are certain kinds of resolutions of the conflict we will avoid using as long as possible. We will spend more time trying to find clever ways to satisfy all of us, and less time plotting how to get our own way regardless. It seems to me that that would be a pretty nice world to live in. I'd like to persuade others that it's worth a try. But of course I can't control them into doing so.

Best to all, Bill P.

Date: Tue May 12, 1992 10:58 am PST
Subject: Character education vs control education

[From Rick Marken (920512 11:00)]

Dag Forssell (920511.22:45) says:

>As I have talked to you and read your posts for a long time, I get the
>impression that you think that "people will do what they will do"
>regardless, and that as a fellow human being you have no business
>influencing them. You did admit to me once, that you just might have
>influenced your kids along the way. How? Did you per chance teach them
>where they might profitably set their reference perceptions, so that they
>might function better?

I want people to be able to control their own perceptual variables as skillfully as they can without interfering with the ability of other people to control their own variables. To the extent that one can help another person (or child) to control more skillfully then that is great. I don't care what people want to control (as long as in doesn't interfere with what I want to control) -- I only want them to be able to control it. My motto is; a control system in control is a control system that's a pleasure to live with (unless that control system is trying to control you or the things you want to control -- relative to a different reference level).

If we take the hierarchical control model seriously then I don't see how anyone could possibly know how to tell another control system 'where they might profitably set their own references for their perceptions'. This doesn't mean that I would not suggest a reference (or force the results that would be produced by having that reference) under certain circumstances. The classic example is "wouldn't you tell your kid not to run out in the street"? You bet your sweet bippy I would (and did) and I would physically haul them back out of the street if they were in it--reference or not. But I certainly wouldn't say that what I am doing is suggesting a profitable reference setting for the kid. I'm suggesting ways that the kid might want to control the perception of getting hit by a car. If I could (which the model says I can't anyway) get the kid to have as a reference "don't run into the street" then what happens when the street is empty and is the only refuge from a group of bike riders barreling down the side walk. Sometimes the "running into the street" reference is good to have set at "yes".

And that's my point; the HPCT model says that there just CANNOT be a right or profitable setting for a reference signal; reference signals MUST be able to vary due to disturbances from the environment or the actions of other control systems. What is a good reference setting in situation A will be a bad one in situation B.

What is important in the HPCT model is not the particular setting of any reference (even the higher order ones that you call standards) but the fact that references vary as part of a closed loop that produces CONTROL OF PERCEPTIONS. Of course, the HPCT model could be wrong and there could be a RIGHT set of references at some or all levels. But I'd need some evidence before I reject a model that seems to work so well at making detailed, quantitative predictions of behavior. As it sits, the HPCT model rules out the possibility of "correct" references --except where "correct" is defined as that setting of the references that leads to actions which, when combined with prevailing disturbances, produces CONTROL. And this just means that "correct" is going to change all the time (sometimes you MUST run into the street, sometimes you MUST NOT -- if you want to CONTROL otehr variables).

>I think that just about the only thing that separates humans from animals
>is the ability to suggest reference perceptions which the young can adopt
>because they choose to.

Humans ARE animals. What's wrong with being an animal?

What humans (and other animals) do is teach their offspring how to CONTROL -- not what level to keep a particular perception, no matter what. I suppose part of teaching control is suggesting references for a perception ("try to bring your arm father back on the backswing") but I think the learner is just exploring the ability to vary that perception as a means of controlling others. What a good teacher teaches is HOW to control -- not "WHAT to control no matter what".

>Influence is for real, and it is important. The world is not populated
>only by well behaved, adult PCT academics, who object to being
>"controlled" by others. To pretend that positive influence through
>teaching "standards" or "principles" is A) impossible or B) bad is a cop
>out. Parenting, management, teaching, leadership and counseling are about
>that.

I'm not saying that teaching "standards" is impossible. I'm saying that if people actually adopted fixed standards that'd be dead in the water; they would not be able to control higher level variables.

I can't help thinking that I am "well-behaved" because I have pretty good control of the perceptual variables that I need and want to control. I have to believe that MOST of those who mis-behave are doing so, NOT because they havn't learned about "right" reference levels for certain perceptions but because they can't control much at all -- let alone what you might suggest as the profitable things to control. Society has been trying to make people "well behaved" by teaching them values, good "standards", etc for CENTURIES. But there are still plenty of mis-behaved people -- especially in places where people have the least ability to control their own perceptions (due to lack of education, money, skills, resources, etc etc). (I have noticed very little serious misbehavior in Bevery Hills; and I hear that Valencia is a very safe place. Is this because the people in these people have learned

the correct "standards"? I think it's because they have excellent control of what they need to control -- and not such hot standards someimes). I believe it is lack of CONTROL that you perceive as mis-behavior, not lack of "good standards" and I find it mean-spirited and coersive when people claim that the solution to "mis behavior" is getting there people to learn better values (standards). How condescending; where is Chuck Dickins when we need him. I think "teaching standards" is just that ol' time religion again; it's certainly not HPCT.

As for influencing my own kids -- of course I want to influence them. But what I really want is for them to be skilled controllers; able to deal with a world filled with unpredictable disturbances that does not allow for inflexability and simple soluitons. I want them educated and loved (so that they can learn with poise). I don't know how to teach control; but I know it's not by teaching the "right" references. One thing that is involved is a respect for the fact that the kid is the only possible system that can know when it's references are set properly; it's when then is a minimum of error at all levels of the hierarchy. My kids are (so far) splendid control systems; that's all I ask (and that they call on father's day).

> Character education is, I think, a very useful
>form of "social control" that is vitally important, no matter where it
>comes from.

And I say --forget character education. To the extent that you are in the position to do so, teach people HOW to control (and keep a good supply of degrees of freedom available for allowing that control -- ie, prevent over- population) and you will end up with a bunch of very nice characters.

>It's all perception!

It's all control.

Regards Rick

Date: Tue May 12, 1992 12:19 pm PST
From: mcnamara
Subject: RE: Character education vs control education

Rick Marken (920512 11:00) says:
>
> To the extent that one can help
> another person (or child) to control more skillfully then that is
> great.

When re-organizing (which kids are doing fairly continuously) control systems may select different references, or the set of control systems may change.
Obviously this is why kids pick up on so much of our own (parents) world view.

> As for influencing my own kids -- of course I want to influence them. But
> what I really want is for them to be skilled controllers; able to deal
> with a world filled with unpredictable disturbances that does not allow

> for inflexability and simple soluitons. I want them educated and loved
> (so that they can learn with poise). I don't know how to teach control;
> but I know it's not by teaching the "right" references. One thing that
> is involved is a respect for the fact that the kid is the only possible
> system that can know when it's references are set properly; it's when
> then is a minimum of error at all levels of the hierarchy.

> I'm not saying that teaching "standards" is impossible. I'm saying that
> if people actually adopted fixed standards that'd be dead in the water;
> they would not be able to control higher level variables.

Exactly right. Over-specialization (hardwiring of environmental responses to disturbances) leads to big trouble when the environment changes.

>
> I can't help thinking that I am "well-behaved" because I have pretty good
> control of the perceptual variables that I need and want to control. I
> have to believe that MOST of those who mis-behave are doing so, NOT because
> they havn't learned about "right" reference levels for certain perceptions
> but because they can't control much at all -- let alone what you might
> suggest as the profitable things to control.

Many people (myself included) have listened to "motivational tapes" when they _wanted_ to re-organize. This "standard setting" is voluntary.

Introduction: I am a electrical design engineer with a passion for things systemic. From what I perceive :-) perceptual control theory _could_ be the best candidate yet for a general systems view of human activity systems. This fight over influence vs. control will resolve, I hope, with the view that re-organization is critical to reference setting at all levels (individual through society).

As far as control theory in my own life (yes I have studied engineering control theory and designed servo-controllers), the main focus is my infant daughter. She has fairly severe eczema (atopic dermatitis). :-(When she starts scratching it, the skin gets damage, she bleeds, etc. This has led to us watching her much more closely than our first daughter for signs of upset (tired, bored, hungry, etc.). We are concerned that this is influencing her "socialization" or expectation of contact with others, but don't know what to do about it. On another plane, we are quite puzzled by what physiological cause there could be for this. We have attempted a dust free environment, she only gets breast milk, Mom has limited her diet to avoid possible allergic substances, etc. So all I want to know is: What control system is "out of whack" and how do we fix it? :-)

Curt McNamara mcnamara@mgi.com
Mgmt. Graphics, Inc.
1401 E. 79th St.
Mpls., MN 55425

Date: Tue May 12, 1992 12:24 pm PST
Subject: Re: grammar, control

[Avery Andrews (920511.2048)]

(Bill Powers 920502.1200)

Observations of grammatical errors and subsequent corrections are useful (people do this sort of thing, and I'm pretty sure that both Bruce Nevin and Penni Sibun know more about it than I do), but I don't think they are a substitute for ordinary grammatical observations, for the reason that you can't apply the Test, and systematically create disturbances for them to neutralize. All you see is them changing their mind about what to say and backtracking a bit, and there's really only an inference that control is what is involved, and you don't have much of an angle on what they're controlling for either (sounding cultured? like a real person? or what?)

the way to study this is to get lotsa data and look for patterns. you can't claim to know what's going on in any particular case, but out of the regularities you can develop a set of hypotheses about types of errors and conditions under which they occur.

Extended work on an individual's language is also problematic from the point of view of both practicality and ethics--to record really large

also, because language use just isn't a thing-in-isolation; it's a social phenomenon, so you're missing the same things the gb'ers miss if you focus on one person.

--penni

Date: Wed May 13, 1992 3:41 am PST
Subject: Re: Character education vs control education

[From Marcos Rodrigues]

Curt McNamara (13-05-92, 13:31) --

>the main
>focus is my infant daughter. She has fairly severe eczema (atopic
>dermatitis).
...
>So all I want to know is: What control
>system is "out of whack" and how do we fix it?

My son has the same problem. He is 11 now and the doctors have suggested that the problem will disappear as he grows older. I find your question quite appropriate because there seems to be no physical cause for it; we have tried all diet control recommended by the doctors and he has been tested for reactions to almost any common food or chemical. The only thing that his body reacted to was a drug used in some kinds of medicine which he's never taken and probably never will.

Some years ago, a doctor told me that eczema is associated with bright children with inquisitive minds. Certainly I don't believe it, but it fits to my son's character. "Bright" in his case means that I never have to repeat things, he somehow grasps immediately what I'm saying. Could be language, science, maths (for his age, of course) or any other general subject. He has an incredible memory, which I guess is the thing that helps his understanding. There is a sharp contrast between him and his 12 years

old brother.

Have you ever heard of such a thing? I don't mean specifically "bright" and "inquisitive", but any distinctive character feature associated with eczema?

Best regards, Marcos.

Date: Wed May 13, 1992 5:50 am PST
Subject: skin

[From: Bruce Nevin (Wed 920413 08:57:01)]

(Curt McNamara (13-05-92, 13:31)) --
(Marcos Rodrigues (Wed, 13 May 1992)) --

It may be worth considering that the skin is the largest organ in the body, that it has the most connectivity to the nervous system of any organ (other than parts of the nervous system itself, of course), and that it has the most connectivity to the environment of any organ.

What does it mean to be "bright"?

There is much in our children's world today to foster their intellectual development. This can lead to an imbalance of intellectual capacity over other forms of intelligence. I use the word intelligence advisedly, because we tend to identify intelligence and intellect, though there is abundant evidence to the contrary (e.g. Sternberg _The Triarchic Mind_). Of course, I am talking about perceptual control that focusses on manipulation of words and symbols rather than control of nonverbal perceptions that may correlate with the words and symbols--or may not.

Waldorf education influences my understanding of this. My girls are in a Waldorf school. The results are very persuasive to me.

I wonder if an emphasis on intelligent engagement with the control of nonverbal perceptions might help. Visual and plastic arts. Dance or gymnastics (acrobatics). Singing, playing a musical instrument, preferably one that emphasizes continuous control (violin) rather than digital (fretted instrument, keyboard). Not a remedial arts-and-craftsy thing, but a serious exploration of the world of nonverbal perception and perceptual control, apart from the manipulation of words and symbols that pervades our cultural universe.

Just a hunch.

Bruce bn@bbn.com
Date: Wed May 13, 1992 10:39 am PST
Subject: eczema

[From Rick Marken (920513 9:25)]

Curt McNamara (13-05-92, 13:31):

>the main

>focus is my infant daughter. She has fairly severe eczema (atopic
>dermatitis).
...
>So all I want to know is: What control
>system is "out of whack" and how do we fix it?

I'd have to know more about what eczema is (physiologically -- ie, what is happening to the skin) and how these physiological effects might effect variables that the skin deals with. (My daughter, now 14, had it, by the way. She still occasionally has a minor bout with it. Neither she, nor I, has any idea what "disturbances" might lead to it. But it has gotten far better with age; and my daughter is, indeed, quite "bright").

I think the medical people have to know more about how the skin is normally involved in various control processes before it can understand what might be wrong with the control systems that end up producing eczema. The skin does seem to be involved in the control of variables that are "psychological", though. I was amazed at how easily I could control the visual representation of my GSR by just imagining things in a "biofeedback" situation. Perhaps the skin is involved in the physiological processes that determine the "gain" of our control loops (this gain being experienced as emotions). Maybe, with eczema, the gain of these gain control loops is too high so that there is poor control of the skin variable involved in the respiration control that is involved in controlling the gain of the perceptual control loop gain, blah blah. This is all, of course, total bull****. What we really need is a model of skin control -- which will show symptoms of 'eczema' under certain circumstances. Until then, you can only deal with it the way medicine deals with most physiological malfunctions -- do whatever it is that ends up eliminating the symptoms (risking, of course, even worse side-effects sometimes).

Regards Rick

Date: Wed May 13, 1992 11:06 am PST
From: Dag Forssell / MCI ID: 474-2580
Subject: It's all perception

[From Dag Forssell (920513-1)]

I composed this yesterday morning, but was unable to steal the time to put it down. So here goes:

Wayne Hershberger (920511)

I've been thinking some more, and would like to propose the following:

You have consumed a few of those breakfasts, perhaps 20,000, and each time you grasped a cup you tested your understanding/perception of the BOSS REALITY. Each time you made contact with something (the cup) as expected, you confirmed your perception to be accurate.

Each reach, step, and movement becomes a test of the validity of your perception of BOSS REALITY. If you make a move every second in each 50,000 second day, you may have made one billion tests of your perceptual understanding of BOSS REALITY, all successful - except for a lot of

fumbles when you were a baby (before your perception machinery was fully developed) and an occasional stumble, when you failed to pay attention.

It is not surprising that you and philosophers before you have concluded that you have direct knowledge of BOSS REALITY, presented in living color and stereophonic sound.

When you stand up and push on one eye while covering the other, all of a sudden, BOSS REALITY does not cooperate. You fall because your perceptual construct of your boss realty is not good enough.

Popper suggests that you can NEVER prove a theory true. But you can prove it false. We just tested it and proved it false.

When - after a billion successes - you prove that all you have is a perception of that BOSS REALTY, you have a choice to make. You can dismiss the experiment that proves you wrong, fall back (intuitively) on statistics and say: One to a billion against does not count. I KNOW my BOSS REALITY and nobody is going to take it away from me.

Or you can say: It's all perception.

Your knowledge of the BOSS REALITY is limited by your perceptual capability. Our "hard" scientists tell us, based on their theories and measurements/perceptions, that their percepts of the BOSS REALITY includes infrared radiation, X-rays, photons, magnetism and a host of other "phenomena." All these are perceptions, too, which most of us incorporate without even studying the constructs in any detail.

Halfway up the HPCT perception ladder, a person may agree with Ed Ford that a husband and wife will have DIFFERENT concepts of "wife," but human nature being what it is, there will be an intuitive tendency to say: One to one against does not count. I KNOW my BOSS REALITY. I know what a wife is (sort of) and I will continue to use that information. After all, mine is the only percept I have access to.

When we come to a miracle, the natural tendency, given a LONG history of perceiving in a certain way, is to say: It may look like a billion to one against to you, but I KNOW my BOSS (REALITY) and nobody is going to take it away from me.

It goes against all intuition and apparent dependability of our basic senses to say: It's all perception, but it is the only conclusion I can defend, given my perceptual constructs.

I think that when a person recognizes and acknowledges this, the person is more free to reorganize (without internal conflict), respect his fellow man (complete with individual perceptual constructs) and promote a better social order with more degrees of freedom for all.

There IS a BOSS REALITY. Our challenge is to perceive it as effectively and accurately as we can, while recognizing that this is ALL we CAN do.

The BOSS REALITY does place constraints on our degrees of freedom.

I perceive that HPCT provides an effective (and as accurate as can be had at present) perception of the BOSS REALITY of our minds.

The question of how to control well with maximum degrees of freedom for all will quickly demand attention to issues of influence, "social control" if you will, the principles or "standards" we live by, and the quality of information in all corners of our Hierarchical Perceptual Control System.

It's all perception! AND Warm Regards!

Dag

Date: Wed May 13, 1992 11:15 am PST
Subject: Character education

[From Dag Forssell (920513-2)]

Rick Marken (920511 14:30)

>Your statement above implies that there are "reasonable" ways to set
>references (if standards mean references) for perceptual variables If
>this is what you meant then I must disagree. Reference settings depend
>on the goals of higher level systems AND disturbances to the variables
>controlled by those systems -- there is no one "reasonable" setting for
>references at ANY level of the hierarchy. Their can't be -- and
>imagining that this is so can lead to internal conflict, interpersonal
>conflict or self-destruction (I think that's what happened to Joan of
>Arc in your example -- lack of willingness to adjust a reference to
>control another variable; she imagined that there are absolute
>references. That's her choice, of course, but for, as for me, give me
>liberty or let me outta here).

I missed this in my first reply. That there is no one "reasonable" setting for anything at any level may be quite valid. Is that a reason to never discuss any suggested settings at the principle level? I believe a lot of people abstain because of the uncertainties. Your reading of Joan's quote differs from mine. I read her as saying that she was willing to die for HER references, not that they were ABSOLUTE. Self destructive? Sure! But in the long run, we all live and ultimately die for what we believe in - hopefully of old age. What do we believe in? HPCT!

Bruce Nevin (5/12/92 08:12)

>I think Dag is using the term "Standard" to refer to perceptions on
>(current) level 10, Principles. I saw this fly by in one of your
>long posts, Dag, but don't recall the date. Did I get it right?

Yes, thanks for listening. "I saw this fly by..." How do most netters operate? It is "electronic" mail. I download everything from MCI mail. I cannot be interactive at a university terminal. So I create a file and actually print everything. That gives me a complete reference archive from day one.

Bill Powers (920512.0930)

>Rick Marken (920511) --

>Control systems that think that there is just one "right"
>reference value for a perceptual variable are the control systems that
>really need to learn PCT!!!

The name is Dag Forssell. I am indeed trying to learn! That is why I am posting.

>>Dag Forssell (920511.2245) --
>
>>Greg Williams (920508) recently commented on the tendency of PCT
>>debaters to bury their heads in the sand, when it comes to "social
>>control."
>
>>Influence is a form of social control, for sure. Why be afraid of it?
>>Influence is for real, and it is important.

>Influence is not control unless you (a) insist that your influence have
>a particular effect on the other person, and if it does not, (b) apply
>whatever means is necessary to make sure it does have that effect.

I appreciate this help at sorting out definitions. I find it a difficult subject. But important to any practical use of PCT.

>Parents influence their children by (for example) advice, commands,
>example, demonstration, and story-telling. Children generally being
>eager for new experiences and not being very sure of themselves in
>situations beyond their capacities, they normally latch on to these
>influences and adopt from them whatever fits their growing
organizations.

This is essential. It seems worthy of a discussion in the specifics.

>If, however, they don't adopt some of them, or reject some of them, the
>parents may then resort to punishments and withholdings as a way of
>trying to make their influences have the desired (by the parent) effect.
>Then we get all the ills that result from concerted attempts to control
>other control systems. The children learn, in protecting themselves from
>direct external control, how to satisfy the parents' reference levels
>and thus remove the pressure. They learn to lie, dissemble, conceal,
>misrepresent, pretend, and otherwise give the impression of compliance
>while internally isolating themselves from their parents. They become,
>in short, alienated from the adult world.

This is why this is such a sensitive issue. Most of us can identify with this.

>Of course a lot of the children simply buy into the system and save
>themselves all that trouble.

Is this a value judgement on your part. Is it all a (bad?) "system?"

>Social influence is not social control. But it's hard to learn how to
>influence (act on) other people while accepting completely that they
>will not be influenced (be changed) if that is their choice. When we
>exert influences on other people, hoping for some change in their

>behavior that's to our own liking, it often happens that there's no
>visible result. What do we do then? If we just try harder, we're falling
>into controlling another person, or trying to. If we give up, we haven't
>achieved what we want. It's hard to find the middle ground, where we
>give it a good try but on detecting serious resistance give a higher
>priority to respecting the other's will as much as our own.

This describes the challenge of using PCT for good.

Violence and social control is bad. Influence may be OK, but we don't much like it either, because it smacks of control. The lines of demarkation get fuzzy.

If a wife is unable to influence her husband, eventually she may exercise social control in the form of divorce.

If an employer is unable to influence an employee to be productive in the line of business the company is in, then he will have to influence the employee to seek other employment. Some will call it (mistakenly?) social control or even violence? Personally, I have been laid off and quit. It is a natural consequence of my own and my employer's requirement for degrees of freedom. But there sure is a lot of unnecessary waste, violence and social control in business. Neither employers or employees are effective in their control. You find conflict everyplace you look.

It seems to me that the absence of appropriate influence leads a person to fail to develop the good information content required for good, effective, satisfying control. I continue to be interested in influence as a constructive activity. It is difficult to deal with.

Since much of this "flies by" let me repeat a portion of my first post on standards Dag (910504). The Principles I wanted to draw attention to are at the end in CAPS.

Here I will insert an excerpt from THE CASE FOR CHARACTER EDUCATION by Frank G. Goble and B. David Brooks. I shall transcribe two pages from

Chapter 7: WHOSE VALUES SHOULD BE TAUGHT?

We sow a thought and reap an act;
We sow an act and reap a habit;
We sow a habit and reap a character;
We sow a character and reap a destiny.

William Makepeace Thackeray

Whose values, people frequently ask, do you propose to teach? Those who ask this question, although they may not realize it, have been influenced by ethical relativism - the idea that there are no enduring ethical values.

When the subject to be taught is chemistry, physics, or astronomy, no one asks whose chemistry? Whose physics? Whose astronomy? It is assumed that the teacher will simply present the available information to the best of his or her ability. Everyone assumes that there is an objective reality about these subjects, in spite of the fact that our understanding of the physical sciences is neither complete nor exact.

The question, whose ethics, implies that there is no objective

reality about ethics and this is exactly what the ethical relativists claim.

"Such a position of normalness," writes Professor Philip H. Phenix,

> ....is a denial that there are really any standards of right or wrong, of better or worse, because the whole human endeavor appears to be meaningless and without purpose... If life is essentially meaningless, there is no point in trying to promote or to improve it. An anomic theory of values is fatal to education, as it is to any sustained cultural pursuit. Unfortunately, it is a theory all too widely held, either explicitly or tacitly, and it should be recognized as an enemy of human morale and of educational effectiveness.

The influence of this relativistic, value-free point of view is illustrated by this statement of Dr. Lewis Mayhew in an address given when he became president of the Association of Higher Education: "Colleges are not churches, clinics or even parents. Whether or not a student burns a draft card, participates in a civil rights march, engages in premarital sexual activity, becomes pregnant, attends church, sleeps all day or drinks all night, is not really the concern of an educational institution."

The problem with this point of view is that it is not realistic and leads to increasing crime and violence and other costly manifestations of social disintegration. There ARE basic ethical principles that are necessary to social progress, and these principles must be identified and taught.

American Viewpoint, whose Good American Program was described in chapter 4, based its program on an empirical code of ethics. The code was developed by writing to hundreds of outstanding citizens and asking their opinions. From this was developed a list of values which had been "hammered out in the anvil of practical experience." The Good American list includes such concepts as conservation, courage, personal health, honesty, initiative, perseverance, reliability, self-mastery, cooperation, courtesy, fairness, respect, tolerance, duty, independence, patriotism, responsibility and understanding.

The American Institute for Character Education, which developed the Character Education Curriculum also described in detail in Chapter 4, based its program on a worldwide study of value systems. This study identified fifteen basic values shared by all major cultures and world religions. These values are COURAGE, CONVICTION, GENEROSITY, KINDNESS, HELPFULNESS, HONESTY, HONOR, JUSTICE, TOLERANCE, THE SOUND USE OF TIME AND TALENTS, FREEDOM OF CHOICE, GOOD CITIZENSHIP, THE RIGHT TO BE AN INDIVIDUAL, AND THE RIGHT OF EQUAL OPPORTUNITY.

This code of personal values, now taught in thousands of classrooms, has not proved to be controversial.

>What really counts is our understanding of human nature. If we
>understand that all people are basically as autonomous as we are, then
>we wouldn't want to encourage a system in which autonomy is ignored or
>overridden by force as a matter of policy (the present most popular
>system). With that understanding, we try to deal with others in a way
>that encourages them to understand things the same way, and to realize
>that if they want to continue being autonomous, they have to support a
>system in which autonomy is generally accepted as a fact. Once you see
>that basic concept, you understand the problem we're trying to solve in
>our social interactions. There's always a conflict between what we want

>other people to do and what they want to do. If we begin by respecting
>the will of others as much as our own, there are certain kinds of
>resolutions of the conflict we will avoid using as long as possible. We
>will spend more time trying to find clever ways to satisfy all of us,
>and less time plotting how to get our own way regardless. It seems to
>me that that would be a pretty nice world to live in. I'd like to
>persuade others that it's worth a try. But of course I can't control
>them into doing so.

This is what I have bought into.

To make this understanding better known and accepted is the challenge. We must show how to apply HPCT for the satisfaction of all. Information offered must tie into what people already (think they) know. It must offer something of immediate interest, address some dissatisfaction or error signal people have, or it is of no interest.

I offer the above list of Principles/Values as something that comes close (at least as a start) to "finding clever ways to satisfy all of us."

If it is of any interest, then there is a lot of material available to review.

Regards to all! Dag

P.S. I feel like the proud father of my own thread. But I must watch the time I spend in this direction. Must promote PCT to the outside world, so I can put beans on the table.

Date: Wed May 13, 1992 11:35 am PST
Subject: Autonomy according to PCT

From Greg Williams (920513)

Again, I want to suggest caution in claiming that PCT supports an ideology of individual autonomy/self-determination.

PCT hypothesizes that, at any particular time, an individual's control structure (in particular, his or her reference levels) determines his or her behavior (perhaps in a probabilistic way; whether there is strict or "absolute" determinism is a side issue). And his or her reference levels, together with the environmental disturbances existing at the time, determine his or her behavior/actions. Others cannot "reach in" to alter those reference signals. One's control structure is subject to reorganization (which is, by hypothesis, stochastic).

From the above, which I take to be unheretical, I derive the following.

1. It appears that each individual has no "free will" from moment-to-moment, since all he or she can do is allow the (possibly probabilistic) dynamics of his or her control structure and disturbances to play out as behavior/actions. Nevertheless, there is one sense in which individuals are "responsible" for their acts: after all, THEIR OWN control structures (conjoined with disturbances) result in their own behaviors/actions.

2. It appears that whether or not an individual's control structure begins to

reorganize at any time is determined (same probability caveat) conjointly by the current structure of his or her control structure and current disturbances. And when a reorganization episode ends is determined (same probability caveats) by the (altered) control structure and the disturbances. (Reorganization stops when, in some sense, a "solution" has been found to a "problem" faced unsuccessfully by the previously existing control structure, which is what started the reorganization episode in the first place.)

3. From 1 and 2, I conclude that an individual's current behavior/actions are determined (same probability caveat) by the individual's history, including events BOTH within and outside the individual.

It will not do to emphasize that "nobody else can make you do what you don't want to do" (which, unpacked, means "nobody else can alter your current reference levels to make them conform to his or her desires") -- after all, you yourself cannot alter your current reference signals, either -- and yet neglect to mention that BOTH you AND others (as providers of environmental disturbances) can play roles in determining (same probability caveats) your FUTURE control structure.

As far as PCT has something to say about it, it seems that we are neither pawns of our environments nor masters of our destinies, yet in the present moment, we are "slaves" of our current control structures conjoined with current environmental disturbances.

Greg

P.S. Gary Cziko has suggested that the next CLOSED LOOP be devoted (at least in part) to statistics and PCT. Fine by me... any other suggestions?

Date: Wed May 13, 1992 11:51 am PST
Subject: altering one's own reference

Greg makes the interesting aside

>you yourself cannot alter your current reference signals, either

Surely there is one sense in which this happens all the time--one ECS changes the reference input to others within the same person.

Greg, what is the referent of "you yourself" in this statement?

"Hi, pull up an eel and sit down," said he,
holding out a pond full.

Bruce
bn@bbn.com

Date: Wed May 13, 1992 12:39 pm PST
Subject: concurrent control in language

[From: Bruce Nevin (Wed 920413 09:17:47)]

(Avery Andrews (Fri, 1 May 1992)) --

As a memory refresher (it's been a while), you said:

> John gave the student article that
>
>gets no meaning

I replied:

>It is specious to say [this] example gets no meaning because to say
>so presupposes that it would occur in native English, and it would not.

And your rejoinder was:

>The hypothesis I'm proposing is that it does not occur in native English
>*because* it gets no meaning by the normal operations of the perceptual
>system described by the grammar. This could be false, but it isn't specious.

This exemplifies the sort of problem that Bill and I also keep running into in our discussions of language, because of the complexity of the subject. One of us takes one perspective and one another and we talk past each other. Both perspectives are valid. What is needed is a perspective that subsumes both, or at least one that makes it both easy to switch from one perspective to another, obvious when one has done so, and obvious which one one has adopted at the moment.

Avery, your perspective here is on the perceptual control of language within an individual living control system. What is going on inside the black box? My perspective here is on the social context for acquiring language. If it's not in the language as social artifact in the child's environment--particularly those who engage the child one-on-one in the necessary language acquisition support systems (LASS)--then the child never has the occasion to control for it and it never gets into the black box. And that is why it subsequently has no meaning for one who has acquired the language. However, the example given above works just fine in Modern Greek:

```
O yanis dhini ton fititi  to  vivlio ekino.
- John  gave  the student the book   that.
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(I don't recall the word for "article" or "paper.") In English, this word order "gets no meaning by the normal operations of the perceptual system" only because it does not occur in the usage of other people in the speech community, in particular did not occur in the usage current in the language learner's LASS. It is easy to imagine a situation where a community of native speakers of Greek learned English vocabulary but retained Greek syntax, and the resulting interlanguage was learned by children in that community. This seems to be precisely the origin of interlanguages (trade languages, koine's) like Swahili. What is needed is a system of social arrangements for cooperative action in which the interlanguage is more advantageous than either of the original languages. As I recall, a trade language of the Manding Empire in West Africa was the basis for Swahili, Black English, Black Portuguese, etc. Keep the familiar, albeit simplified, syntax and substitute in vocabulary known to the persons with whom you are trading. As you know, it has been argued that the history of English sounds a lot like this too.

Now I still have to say there is something specious about your statement, as I quoted it. To say that the unexpected word order, placing "that" at the end, "gets no meaning by the normal operations of the perceptual system described by the grammar" begs the question "does the grammar in fact describe the normal operations of the perceptual system for control of language?" It begs this question because an answer to the question is subordinated as a modifier of "perceptual system" when you say "the perceptual system [which is] described by the grammar". This is no longer a question, it is a presupposition; it presupposes an answer to the question. Begging the question is specious. Placing the question in the foreground is not. Then a particular answer to the question may be false or not, as you say.

(Bill Powers (920501.1430)) --

>study the actual process of speech production and error correction while it
>is going on. This will directly show what is considered an error and what
>corrected form is considered OK by the person, no matter how the person
>might describe or misdescribe the rules actually in effect. If you then ask
>why a person corrected certain errors, the answer in many cases (with non-
>linguists) will be that it just sounded wrong, and sounded better the other
>way. That's probably the kind of answer you want, because it shows that the
>correction was made by the actual machinery and isn't just a guess at a
>verbal generalization. I suppose the subjects you want for such experiments
>would know nothing about grammar or syntax, but would still speak well.

Vicki Fromkin has done a lot of work with slips of the tongue. I posted comments on an article by Ewing and others on an experiment inducing slips of the tongue to ascertain relationships among different levels of control. Such work is considered too "data bound" by many in the field. Too bad for them.

(Rick Marken (920501 17:00))

>>(BTW, Rick, this is the sort of place to look for control, not in the
>>enforcement of rules learned in school.)
>
>Same kind of problem I had with Avery -- what do you mean? When

Sorry, I don't mean to deny that control is involved in responding to overt peer pressure. Control in the face of disturbance by real or imagined social influence of others is certainly control. But this is control for one extant variant of language usage over another. Rules like those that say one choice of "that" vs. "which" is "proper" and another is "improper" in a given kind of syntactic construction are based on the relative prestige of social groups whose members learned one way or the other at their mother's (or nanny's) knee. My purpose was not to deny that control is involved here. My purpose rather was to emphasize that control is involved at more basic levels in constructing or interpreting a relative clause, whether begun with "which" or "that". Genie could not do the latter. And of course the syntax of relative clauses, and of modifiers in general, just scratches the surface.

It is important to realize that the mechanics of putting words together in a construction like a relative clause (syntax) is distinct from the semantics of which pairings of words in a given construction make sense. Genie could put words together on a semantic basis in simple

subject-predicate constructions, but not in the more complex socially institutionalized syntactic constructions that support subordination and coordination of assertions.

Penni's work, so far as I have read, seems to concern itself with higher-level discourse structure. The sentence-level syntactic constructions are rudimentary. Her results converge with Harris's in _The Form of Information in Science_ and _A Theory of Language and Information_. In that work, it was shown that information structures found in discourses of a sublanguage of a science correlated with objects and relations in the domain of the science, and that changes in the information structures correlated directly with changes in the science (changes in immunologists' perception and understanding of the domain of immunology). A (r)evolutionary development in the science coincided in time with a change in word classes and permitted word class sequences in the sublanguage grammar. It would be interesting to see if the system at Xerox PARC could accomodate a domain and discourse structures of that complexity. Like Penni's work, Harris's concentrates on domain-driven structure, leaving structures of logical argumentation (which appear to be superordinate over the former) for future investigation. Unlike Penni's work, Harris's includes a comprehensive account of sentence syntax.

(Bill Powers (920502.1200)) --
(Avery Andrews (920501)) --

>>control: but how to introduce appropriate disturbances into sentence
>>generation?
>
>examples of things that are somewhat tricky to say or describe.

Most of the examples you propose, Bill, concern control for semantic relations, that is, the correlation of words with nonverbal perceptions. Things that are tricky to say *because* they are tricky to describe. There are also things that are tricky to say because the conventions for things like reducing interrupting comments to modifiers are tricky to manage.

> "The man with the green car's dog has fleas."

This is more of a problem written than in speech. "With the green car" gets lowered intonation in speech, and "dog" pops back up to the level of amplitude and pitch that it would have had if "with the green car" had never interrupted between "the man" and "'s." This is the more obvious, the longer the interruption. Within that lowered intonation, something that differentiates can receive contrastive stress: "The man with the GREEN car's dog has fleas." (As contrasted with the man with the WHITE car's dog.) You can get "The GREEN-car man's dog has fleas" with the contrastive stress, but hardly without.

Just how would you say "the Queen of England's unexpected visitor"? It is a matter of what is called good prose style to avoid such pitfalls, of which any language sets its users many. That famous shibboleth, the dangling participle, provides many a hilarious example of pernicious ambiguity. Hopefully, people will eventually get tired of complaining about certain sentence modifiers in sentence-initial position, because of ambiguity with their use as verb modifiers.

("Hopefully" does not modify "get tired of complaining" but rather a zeroed performative "I say" on the sentence as a whole.) Clearly, what applies to one sentence modifier without complaint should apply to all. (Neither does "clearly" modify "applies," but no one complains.)

But control for unforeseen (or picked-on) ambiguity is still at a semantic level. You have two syntactic constructions that come out as the same sequence of words, but the constructions correlate with different nonverbal perceptions, one of which is inappropriate, perhaps even humorous. Underneath that, what constitutes a syntactic construction? What perceptual control is involved in controlling the mechanical mannerisms of the reduction system, quite apart from the dependencies of operators and arguments? Why can't you say "This is so much funner than doing my income tax"? (Though my kids' generation use "funner" quite freely--the language seems to be changing there.)

Consider things like:

> The man which the dog with fleas--I mean, the man with the dog with fleas, the dog which has fleas--still thought it was his neighbor's fault.

Any place where operator grammar sees a reduction, semantic control applies transparently in the correlation of unreduced words to nonverbal perceptions, but less obviously to the reduced forms of words and phrases.

```
the dog--said dog has fleas--got lost
the dog which has fleas got lost
the dog with fleas got lost
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Syntactic control is involved in the correlation of reduced forms to more explicit forms. The difference may be highlighted by an ambiguity that has perhaps escaped notice:

```
the dog--said dog has fleas    |        the dog--said dog is with fleas
the dog which has fleas        |        the dog which is with fleas
                        the dog with fleas
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I imagine a Bizarro cartoon captioned "dog with fleas." There they sit, the dog and a small crowd of fleas, side by side under their respective beach umbrellas.

At a lower level than semantic control and syntactic control is control of phonology and phonetics. Tips of the slung, I mean slips of the tongue.

All of these are going on at once. Control for dialect membership (one's own, one's companions' different dialect, a model of the dialect associated with a more prestigious social class, a parody of a socially inferior (or superior) dialect, and so on); control for meaning; control for syntactic construction (within dialect); control for pronunciation (within dialect). As Avery says, it is not always easy to tell which kind of control has been disturbed when someone says "I mean . . ." .

Studying a single idiolect in depth (speech of a single language user) is certainly an option. There are problems. The processes of

observation can disturb and influence what you are observing. This is obviously so when you use yourself as an informant (though it has not been so obvious to the recent generations who have so abundantly demonstrated the truth of this, linguists trained to do just this and little more). You cannot make much sense of control for conformity to or distinction from socially marked ways of speaking without considering the latter. You cannot make sense of variation in reference perceptions within the same speaker from one occasion to another, and change in them over time, except with reference to language as a social artifact.

Speech is different from the control of a pointing finger in a way that I think is important for all the social sciences. In the usual case, behavioral outputs are incidental byproducts of control. They are not themselves controlled. Some other perception is controlled, and the behavioral outputs are variable means, whatever it takes in a disturbance-prone environment to make the controlled perception match a reference perception in memory or imagination. With speech, however (and with any conventionalized behavior) the form of the behavioral outputs is itself subject to control, concurrently with the perceptions the control of which the behavioral outputs are the variable means.

This is possible whenever there is "free" variability that is not constrained by the contingencies of control--more than one form of behavioral outputs can accomplish the same controlled perception. Then choice among alternatives (or in the range of free variability) itself is exploited as an aspect of self image, or social standing, or relationship to others involved in the transaction, etc.

Even pointing with the finger can have a personal style, or a manner associated with a particular community.

To accomplish this, the behavioral outputs involved in effecting control of one perception must themselves be monitored and controlled with respect to particular choices among their range of free variability.

I hope that this is clear, and that it contributes to a useful discussion, especially with the sociologists and social psychologists among us.

Bruce
bn@bbn.com

Date: Wed May 13, 1992 1:16 pm PST
Subject: standards

from Ed Ford (920513.12:12)

Concerning standards:

We all of us have a variety of perceptions of how things ought to be. This is found at our systems concepts level. In order to control for this, we set for ourselves certain principles or standards that reflect those concepts and will become the basis upon which we make our decisions.

It seems to me we are setting standards for ourselves and in

cooperation with others (such as in a home, a community, or within an organization) all the time. We are also insisting others live by the community standards we set or else we try to control those who refuse to voluntarily follow our standards. We teach our children cooperative standard setting with others as the most sensible way to live in harmony. That is why we have communities filled with all kinds of standards, called laws. We as communities and families have certain values and we set certain standards within the home or community that reflect those shared and agreed to values. We also teach our children how to set their own standards, and, just as important, we ask them to explore the down-the-road consequences of the standards they've set. This I've called teaching responsibility. I define responsibility as the willingness and ability of people to follow standards and rules and ultimately to set their own, without infringing on the rights of others.

Recently, David talked about doing group therapy with juveniles. I've done this quite extensively in various types of settings (mostly schools and correctional facilities). The juveniles are there because of their refusal to obey the standards of the community in which they live and also for having violated the rights of others. I think the purpose of group therapy is to teach those skills which lead to satisfying lives, which includes learning the skills for making and maintaining satisfying relationships as well as the skills for become a self-sufficient, self-supporting, responsible human being.

The real issue for me is what is the most efficient and effective way to teach these skills at home or in various social settings. Since I am really only an influence (see Powers (920512)), I have found the best way for me to work with others is to first find out if the living control systems with whom I am dealing a) want to deal with me and b) have reference signals that have to do with improving their lives.

I have a close friend who has a 17 1/2 year old son who lives at home, doesn't work, gets up at 6 p.m. and goes out till 5 a.m., is involved in stealing, etc. My friend is running a real conflict, where one reference signal is pulling toward throwing the kid out of the house (the children has gotten violent at times) but there are two other reference signals, one that wants to avoid physical and possible violent confrontation and another signal demonstrating a great deal of love for this child. He also has several other reference signals which include harmony with his wife (who is all for throwing the kid out of the house) and another that involves maintaining the standards for harmonious living within this home. That child is not willing to change his life style and is unwilling to deal with either his father or myself. It's very nice to control for what you want but when it runs against the prevailing standards of where you live or work, then you have to live with the eventual consequences of your decisions.

The way I teach others how to use their control systems is through asking questions. I find little difficulty with the people I work with (including corrections) to get people to move up one or more levels. In fact, as soon as I get my clients to list their areas of importance to me (systems concepts level) and have them prioritize those areas in terms of importance, that's when I find therapy really gets going. As they begin to identify the areas of conflict (conflict between two reference signals at the highest order), evaluate what area they want

to work on or where they would most likely succeed, set the kind of standards for the area where if they were able to accomplish their goals, would them bring satisfaction, that's when they seem to find some relief. When they say they feel better, what they're really saying is "I think I can now figure out a way for making things better in my life." Obviously the real proof is when they begin to succeed.

With regard to standards within social organizations: Most if not all organizations and communities have set standards, and you have to be willing to live with those standards or you leave (or don't join). There are many belief systems that say that if you want the perceived benefits of being a part of us, and you want identify yourself with us, then you've got to accept our beliefs, and abide by the standards we've set that reflect those beliefs. And I think that's fair. In my life, I've joined several organizations whose standards were such that I left that organization. Others I have remained with, the CSG being one.

Sometimes we set standards that just involve ourselves. I'm a very strict vegetarian and yet I've never tried to impose these standards on my wife or children. I've certain standards in other areas of my life. I figure the way I live my life is the best influence I can provide to others. One thing for sure, I've learned not to try to impose my beliefs and subsequent standards on others.

It's all perceptions, but we're responsible for our own.

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

Date: Wed May 13, 1992 3:57 pm PST
Subject: The "self"

From Greg Williams (920513 - 2)

Bruce Nevin asks about the referent of "you" in my previous post of today. It's that mythical transcendental mystical self, of course. It's not my construct, and it certainly isn't PCT's!

Sorry but I don't like see-food.

Greg

Date: Wed May 13, 1992 5:22 pm PST

Once again, I have no idea what has been going on on the net, but I found a journal today which some of you may or may not care to know about. I don't know much about prestige or whatever else might relate to opinions about journals, but I do know that _New Ideas in Psychology_ has a recurring theme, self-determination/volition/will etc among other interesting topics.
I found it via references in Howard's article in Volitional Action.

So maybe everyone is familiar with it or no one cares but if you're interested in getting some ideas out there to be discussed (much like is done here) with people sympathetic to volitional studies, ya might want to check it out.

The first issues in 1984 have alot on voltion and I think 1990 ones do also. Other issues have it scattered around.

Mark

Educational Psychology 210 USmail: 405 South 6th St. #4

College of Education Champaign, IL 61820
Univ of Illinois at Urbana-Champaign
phone: (home) 351-8257 e-mail: (Internet) m-olson@uiuc.edu
(office) 244-8080 (Bitnet) FREE0850@uiucvmd

Date: Wed May 13, 1992 7:15 pm PST
Subject: standards and models

[From Rick Marken (920513 15:30)]

I have my references for 'standards' just like everyone else. If asked I would say I like people (including myself) with high levels for what I perceive as honesty, integrity, responsibility, and so on. It's difficult to talk about 'standards' without having an idea of what constitute 'good ones'. So I suggest that we move this discussion from a discussion of standards (just one type of perceptual variable) to the model that supposedly informs our understanding of human nature. I might prefer particular system concepts, standards (principles), programs, etc -- ie, I might have a collection of references which can, over time and variations, be perceived as a particular "ideology". That's just me -- my system that grew up over the last 46 years. I'm not interested in pushing my ideology -- just one component of that ideology -- a model of human nature called HPCT.

So, what I believe in (as far as this audience is concerned) is a spreadsheet! The 3 level spreadsheet hierarchy that is described in one of the papers in my book (be sure to order a copy from Greg!!) captures what I believe is the basic functional organization of a human being. Some things are missing--like the reorganization system. But this model gives a good picture of my image of an organized (grown up) purposeful adult. One nice thing about this model is that it is ALL NUMBERS. The numbers that are perceptions are functions of other numbers -- the function defines what is perceived. Numbers are nice because people don't care that much about them. The perception numbers at level 2 of the model, for example, could be representations of the degree to which some standard (like "honesty") is being perceived (in the spreadsheet level two perceptions are actually functions of linear combinations of intensity perceptions). The spreadsheet has four control systems at three levels; the reference for the highest level systems are fixed (they are numerical constants) but they could be changed randomly by a reorganizing system. The model acts to keep all its perceptions matching all its references. So the level 3 systems adjust the references to ALL level 2 systems (changing the reference NUMBERS) so that the level 3 perceptions are maintained at the reference levels. The spreadsheet does this even when you change the enviromental variables (also numbers) on which the controlled perceptions are based -- that is, it controls a hierarchy of perceptions in the context of changing environmental disturbances and in the context of the changing control actions of all the individual control systems.

If you give names to the numbers at each level of the hierarchy then things can get personal. For example, if you think of system 1, level 2 as controlling a perceptual "standard" called honesty (as one means of controlling the higher order perceptions, which might be called system concepts) then you have to say that the system is varying its reference for honesty to control whatever perceptions are being controlled by the higher order variable. This is why I say that I don't think that there can be fixed references for ANY perception -- it's not because I'm pushing moral relativism or personal autonomy or libertarianism. The only thing I am pushing is the PCT MODEL (and I can envision it best and see it working best in the spreadsheet implementation -- because I know what the numbers mean; I know this is not the easiest way for many people to visualize the model, but it does have that one nice feature -- it doesn't hit any emotional buttons).

So I suggest that when we discuss these big philosophical issues, we try (to the extent we can) to relate them to what we actually know -- the HCPT model. HPCT is a real, working model and many aspects of it have been tested and passed with flying colors. There are many aspects of the model that we don't understand (like how it could perceive something like "honesty") any many things that will surely need to be added or changed as a result of research (Greg's suggestion that higher order outputs may influence lower level parameters besides references inputs, for example).

I think if we talk about functional organization more, and specific perceptual variables and their references less, we might get a better idea of what HPCT is about. The words (and the fact that people are themselves control systems with their own references for standards and whatever) can really get in the way. HPCT is HPCT -- it's not liberalism, radicalism, libertarianism, judaism, mormonism, monotheism, etc etc. It's a functional model that explains (purportedly) why people behave according to any of these principles. The model is a bunch of numbers that are functionally related to other numbers. It doesn't say what it is "best" for those numbers to represent.

If there is any "value system" implied by the HPCT model it is just that the model should WORK -- ie. it should be able to keep ALL its perceptual numbers equal to all it's reference numbers. Anything that prevents the model from doing that is something that should be "fixed".

One last point:

Dag Forssell (920513-2) says:

> The Principles I wanted to draw attention to
>are at the end in CAPS.

> These values are COURAGE, CONVICTION, GENEROSITY, KINDNESS,
>HELPFULNESS, HONESTY, HONOR, JUSTICE, TOLERANCE, THE SOUND USE OF TIME
>AND TALENTS, FREEDOM OF CHOICE, GOOD CITIZENSHIP, THE RIGHT TO BE AN
>INDIVIDUAL, AND THE RIGHT OF EQUAL OPPORTUNITY.

>I offer the above list of Principles/Values as something that comes close
>(at least as a start) to "finding clever ways to satisfy all of us."

The words in caps seem to describe perceptual VARIABLES -- one can perceive

degrees of courage, conviction, generosity, etc. How do I derive from the HPCT model what value is the right value to set for each of these variables? I just don't think PCT has anything to say about this other than "people can control perceptual variables lke courage, conviction, etc.". See the problem with talking about this stuff. People CARE about these things. I CARE about these things. But you are claiming that certain levels of these variables
are RIGHT. If I say "NO" it looks like I'm in favor of lying, cheating and being a chickenshit. When you say that there might be "RIGHT" levels for certain perceptual variables (the "standards") , what I hear is the claim that "I can set one of those level 2 reference numbers to a CONSTANT in the spreadsheet hierarchy and everything will work even better -- the only thing that I have to do is find the RIGHT number". Well, I know that that is not true -- quantitatively.: it's not true of numbers in a control hierarchy. If you believe that those numbers are a representation of perceptual variables and that things like honesty are perceptual variables, then I leave the conclusion to you.

But I am open to any model based (and research confirmed) evidence that there are any RIGHT constant values for variables in the HPCT model. I mean, HPCT may be my ideology, but it is open to test (that is one NICE thing about numbers).

One more point (and violating my own injunction to not talk about standards): You quote favorably from a book about character education. Here is one statement:

> The question, whose ethics, implies that there is no objective
>reality about ethics and this is exactly what the ethical relativists
>claim.

Do you say that there IS an objective reality about ethics? And just as I was about to believe that it was all perception. I take it the authors have access to that objective reality. Inquiring minds (like mine, locked in their perceptual experience) want to know what it is.

> "Such a position of normalness," writes Professor Philip H. Phenix,
> [must be "normlessness" no? -- rm]
> ....is a denial that there are really any standards of right or
> wrong, of better or worse, because the whole human endeavor appears
> to be meaningless and without purpose...

Just speaking from the point of view of the PCT model, however, there are indeed standards (principles) in this model; they are perceptual variables and there are references for the value of these variables. To the extent that principles deviate from these references they are 'wrong'. Keeping principle perceptions at their references is one of the MANY purposes of the model. If this model plunges me into the abyss of meaninglessness -- then off I goooooo o o o o.

Best regards Rick

Date: Thu May 14, 1992 9:49 am PST
Subject: controlling for participation

[From: Bruce Nevin (Thu 920414 07:44:16)]

(Greg Williams (920513, 920513-2)) --

I didn't just mean to tweak your beak about the seat of volition, Greg, I'm asking quite seriously for some elaboration on your remark that we can't adjust our own reference signals, just as no external agent can adjust them. What follows spins off my thoughts. I still would like to know yours.

It sure appears as though there is a driver in the driver's seat, doesn't it? Awareness identifies with some set of purposes, and among them is the presumption of having initiated those purposes. Is the driver a volunteer or a draftee? You argue draftee (determinism). I would leave that question open still.

I agree with you that Control Theory does not entail a liberal ideology. To foresee a great amelioration of the human condition with the awakening out of S-R slumber into the New Age of Control Theory is good motivation, but naive. Mr. Nobel thought his invention of dynamite would do the trick. However, I come to that view on a different basis.

Failure of control theory to guarantee autonomy is due to the propensity of living control systems to bring free variability in their behavior outputs under control for conformity to one another for social purposes. People (mammals) bind their freedom to social purposes.

Let me try a quasi-postulational approach.

Manner: A particular conventionalized choice among free or available variants.

Variants: A range of variation in behavioral outputs.

Free: Variants are free if the differences between them make no difference (disburbance) for other ECSs.

Available: Variants are available if (a) gain on the control of a manner exceeds gain on some other ECS to which it makes a difference, and (b) said other ECS can control the net disturbance by changing reference signals to other ECSs with lower gain.

Living control systems control free variation for conformity to the manner of co-members of a social group. By doing so, they assert membership in that group.

Control for conformity to the manner of co-participants in real time may conflict with control for conformity to the remembered or imagined manner of (intended) co-members of another group.

Disparity of manner that goes unnoticed is socially unmarked. Disparity of manner that occasions conflict (often in both parties) is socially marked.

A shibboleth is a manner that control systems take as a sign of membership in a social group, such that disparity with it is always socially marked.

Children who are taught shibboleths and other socially marked manners for their social advantage, rather than learning them as co-members of a social group that controls for them, may come to associate them with emotional valuations of good (praiseworthy) and bad (punishable).

An assymetric social transaction is one in which one-down participants control for conformity to the expectations of one-up participants.

People who experience a one-down role in an assymetric social transaction typically seek ways to play a one-up role in the same kind of transaction with other players.

Children experience themselves as one-down a lot. Children play one-up roles with other children a lot. Children who learn shibboleths and socially marked manners by having them enforced in a one-down way turn about and enforce them on other children in a one-up way.

And so it goes.

Control theory does provide a framework in which people may understand and cultivate non-hierarchical social arrangements. It does not guarantee that they will do so. That hoped-for evolution into a world of fraternal/sororal amicability depends upon resolution of emotional craving for redress, in a vast number of individual living control systems, out of a long heritage of assymetrical social arrangments. The abused child who grows to an adult who abuses children marks only the tip, emerging over the horizon, of an iceberg whose distant shores have yet to be touched by any boatload of social benefactors.

Control theory has very little to say, yet, about the basis of attachment (craving, addictive demand) or emotion.

Even if we do all agree on non-hierarchical social arrangements, or when we admit only leadership based on personal attributes needed for a shared purpose presently at hand (the anarchist program), people will still control free variation for conformity to the manner of co-members of a social group. They will still bring free variability in their behavioral outputs under control for conformity to one another for social purposes. Like mammals in general, and perhaps lower organisms too, people will still bind their freedom to social purposes.

Withal, it is important I think to distinguish autonomy from independence. Independence is freedom from external constraint. Autonomy is control within one's own domain. But one's domain is defined by participation in social groups and social arrangements. They incur a cost in independence. If the cost of particular social arrangements is not outweighed by a gain in autonomy, one seeks ways to change them. One may exchange one set for another. Attempts to abandon them entirely don't appear to work. Perhaps that is a biased view from within my social arrangements, but if anyone knows better from experience they (by definition) aren't telling us. Conversely, if we exclude from Control Theory an account of control for social conformity, we are not talking about people.

Bruce
bn@bbn.com

Date: Thu May 14, 1992 11:26 am PST
Subject: Blindmen, K.o and K.f

[from Gary Cziko 920513]

Rick Marken (920508 8:30) said:

>Now, if k.o is VERY large (say, 100,000) and k.f is relatively small
>(say, 100) then the ratio . . .

Now that I understand the math, I'm wondering why k.o has to be so large. From what I remember of playing with Powers's Demo2, it seems that the model with a k.o. of only 30 or so with a k.f. of 1 acted pretty much like I did in the tracking task. I suspect, however, that the units the computer demo is using is not the same units that I am using as a person making muscles contract based on miniscule neural currents starting in my retina so that k.o. is actually much larger than that of the Demo2 model. Am I on the right track here?--Gary

Date: Thu May 14, 1992 1:49 pm PST
Subject: flying by

I said "I saw that fly by." Dag asked what I meant, supposing it had to do with my way of reading email.

I don't have time to read everything as it comes in. I try to scan on my terminal. Then I put it in files and print it. I was reading these on the train (at the expense of my linguistics homework). Now I am riding with a friend who drives in, and it is harder. As it happens, the quick scan on line was all I had seen of that particular message. Hence, I had seen it fly by.

Martin's hypercard stacks sounded real nice. But I don't have a Mac.

Bruce

Date: Thu May 14, 1992 3:44 pm PST
Subject: LITTLE BABY

[From: C. Love (920514.1000)]
[To: CSG-L LIST]
Hello again,
I have a few questions for the PCT folks. My "little baby" is growing up more and I need a little advice. There are seven ECMs in my current system, three are used to drive the arm (2 at shoulder and 1 at elbow) and four for the eyes (one for left eye vertical, one for left eye horizontal - same for the right eye). Now since all this is happening on a Macintosh things must be done *one at a time*.

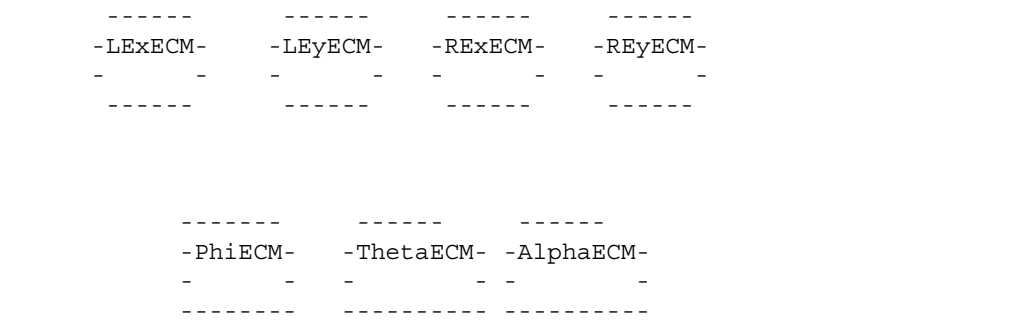
Essentially, these are the steps taken in a *time slice*:
1) Retina percepts are made available to all interested ECMs.
2) Angle Percepts are made available to all interested ECMs.
3) All ECMS calculate the weighted perceptual input and store this in its

internal cell called "Percept Sum".
4) Task Reference signals are made available to highest level ECMs.
5) Eye Reference Signals are made available to angle ECMs.
6) Angle ECMs provide their reference signals to (deltas) to external world, which adds these to existing angles, i.e. modifies angles.
7) Each ECM now has all its signals necessary to do the internal calculations so internal ECM calculations occur.
This involves determining the percept and reference output for each ECM.
8) Learning Phase. This involves adjusting the weights. Well, I'm working on it....

The question is whether or not this sequence of events is a good choice or not. I want to know whether it may be better to, say, propagate the reference signals from the upper ECMs down to the lower ones before evaluating the lower ones. This is something similar to what happens in BP. Here, everything happens *simultaneously*, although still procedural. Intuitively, I think things should happen this way. What I'm getting at is that signals at all levels are unaware of whether or not they're receiving new or old signals; they just take them in, process, and crank out a reference and percept. In this manner I think this *simultaneous operation is correct.

Any opinions?

Thanks,
Chris Love.
P.S. Here is a rough graph of the architecture...



Ok These three (external angles) receive the deltas from the three angle ECMs (interanal) and the Percepts (external), eg. LETx=Left Eye Target x-coord, LEFx= Left Eye Finger tip x-coord, are fed to all ECMs. Finally the reference (task) signal for the top four ECMs are zero input, i.e., match finger to target

That's all.

Date: Thu May 14, 1992 6:32 pm PST
Subject: Contextual Standards

From Greg Williams (920514)

I find much appeal in the recent posts by both Dag Forssell (920513 - 2) and Rick Marken (920513 15:30) on standards and PCT. It seems to me that Rick's viewpoint, with PCT ("all perception/all control/all numbers") in the foreground, addresses the issues in a general manner, while Dag addresses some particulars. I can see validity to both in their special provinces -- but I think everyone must beware of being overly provincial.

From an examination of the histories of the diverse ethical systems which have flourished around the world at various times, ethical CONTEXTUALISM (rather than relativism or absolutism) might be the best model. The nasty connotations of ethical relativism (some noted by folks quoted by Dag) are apparent when the issue is framed as: In THIS society, you're trying to tell me that anything goes (makes sense, fits in, works)? Obviously ("OBVIOUSLY!") THE WAY WE UPSTANDING CITIZENS DO IT IS WHAT IS BEST!!! The nasty connotations of ethical absolutism (pointed to by Rick) are apparent when the issue is framed as: Why don't those people in that OTHER society do it the way WE do? Obviously ("OBVIOUSLY!") our way works, and so it should work for them, too.

An ethical contextualism offers a middle road, recognizing that within a particular context (sometimes quite broadly defined -- i.e., we're all human), there are certain standards which DO "work," but also recognizing that if the context is different, those standards might cease to "work." Most tribes, I have read, refer to themselves as The People, which emphasizes their distinctness from others who AREN'T The People. That insularity, rooted in ongoing personal confrontations with a particular context, makes great sense up to a point. Then the conquerers come along, of course, and try to impose a new ethics (no more infanticide, etc.) AND A NEW CONTEXT. If the new ethics precedes the new context (and probably even if it doesn't), there is a great likelihood of pain.

Personally, I would like to see more recognition that individuals' contexts are much more variable WITHIN our own society than many people like to admit, and so there are grounds for ethical pluralism (e.g., in attitudes toward abortion as influenced by economic status). Yet I understand that there is a perceived need to restrain such pluralism in hopes of keeping "us" (e.g., U.S.) "together" in the face of "challenges" (mainly "foreign competition," it seems, these days) from "outside."

So I can see the cases for local (sometimes VERY local -- and possibly VERY ephemeral, too) "absolute" standards AND for the contextuality of ALL of those standards, seen more globally.

It's all contextual.

Greg

Date: Fri May 15, 1992 9:21 am PST
Subject: Autism and HPCT

FROM CHUCK TUCKER 920514

I was reading the 18 May edition of NEWSWEEK and noted in the section on "The Arts" (!?!) mention of a book by Jean and Sean Brannon THERE'S A BOY IN HERE Simon and Schuster $20 which apparently discusses how Jean(Mother) was able to get Sean (Son) to alter his behavior from "autism" to "ordinary". The article notes that the "behaviorist approach" has shown some "remarkable results in some cases" but ". . . another promising - although controversial - treatment has been pioneered by psychiatrist Martha Welch, who runs "the mothering center" for the families of autistic children in Greenwich, Conn. Welch teaches parents to interrupt the autistic child's withdrawal, physically trying to get close to the child and enticing him into the real world."(70) Judy Brannon, it states, "... simply refused to let her son slip out of her reach into the hypnotic trancelike oblivion he preferred. She hung on to him, fighting him at every step, shouting at him, shaking him, physically stopping him from repeating aimless activities and forcing him to look at her, to listen
to stories or play games."(70)

Now this reads like force to me but the question is: In these circumstances isn't such force warranted as disturbances to get the person to ALTER THEIR OWN CONDUCT? Obviously., from a HPCT view the Brannon's see their acts as the proper choice but this does not approve of all acts of force or specially design disturbances but is does bring up the question: When and where are disturbances (even physical force) appropriate? (excluding the whisking of a child out of the path of an oncoming 18 wheeler!) Don't we learn from our errors and are they not OCCASIONED (note this word) BY DISTURBANCES?

COULD THIS BE A TOPIC FOR A "CLOSED LOOP"????

Regards, Chuck

Date: Fri May 15, 1992 11:13 am PST
Subject: Re: Speech Recog and Neural Nets

Hi Chris: say, could you recommend a good general
text on speech recognition? (something real modern-like!)

If you know the details pls provide Authors initials date and publisher

oh yea and the title!

Thanks Tim.

Date: Fri May 15, 1992 1:19 pm PST
Subject: models in context

[From Rick Marken (920513 09:00)]

Dag Forssell (920513-2)] says:

> That there is no one "reasonable"
>setting for anything at any level may be quite valid. Is that a reason
>to never discuss any suggested settings at the principle level?

The change in the height of a column of water depends on the volume, not the mass, of an object that is placed in the water. Is that a reason never to discuss ways to bring the water level to a particular height by suggesting settings for the mass of the object to be added? I think the answer to your question is another question; What do you consider to be a waste of time?

Ed Ford (920513.12:12) says:

> We all of us have a variety of perceptions of how things ought to
>be. This is found at our systems concepts level.

There are called references; they define what we ought to be perceiving. These exist at ALL levels in the model -- not just the system concept level. So we have references for how much pressure to feel on our fingers and how much like a fist our hand configuration should be in and how rapidly our hand configuration should be changing (ESPECIALLY WHEN CAPTURING SATELLITES __ WHAT A GREAT PIECE OF CONTROL!).

> It seems to me we are setting standards for ourselves and in
>cooperation with others (such as in a home, a community, or within an
>organization) all the time.

This is the crux, I think. We care about 'standards' because they often determine lower level actions that might influence the variables controlled by other people. I think Greg picked up on this in his last post:

Greg Williams (920514) suggests

> ethical CONTEXTUALISM (rather
>than relativism or absolutism) might be the best model.

>An ethical contextualism offers a middle road, recognizing that within a
>particular context (sometimes quite broadly defined -- i.e., we're all human),
>there are certain standards which DO "work," but also recognizing that if the
>context is different, those standards might cease to "work."

Yes; and the important context is other control systems. My spreadsheet model has to be expanded to two (or more) simultaneous hierarchical systems working in the same envronment of numbers (degrees of freedom). I think you would find that these models would quickly run into conflict if their HIGHER LEVEL (level three) systems were controlling for the same variables relative to different reference levels. There would always be less conflict at the lower levels because the references for those levels can be changed by the higher level systems that see that there are lower order errors.

Actually, I think I will do this modeling effort; but my intuition is that the only way to solve the problem of multiple interacting control systems, operating in the same environment, is to align the references for the highest order systems that are controlling the same perceptual variables. I wonder if the solution would be found automatically (through reorganization) or whether there needs to be a system that actually perceives that there is conflict and looks for a cooperative solution. I think the former might work.

So I think it's possible that alignment of higher order references may be a natural consequence of being reorganizatble, hierarchical control systems. Of course, the values at which these systems get aligned are not necessarily determined -- just as long as they are aligned. I think this is why we see such remarkable differences in cultures (as Greg noted -- we ought to get some anthropologists in on this). But there are remarkable differences between cultures in terms of system concepts like marriage (polyandry, monogamy, polygamy, etc) -- and they all work; apparently because everyone buys into that reference. Of course, once pressures lead individuals to shift references (our society seems tacitly moving from monogamy to serial monogamy -- largely as a result of an unpredictable disturbance; people are living longer) conflicts between control systems increase -- as would be expected until the group is able to "realign".

By the way -- all you "standards" fans; fear not. It's highly unlikely that any society will align on a system concept that demands really "bad" standards like murder. There are standards that are self correcting (the people who aligned on the system concept that demands "murder" would be quickly eliminated from the pool of control systems). Note, by the way, that most societies have aligned on system concepts that make it perfectly ok to murder the members of other societies. But that's getting into more substance than I think is appropriate -- back to models.

>It's all contextual.

Ok, I'll buy it.

How about another:

It's all interacting control systems

Best regards Rick

Date: Fri May 15, 1992 1:34 pm PST
Subject: What CT says about X

[From Bill Powers (920514.0600)]

An essay inspired by bits and pieces that have appeared on the net:

What does control theory say about X?

Such questions almost always contain hidden propositions: X is a phenomenon, and control theory should have something to say about it. But to know what is being asked, you have to ask what theory underlies X. Most questions about X beg some question, assume a theoretical stance that existed long before control theory came along.

Control theory is:

not trait psychology	(characteristic --> behavior)
not top-down causality	(cognition --> behavior)
not bottom-up causality	(circumstances --> behavior)
not S-R causality	(events --> behavior)
not intervening variables	(A --> B --> behavior)
not categorical explanation	(things like A cause behaviors like B)

not associationism (A cooccurs with B)

NOT ANYTHING THAT CAN BE LAID OUT ALONG A STRAIGHT LINE.

A Chomksyite evidently proposes "because I can perceive a certain structure of relationships in language, that structure produces language." The question is thus, "what does control theory have to say about the way structure produces language?"

A Harrisite evidently proposes "because I can perceive sequences of operators and arguments, operators and arguments cause language." The question becomes "what does control theory have to say about the way the occurrance of operators and arguments produces language?"

And others seem to propose "because children I perceive as bright may have eczema more often than others, brightness causes eczema." The question: "what does control theory have to say about the effect of brightness on having eczema?"

The most common mistake in model-based explanation is to put an emergent phenomenon into the model as a box defined to produce that phenomenon. In diagrams of control systems one often finds a box labeled "controller." But control is not something that any one component of a control system can do: the "controller" box by itself controls nothing; it simply transforms an input into an output. Control is what emerges when input-output components are interconnected in a certain way and allowed to interact with an environment. The term "control" should not appear anywhere in a model of a control system.

If it seems that certain words act as operators in relation to certain other words that act as arguments, then a model that explains this phenomenon should contain neither "operators" nor "arguments." To say that operators "take" arguments is to put a box into the model that produces the emergent phenomenon.

If it seems that there is structure in language, then a model that explains this phenomenon should not contain that structure, but only components that lead to phenomena which can be seen as having that structure. The apparent structure should emerge from the underlying processes. Even to say that a word can "modify" a phrase is to make the modification process part of the model instead of emergent from it.

If it seems that bright people have eczema more than others do, then a model that explains this phenomenon should contain neither "brightness" nor "eczema". It should have an organization from which emerge phenomena that can be seen as brightness and as eczema.

Every person comes into control theory from some other point of view. As a result, every person has beliefs about the questions that must be answered in order to make progress toward understanding some facet of human nature. But all accepted points of view from the past lead to questions that imply an answer that can be laid out along a straight line. Control theory can't answer such questions (or validate the theory that led to them) because control theory proposes that behavior is NOT produced by processes that can be laid out along a straight line -- processes in which the outcome can be considered separately from its antecedents.

The most important question to ask is not what control theory has to say about X, but what OTHER theory is behind the very question. It is likely to be one of the kinds of theories listed above. Before you ask such questions, it would be profitable to ask about the assumptions behind it. That may tell you the answer without your having to ask the question.

Best Bill P.

Date: Fri May 15, 1992 1:37 pm PST
Subject: Standards

[From Bill Powers (920514b)]

An added comment to back up Rick Marken's excellent comments (920513) about standards in relation to Dag Forssell's list of desirable standards (920513b). Rick points out that these are not reference levels, but variables. It's easy to show that they are variables just by finding words to indicate other states than the states one automatically assumes for them (the ones you like best). Here's Dag's capitalized list with some side-comments:

COURAGE,	Bravado, foolhardiness
CONVICTION,	Stubbornness, prejudice
GENEROSITY,	profligacy, gullibility
KINDNESS,	bleedinghearted sentimentality
HELPFULNESS,	nosy do-goodism
HONESTY,	bluntness, cruel candor
HONOR,	hubris, egotism, bushido
JUSTICE,	revenge, brutality, litigiousness
TOLERANCE,	naivete, permissiveness
THE SOUND USE OF TIME AND TALENTS,	working for someone else
FREEDOM OF CHOICE,	abortion as belated contraception
GOOD CITIZENSHIP,	supporting the war
THE RIGHT TO BE AN INDIVIDUAL,	offending everyone
THE RIGHT OF EQUAL OPPORTUNITY.	the right to sleep under a bridge

The problem with lists like these is that they define only dimensions of perception, variables, but by implication they specify some particular state of the variables that is "best." The right level for one person is too much for a second and not enough for a third. The right level for today and this person is the wrong level for tomorrow and someone else.

Even the perceptions that go with the words are different in different people. When a manufacturer supports the "Right to Work" act, a labor union opposes it, because the words mean one thing to the manufacturer and another to the union. When an inhabitant of South Los Angeles asks for the right of equal opportunity to work at rebuilding the wreckage, a white construction worker objects because it will deny him or her equal opportunity to make a buck doing the job at a higher wage. Freedom of choice is an empty promise for a person without the means of implementing any choices; for others, it's an excuse for maintaining segregation and shielding themselves from contamination by the rabble. A "sound" use of time and talent means, to some people, not wasting your time on fripperies like music and art and theorizing, but devoting your efforts to maximizing (somebody else's) profits. To a lot of people, honesty means that it's OK to cheat the IRS or a business rival, but not to cheat me.

The names of standards refer to things that are not words, but are shifty attitudes that vary with circumstances. All that makes sense of any kind of standard set to any momentary level is the system concept under which it is adopted. I thought that Ed Ford's recent discussion of standards hit a lot of nails on the head.

I also thought that Rick's statement hit a nail on the head: you can't set a reference signal to a constant value and expect the higher systems to go on working properly. They work by VARYING lower reference signals, not by picking one setting and sticking to it. This isn't "moral relativism;" it's simply recognition that the system concepts that organize and use principles are more important than any particular principle, or any particular state in which to maintain a given kind of principle. Moral rules followed blindly and implacably can generate the cruelest of all human aberrations. Bruce Nevin and Greg Williams came up with some nice examples of general rules (snatch the child out of the street) that don't make sense in different situations (a motorcycle gang riding down the sidewalk).

The only reference signals (and perceptions) that can't be changed freely as required by higher levels are system concepts. And the only reason we can't vary our reference signals and perceptions at that level with complete freedom is that there seems to be no place to stand except another system concept -- if there is a higher viewpoint, it's impossible to put into words or systematize. If there's free will, the only place it can work is at the top, because everything else is dependent and interconnected. And even at the top, we're free only to be human.

Best, Bill P.

Date: Fri May 15, 1992 2:56 pm PST
Subject: tar brush sweeps wide

[From: Bruce Nevin (Thu 920414 14:16:27)]

(Bill Powers (920514.0600)) --

>If it seems that there is structure in language, then a model that explains
>this phenomenon should not contain that structure, but only components that
>lead to phenomena which can be seen as having that structure. The apparent
>structure should emerge from the underlying processes. Even to say that a
>word can "modify" a phrase is to make the modification process part of the
>model instead of emergent from it.

If it seems that there are words in language, then a model that explains this phenomenon should not contain words, but only components that lead to phenomena which can be seen as being words.

If it seems that there are word dependencies in language, then a model that explains this phenomenon should not contain word dependencies, but only components that lead to phenomena which can be seen as being word dependencies.

If it seems that there are classes of words in language, defined by the classes of other words that participate in dependencies with them

(dependency on dependency), then a model that explains this phenomenon should not contain word classes, but only components that lead to phenomena which can be seen as being such word classes.

If it seems that there are reduced forms of words in language, available for more or less arbitrary (historically contingent) subclasses of words under stateable conditions mostly based on redundancy, then a model that explains this phenomenon should not contain reduced forms of words, but only components that lead to phenomena which can be seen as being or yielding reduced forms of words under such conditions.

I have no problem with any of this, and find it in good accord with all that I have been proposing. Operator grammar needs no more than the above.

>A Harrisite evidently proposes "because I can perceive sequences of
>operators and arguments, operators and arguments cause language." The
>question becomes "what does control theory have to say about the way the
>occurrance of operators and arguments produces language?"

If I have seemed to propose that operators and arguments cause language then I have not expressed myself well. Help me out, please. Quote something that I have said that seems to you to be inconsistent with what I claim (above) to have been saying.

>If it seems that certain words act as operators in relation to certain
>other words that act as arguments, then a model that explains this
>phenomenon should contain neither "operators" nor "arguments." To say that
>operators "take" arguments is to put a box into the model that produces the
>emergent phenomenon. . . .
>Even to say that a
>word can "modify" a phrase is to make the modification process part of the
>model instead of emergent from it.

I have tried to avoid metaphors that seem to attribute agency to language and to objects and relations in language, recognizing your penchant for taking these expressions literally.

>all accepted points of view from the past lead to questions that imply
>an answer that can be laid out along a straight line.

This is a sweeping generalization indeed. However, it is a generalization about all efforts "make progress toward understanding some facet of human nature." Harris's work has been to disclose and describe the informational structure in language. Interpretations as regards human nature or how language came to be that way or how people come to acquire language, and so on, have been purposely outside the scope of his work. (The only exception, to my knowledge, is the brief speculation on origins of language in the small _Language and Information_ volume based on the Bampton Lectures, and the main point thee is that it is not as hard as has been sometimes made out, because the requisites for language are not as complex or sophisiticated or different from other kinds of perception as has often been made out.) This has not made for great popularity or overwhelming influence in the field. It does, however, exempt his findings from your generalization.

>Control theory can't

>answer such questions (or validate the theory that led to them) because
>control theory proposes that behavior is NOT produced by processes that can
>be laid out along a straight line -- processes in which the outcome can be
>considered separately from its antecedents.

Operator grammar does not propose how the operator-argument informational structure found in language is produced. It is the Generativists who reify grammatical generalizations as mental organs.

The structure is there. The interpretation of it, or an account of how perceptual control systems bring it about, is up to us.

Bruce

Date: Fri May 15, 1992 2:58 pm PST

[From Rick Marken (920514 12:30)]

Gary Cziko (920513) says:

>Now that I understand the math, I'm wondering why k.o has to be so large.

Jan Talman pointed out an error in my math; my equation 4 should be

r = s*/k.f - (k.o/k.f) d

So the relationship between reference and response depends on the feedback function. This will require some major rewrites (Bill P already suggested others) on the reinforcement section; it also has interesting implications for the cognitive view. But it doesn't significantly change the conclusions of the paper (that the different perspective on behavior are different way of looking at the aspects of control described in equation 4).

k.o is generally large because it involves the amplification produced by all those big strong muscles. k.f probably is generally close to 1 because it is just a mapping of physical units into other physical units; there can be some amplification but that would involve adding energy into the system -- making k.f the representation of some kind of machine that has an energy supply. The magnification due to k.o requires an external energy supply too -- control systems must eat. Because they have this external energy supply, control systems are "open" in the thermodynamic sense; and are, thus, free to violate the first (second?) law of thermodynamics -- they reduce (rather than create) entropy.

Regards Rick

Date: Fri May 15, 1992 3:08 pm PST
Subject: autism and autonomy

[From: Bruce Nevin (Thu 920414 15:07:10)]

(CHUCK TUCKER 920514) --

>COULD THIS BE A TOPIC FOR A "CLOSED LOOP"????

Well, one prerequisite is that we talk about it enough to produce enough email material to be reprinted in Closed Loop. I would welcome seeing this thread renewed, whatever its potential for reprinting. We have (or had) some people with direct experience on the net.

I see autism as a limiting case of opting out of social membership, in which independence appears to have a terrible cost to autonomy.

I won't be so fatuous as to quote myself, but see what I said about independence vs. autonomy this morning (Thu 920414 07:44:16) if this is not clear.

Bruce

Date: Fri May 15, 1992 5:08 pm PST
Subject: standards

[From Rick Marken (920214 15:30)]

A public reply to a personal note from Ed Ford:

I know no one whose standards I admire more than yours. I think you and I have similar ideas about what constitutes an admirable individual. I think we run into a problem with these damn words. That's why I like models, I guess. It let's one back away and just look and see how it works. I think you are able to do this with real people better than anyone I know.

> I would ask that you reread AT LEAST the first
>four chapters of my book (which cover that particular area) and then I
>think you'd see where I am coming from and could help me with our
>apparent differences.

I certainly will reread those chapters [the book is "Freedom from Stress", by the way]. But I am sure that our apparent differences have more to do with what we want to communicate about PCT (and human nature in general) to our respective audiences than with what we understand PCT (and human nature) to be.

> If you want to send me a personal answer, fine, or if you want to
>send it via the whole net, that's fine too. I'm very interested in
>trying to understand where our differences are.

I took the liberty of putting this on the net because I don't mind if everyone knows how much I like you.

Best regards Rick

Date: Sat May 16, 1992 2:21 am PST
From: Dag Forssell / MCI ID: 474-2580
Subject: Disturbance

[From Dag Forssell (920516-1)]

Rick Marken (920513 15:30)

>The words in caps seem to describe perceptual VARIABLES -- one can
>perceive degrees of courage, conviction, generosity, etc. How do I
>derive from the HPCT model what value is the right value to set for each
>of these variables? I just don't think PCT has anything to say about
>this other than "people can control perceptual variables like courage,
>conviction, etc.". See the problem with talking about this stuff. People
>CARE about these things. I CARE about these things. But you are claiming
>that certain levels of these variables are RIGHT.

You cannot derive any values at all from the HPCT model, especially when it is viewed as a mathematical spreadsheet. I am not claiming that a certain level of these variables or references are RIGHT. You are inferring that from the chapter I quoted. What the chapter said to me (in the part I wanted to draw attention to) was that most people upon reflection come up with most of this list. I have meant to offer the observation that perhaps a lot of people get along quite well in spite of holding religious systems concepts that are totally incompatible because they tend to set references at the principle (what Ed calls standards) level SIMILARLY anyway. (I have used the word "reasonable," meaning well thought out, but never in my mind suggested ABSOLUTE or CONSTANT; that is your interpretation and contribution - it does make for feisty argument). Perhaps that shows that more "down to earth" systems concepts /understanding based on experience instead of "intellectual /religious" constructs is what really influences the principles most people go by.

I do understand that there is not just one "right" reference value for a perceptual variable anywhere in the hierarchical structure.

I do not understand your emphasis on VARIABLE to describe the list, as if to disqualify reference. As I understand it, precisely the same perception that we call the reference is what "behaves" to create the specified perception of what we call the variable. The words used to describe the reference and the variable perceptions are identical, since the perceptions are identical. You have to specify that you are referring to one or the other. Neither is fixed, since the reference is set at the moment as part of the entire, interacting hierarchy.

> "Such a position of normalness," writes Professor Philip H.
Phenix,
[must be "normlessness" no? -- rm]

Yes, my transcription error -- df

I can see how this segment raised your ire/suggested that I am advocating ABSOLUTES. It was part of the chapter and the argument in favor of character education.

Personally, I believe it comes naturally to want to find some meaning in your own life. I think meaning can be found in secular systems concepts just as well as in religious systems concepts.

Rick Marken (920513 09:00)

>I think I will do this modeling effort; but my intuition is that the
>only way to solve the problem of multiple interacting control systems,

>operating in the same environment, is to align the references for the
>highest order systems that are controlling the same perceptual
>variables.

Great! Maybe we will be able to illustrate more of how control systems disturb one another. You can get part of the way there with rubber bands, but only on one level, of course. I share your expectation about the requirement. This means that we have to talk until we have the same systems concepts, after all. It will not be enough to say that you subscribe to the same principles.

This is what Greg (920514) observes, as applied to each tribe or subgroup in its context.

This entire exchange has caused me to reflect on my own assumptions and understandings. My ideas relating to character education go back to 1980-83. I have not scrutinized these particular systems concepts in the light of PCT until now. I have already reorganized some, but have not settled down yet. I find merit in Ricks observation that: It's all control. (That is what the closed loop handshake stands for)!

HPCT as a model has much to offer. My interests focus on how to teach and apply it. Since we live in a real world with finite degrees of freedom, and a boss reality to study, it gets important to reflect individually on the specific perceptions you fill your own hierarchy with at all levels, so that you can control well. Numbers are not enough. As a parent, manager, teacher or counselor, it is my challenge to assist those who want to be assisted to fill themselves with good information. Good information will include an understanding of PCT.

I have just reviewed "An agenda for the Control Theory Group" in Living Control Systems II. On pages 171-172, Bill writes:

>It's strange how difficult it is to find applications for the first new
>conception of behavior in over three centuries. We've all had this
>trouble, but those of us who have had the most trouble are those who
>know the most about the life sciences. I think that's a clue. Think of
>clinical psychologists (we have a few of them here). What could clinical
>psychologists do with control theory? Well, they could play how-and-why,
>they could try to find out what the client is controlling for. But how
>would they use the techniques? Probably to try to help people get over
>anxieties, phobias, compulsions, depression, or stress. And that's where
>I think the difficulty starts.
>
>No matter what discipline in the life sciences you are attached to as
>a scientist or layman, when you look around for something to study, you
>find that the human pie has already been sliced. Everything has a name.
>People study personality, intelligence, aptitudes, attitudes, and
>preferences. They study mental illness. They study customs. They study
>marital problems. They study crime. They aren't trying to find out if
>such things really exist: if they have names, they must exist. The
>categories in which we think about human behavior have already been
>established, and that is really why it's so hard to wedge control theory
>into the structure.

It occurs to me that some of the problem I have just wrestled with is what Bill describes here.

There is no such thing as character. There is only effective control. Internalizing the systems concept of yourself as an autonomous control system and adopting the same systems concept an behalf of others (a value judgement) may lead you to principles of similar appearance as those that have been labeled character. As I said, I have not settled yet.

Additionally, perhaps there is no such thing as violence, coercion, social control, influence etc., which it is why it becomes difficult to distinguish one from the other.

What there is is control by one system which creates disturbances for another control system. If two systems control the same variable, you have to look at the coupling of each to the variable: loose or tight, and the resources (or amplification or force) available to each. In arm wrestling, two control systems control the same variable with tight coupling. The control system with the most force minimizes its error signal. The other system gets a large error signal.

Bill Powers (920512.0930)

>Influences should be thought of as disturbances. That is, you can
>perform an act that by itself would alter the other's perceptual world
>if it were the only influence.....

It makes sense to me to see influences as disturbances. Can you see information as disturbances also? In one book on listening I read long ago, the author suggested that in active listening, you choose to anticipate what the speaker will say next, see what he does say, and compare the two. When you guessed right, you confirm with satisfaction. When the speaker says something else, you think about it intensely. Either way, you are alert and hear well. Of course, you may control so you hear what you want to hear instead. You put the words into your own context.

With this in mind, I can think of reading a post as a lot of small and some not so small disturbances. I have to recognize that I am disturbing just the same when I post. Some of the information flies by with minimal disturbance, some is unsettling.

As a parent, I create disturbances for my child in many ways, which the child has to deal with. Thus the child fills with experiences /perceptions /understanding throughout the hierarchical structure. If I plan the disturbances well, the child learns to control well. I could say that I deliberately create error signals in my kid. This thinking agrees with Chuck Tuckers post (920514).

Bill said ((920512.0930):

>Parents influence their children by (for example) advice, commands,
>example, demonstration, and story-telling.

I am now beginning to think of all these forms of influence as made up of disturbances. Make sense?

I am controlling and perceiving as best I can.

Dag

Date: Sat May 16, 1992 2:55 am PST
Subject: k.o

(Rick Marken (920514 12:30)) --

(Except the mail header says it's from Gary.)

You might consider including in the paper some comment that k.o is so large because it is a representation in the model of biological and physical facts about muscles and their use of metabolic energy.

Will you be distributing the revised paper? I'd like to send it to some people.

Bruce
bn@bbn.com

Date: Sat May 16, 1992 3:43 am PST
Subject: LITTLE BABY

[From Rick Marken (920515 08:30)]

C. Love (920514.1000) says:

> I have a few questions for the PCT folks. My "little baby" is growing up
>more and I need a little advice. There are seven ECMs in my current system,

Is an ECM a control system? Ectoplasmic Control Module?

>three are used to drive the arm (2 at shoulder and 1 at elbow) and four for
>the eyes (one for left eye vertical, one for left eye horizontal - same for
>the right eye).

Just a verbal nit -- but if ECMs are control systems then its probably better to say that they are "used to drive perceptual variables that are influenced by movements of the arm and eyes". That just keeps the conventional roboticists
on their toes (or drives them away -- where's Randall Beer, by the way?).

> Now since all this is happening on a Macintosh things must
>be done *one at a time*.

>Essentially, these are the steps taken in a *time slice*:

... the steps

>The question is whether or not this sequence of events is a good choice or
>not. I want to know whether it may be better to, say, propagate the reference
>signals from the upper ECMs down to the lower ones before evaluating the lower
>ones.

It looks like you are, indeed, buiding a hierarchy of control systems. The order of calculation problem is interesting -- in fact, I think it's causing me problems in my Excel spreadsheet (which you might look at because Martin Taylor has a working copy). I think the order of recalculation in excel does cause problems sometimes (I've never had such problems with lotus; maybe i'll go back to ibm?).

For any individual control loop, the way to compute the variables is in the intuitive sequence:

In the following

i = input variable
o = output variable
d = disturbance variable
p = perceptual signal
r = reference signal
e = error signal

1. compute the current input (environmental) variable value (i = o + d)
2. compute current perception (p = f(i))
3 compute error (e = r-p)
4. compute output (o = g(e)) where g() includes the gain and integrations that handle the dynamics of the loop

At this point you could repeat (if there is just one control loop) or start the same calclulations for another loop AT THE SAME LEVEL IN THE HIERARCHY. Actually, if the calculations are for the lowest level loops in a hierarchy, I would carry out step 1 for ALL inputs before doing steps 2-4 for each loop. That is, determine the current state of the environmental input to the entire system; then use 2-4 for each system.

Once you have completed a 2-4 cycle for all the lowest level loops you have the values of p for all these loops; these (as well as some i values) are the inputs to the next level of loops; so do the 2-4 step for each system at level 2 in the hierarchy. Once all the level 2 systems are complete, do the level 3 -- etc.

Thanks for asking this question -- it makes me realize how lucky I am that the spreadsheet model works at all; I'll have to go check the recalculation order in excel to see what the problem might be; and also see why it works so well in lotus.

Hasta Luego Rick

Date: Sat May 16, 1992 3:58 am PST
Subject: Re: LITTLE BABY

[From Joe Lubin (920515 12:00)]

C. Love (920514.1000); Rick Marken (920515 08:30) --

One suggestion. To (i) model the hypothesis that lower-level control systems operate faster than the higher-levels, and (ii) to give each level a chance to settle down a bit before its reference signals prod it again, iterate each levela few (4?) times for each iteration of the level above it.

So if you have a two level system, one external clock cycle will encompass four
lower-loop updates and 1 higher-loop updates. Since nobody has the hardware to
build really deep (hierarchically) systems yet, it is not clear whether this strategy would be necessary/useful among the higher-levels, although it couldn't hurt.

Joseph Lubin jmlubin@phoenix.princeton.edu
Civil Eng. Dept. 609-683-5301
Princeton University 609-258-4598
Princeton NJ 08544

Date: Sat May 16, 1992 4:14 am PST
Subject: Re: LITTLE BABY

[From Rick Marken (920515 10:00)]

Joe Lubin (920515 12:00) says:

>One suggestion. To (i) model the hypothesis that lower-level control systems
>operate faster than the higher-levels, and (ii) to give each level a chance to
>settle down a bit before its reference signals prod it again, iterate each
> levela few (4?) times for each iteration of the level above it.

This technique was used by Powers in his two level force control model described in his Byte magazine series (I think it was in the July, August or September issue, 1979) -- it was the third article in the series. It's an excellent article; it is not reprinted in Living Control Systems I or II. But it's worth a look, Chris.

In that article, Bill describes a two level model that controls the x and y component of force exerted by a muscle group anchored at a center point. In the BASIC program version of that system, the lower level system went though several iterations of calculation before the high level system started its calculation.

My experience has been that this extra lower level calculation is unnecessary for the stability of a multi level hierarchy (I don't do it in the spreadsheet model and I didn't use it in other programs using Pascal and Basic). I've found that all you need is the appropriate slowing factors on the outputs going to lower level references; the output (and, thus, the speed of the loop) should be slower for higher level than for lower level systems. The extra lower level loop is a good way to imitate a "transport lag" delay for the higher level system (those systems must wait for a period equal to the number of iterations to get their inputs). While such a lag actually exists
in the human nervous system (and it is obviously longer for the higher than for
the lower level systems) it does not seem to be a stability requirement. In fact, it is probably something that must be smoothed out by other aspects of loop dynamics. But, since lags exist, it's probably a good idea to

put them into the models -- just for the sake of realism.

Hasta luego Rick

Date: Sat May 16, 1992 4:35 am PST
Subject: Little Baby & Little Man

[From Bill Powers (920515.1000)]

Chris Love (920514.1000) --

It looks as though you've done things with Little Baby a lot like I have done them with Little Man. Looks good.

A tip on depth perception. If the left eye and the right eye both see the fingertip and the target, the lateral separation between the two as seen by the two eyes will be different if the distances are different. Define:

LEt = target angle seen from left eye
LEf = finger angle seen from left eye
REt, REf ditto for right eye,

All angles are measured from a retinal reference point, say the center. The angular separation of target and finger in the two eyes is

LEs = LEt - LEf
REs = REt - REf

The difference between LEs and REs is

REs - LEs = REt - REf - LEt + LEf
= (REt - LEt) - (REf - REt)

The final terms on the right show the amount of "doubling" of the target image minus the amount of doubling of the fingertip image, after the left and right images are combined. No matter how the eyes are converged, the only way the amount of doubling in the two images can be the same is for the target to be at the same distance from the eyes as the fingertip. So the reference-doubling can be taken as REt - LEt, and the perceived doubling as REf - LEf. The error signal drives the elbow angle to change the "reach" distance; when the error is zero, the fingertip is at the same distance as the target and both images are doubled by the same amount.

I think this is a more "physiological" way of perceiving depth than computing sums of squares.

Actually, in Version 1 of the Little Man I used

k * (eye separation)/(REx - LEx) as the distance perception, because it gets larger as the disparity decreases, the way we actually perceive distance. Of course you can't let the denominator go to zero. Alternatively, you could say that we control for proximity, and use the difference-signal right-side-up.

My Version 1 didn't have separate eye control -- I just tilted and rotated the head with the eyes fixed in it. Version 2 does move the eyes in the

head in addition to head control, and I will shortly add the control systems to make each eye track the target independently, with a fraction of the movement being head movement. Then another approach to depth perception becomes possible, which you might like to try:

With the eyes converging, the output signals of control systems for each eye can be perceived as "reafferent" signals (equivalent to sensing the reference signal sent to oculomotor systems, an imagination signal). The difference between these signals indicates, by the same arithmetic as above, the distance at which the eyes are converged. This is subjective distance, of course, not linear objective distance.

Now we have THREE depth signals: the doubling of the target image indicates target depth, the difference in eye-position signals indicates depth at which convergence takes place (whether there's an object at that depth or not), and the doubling of the fingertip image indicates the fingertip depth. I think this arrangement will prove useful when I get around to introducing true "transition" control -- path planning, as I now think of it. The fingertip is first controlled to show zero doubling: this brings the fingertip to the same depth as the convergence depth. Then the convergence depth is varied to reduce the target doubling to zero. In the process, the fingertip tracks the moving convergence depth while the convergence depth is moving toward and tracking the target depth. This will allow adjusting one time constant -- that of the convergence control systems -- to vary the speed with which the fingertip moves from an initial position to the target. This same principle would be used for x and y control, which are easier. The great advantage of this two-step arrangement is that when the target suddenly jumps to a new position, the kinesthetic systems that move the finger aren't hit with an infinite rate of change of position reference signals.

Of course when I actually get around to doing this, a completely different idea might suggest itself. I won't do this until Version 3 -- Version 2 goes to Greg Williams today for wringing out the uglies.

Your sequence of events looks OK to me. The trick is to complete all calculations for a time slice that involves NO CLOSED LOOPS, so that in effect everything is changing simultaneously. Then you close all loops with one final calculation that goes through the external world and changes the inputs to the perceptual systems. You seem to be using purely integral output functions (adding deltas) so you won't have any problems with computational oscillations unless you make the deltas too large. If you run into instability (smooth overshoot or oscillation) problems, you might try adding a proportional output component (proportional to error but not integrated). This will put some damping into the system.

You say, "I want to know whether it may be better to, say, propagate the reference signals from the upper ECMs down to the lower ones before evaluating the lower ones. "

The trick here is to figure out how to make all functions work in parallel. Consider two levels of perception. If you evaluate the lower level first, then convert it to the higher level perception, you've changed the lower perception without simultaneously changing the higher one -- the higher one now depends on the lower one at the END of the time slice rather than at

the beginning. So strictly speaking, you should evaluate the top level of perception first, then the next lower, and so on, so every perceptual function responds to the state of its inputs as of the start of the time slice (the end of the previous slice).

On the output side, you have to do the opposite: compute the lowest ECM's output FIRST, and then the next highest one, so that the lowest reference signal is correct for the start of the time slice before you then change it.

All in all, this says that you should start at the outputs of the lowest level system and work backward through that ECM, then work backward through the next level up, and so on.

Having said all that, I now confess that I actually do it forward, going up the perceptual side and down the output side, computing as I go. The reason isn't laziness, but Greg Williams, who insists that the model should contain somewhat realistic transport lags. When you do this, you find that a proportional model won't work because it oscillates violently. An integral-output model (like yours) is required to compensate for the transport lags, implicit in the computing cycles and the definition of dt. By defining dt so that one computing cycle is the transport lag you want, you then have to stabilize the model in a realistic way. I haven't really done this very carefully. The best way would be to do exactly simultaneous calculations, and introduce transport lags as shift registers so you can specify the exact lag at each level. I didn't do it that way, because you have to make dt very small and everything runs too slowly (for me).

By the way, don't confuse "transport lag" with "reaction time." Reaction time, as normally measured, is a much higher-level phenomenon. Transport lags at the levels we're talking about are around 9 milliseconds for the spinal loops, maybe 50 milliseconds for the next level, and possibly 100 to 150 milliseconds for the lowest visual control level (not the saccade level). The usual reaction times you read about are around a quarter of a second, which is way too long for these control systems. And the way they're usually measured, you can't tell a true transport lag from an integration lag (which is a much more tractable and friendly beast).

>These three (external angles) receive the deltas from the three angle >ECMs (interanal) and the Percepts (external), eg. LETx=Left Eye Target >x-coord, LEFx= Left Eye Finger tip x-coord, are fed to all ECMs. >Finally the reference (task) signal for the top four ECMs are zero >input, i.e., match finger to target

You can also use the Target position as the reference signal -- same difference. In fact you'll want to if you add any higher level systems. The reference signal is then the desired fingertip position, set by a higher system concerned with a finger-to-target relationship (which needn't be zero distance). But yes, what you're doing sounds fine and it's the way I did it in Version 1.

After Greg goes through the Little Man Version 2 and OKs it, I'll make it available for distribution -- at a price. I need a newer faster portable computer with a bigger disk and a tape backup. Of course it will be shareware for those who can't afford $100 and can't wring it out of their institutions.

For those awaiting Version 2 of the Little Man, here are the capabilities currently in it (no big changes before release):

1. Spinal control loops incorporating stretch and tendon reflexes, with a linear muscle model (I may stick in the nonlinear one before release -- turns out that it's just a square function, which will cause no difficulty).

2. An arm model with two shoulder degrees of freedom and one df at the elbow, with full kinematic treatment. Input to the arm model consists of three torques; output consists of three angles one dt later.

3. A second kinesthetic level that changes to x,y,radius coordinates of control where x and y are angles, not Cartesian coordinates. This level is necessary to obtain a fixed reference position for the arm in a gravitational field. When the first level alone is used, turning gravity on makes the arm slowly sag, as in "waxy flexibility" typical of spinal preparations.

4. A visual level that works in either of two ways (not simultaneously in this Version): direct visual control as in Version 1, and visual control through an adaptive 3-D map. The latter permits the model to reach out and touch a target that is seen before but not during a movement, and in general to reach out to visually remembered positions. This map-control model reproduces fast-movement fingertip trajectories from the literature very nicely, whereas the pure visual control mode does not.

The eyes track the target by moving in the head; the head also moves part of the way. I may put in independent convergence in the release, but it won't be used for depth perception. Haven't decided how that should go yet.

5. Two testing modes that drive the spinal systems and the kinesthetic second-level systems with square-wave reference signals. All parameters are adjustable from the keyboard so you can experiment.

6. An overall testing mode that permits manually or automatically jumping the target back and forth between any two positions, while you adjust the visual control parameters.

7. A learning mode that can be used when map control is on, gradually making the kinesthetic space agree with the visual space. A random target positioning mode is available so you can turn learning on with a cleared map, go away and read a book for a couple of hours, and come back to find the map all adapted (1331 cells). This will permit putting prism glasses on the Little Man and demonstrating fast pointing errors that gradually adapt out (as long as the change isn't too drastic -- there are limits to the amount of correction possible). And of course have to be undone when the glasses are removed. I haven't actually set this up, because the initial adaptation process demonstrates the principle adequately; the kinesthetic system begins out of whack with the visual system when you initialize a new map.

You can also have the model learn just two points in the map, for quick testing.

The "purpose" mode still works (with learning off but under map control); the Little Man draws a circle continuously around the target.

The "live block diagram" mode still works, although it's not detailed enough.

Maybe after Greg has played with the model for a while he can post a review of how it works.

Best, Bill P.

Date: Sat May 16, 1992 4:48 am PST
Subject: an ethics issue?

To: CSGnet people
From: David Goldstein
Subject: an ethics issue?
Date: 05/15/92

I would like people's opinion about what is an approach to handling the following situation from an HPCT viewpoint:

An older person has gone through some medical testing. The tests point to a cancer condition. But the diagnosis can only be probable without a biopsy. The older person refuses the biopsy. The older person is experiencing considerable pain and GI symptoms and is losing weight because of fear of eating. The symptoms will continue and worsen if the cancer diagnosis is correct.

Most of the older person's children don't want to take hope away by "hitting the person in the face" with a diagnosis of cancer. Their attitude is to provide caring and doing. One child wants the parent to know.

What is the HPCT approach to handling this situation? My reading of HPCT is to tell the older person if the person wants to know and don't tell otherwise. It is not so easy to tell what the older person wants to know, however. The older person may be in conflict about wanting and not wanting to know. The older person has already made the decision that the person does not want any chemotherapy or surgery or any other "heroic" efforts.

What is your reading of HPCT in this ethics sort of situation?

Date: Sat May 16, 1992 4:54 am PST
Subject: A Fine Kettle of Fish

From Greg Williams (920515)

>Bruce Nevin (Thu 920514 07:44:16)

>I'm asking quite seriously for some elaboration on your remark that we
>can't adjust our own reference signals, just as no external agent can
>adjust them.

I WAS (920514 - 2) being serious, even about not liking fishy-type food -- though not elaborate, and maybe a bit ironic, or even snide. Perhaps in the future I should add a straight-lipped happy face to indicate SERIOUS snideness? :-| At any rate, here's a (still serious) elaboration.

>It sure appears as though there is a driver in the driver's seat,
>doesn't it?

It sure does to me. But PCT does NOT address the "reality" of this appearance, except in a kind of negative way: the theory says nothing about there being a "driver" (or "self," or "you" to whom I was referring toward the end of my first post yesterday) who "steers" (or interacts in ANY way with) an individual's control structure. The closest the theory comes to a "driver" is HPCT's highest reference level, which, in a sense, "governs" the operation of the rest of the hierarchy (yes, various reference signals are changing). But what sets the highest reference level? I think the theory as currently held says: history (including various events both INSIDE and OUTSIDE the organism). My central claim is that the moment-to-moment operation of an individual's control structure is NOT MIRACULOUS, but rather a function of its past history (perhaps in a probabilistic way -- I have no desire to get into questions about "absolute" determinism). According to PCT, we are CYBERNETIC beings, behaving/acting in certain ways because of the history of each of our control structures IN INTERACTION WITH each of our environments (or more exactly, those portions of our environments which affect our individual control structures -- what Maturana terms our "niches").

However, it is possible for a self-determination believer to ADD some constructs to PCT and thereby have it support his or her position. Perhaps the willful "self" can alter the operation of the hierarchy by "deliberately" altering that structure, so it jumps from one state to another. Perhaps that occurs via something like Eccles' miracles in the synaptic terminations, or even in the pineal gland (though I suspect one might do better to look in the hippocampus). Or maybe there is a material embodiment of will, and no miracles are needed -- again, that must be an ADJUNCT construct to current PCT.

Skinnerianism, at root, claims that we behave now in ways determined solely (virtually -- some of them waver to varying degrees on the question of some rock-bottom innateness) by the history of our niches. Some PCTers emphasize that we behave now in ways determined solely by the current structures of our control hierarchies, without noting that those current structures were determined by the history of our niches AND our control structures. Niche history is not our only current-moment determinant, according to PCT, which is thereby NOT like Skinnerianism. But neither is the current structure the end of the relevant causal analysis.

>Withal, it is important I think to distinguish autonomy from
>independence. Independence is freedom from external constraint.
>Autonomy is control within one's own domain.

Some folks want a "transcendent" freedom that is MORE than just independence from external constraint. They argue that, given that nothing in your (their construct "your") niche is preventing you from killing somebody, it is your "free moral choice" to "decide" whether or not to kill, and some of them further argue that your "decision" on the matter is subject to the wraith of the supernatural. Again, I think they will find no support for this point of view in PCT.

A fine kettle of fish, indeed. I don't feel very hungry right now.

Greg

Date: Sat May 16, 1992 5:00 am PST
Subject: Re: standards

Dear Rick, FROM CHUCK TUCKER 920515

I just could not resist this: By what standards do you determine that you "like" Ed (or anyone for that matter)?

I think this is a problem too: This is no "good" reason that anyone should like anyone else but some of us do.

I think what we need is to submit these notions that we (or you) are putting in this model to THE TEST? Devise an experiment that might be able to determine that a person is using a standard with regard to another within the confines of a particular act (this, by the way, was the assignment to my class this Spring, no one was able to do it even after reading HPCT all semester).

I think we (really I) must remember that this model (HPCT or PCT) is one which is developed out of engineering and seems to apply quite well to artificial systems that can be build and with living control systems UP TO LEVEL THREE. There is some information from a variety of studies and other experiences that we who are using this model that it it useful for explaining or understanding or comprehending behaviors. DON'T MISUNDERSTAND ME - I FIRMLY BELIEVE THAT THIS MODEL WILL WORK BETTER THAN ANY ONE THAT I KNOW OF - BUT I REALIZE THAT IT STILL NEEDS EXTENSIVE TESTING WITH "THE TEST". I have not done the work that is required and I have not seen it done by anyone else BUT I believe it can and will be done.

Don't you agree? Best - Chuck

Date: Sat May 16, 1992 5:28 am PST
Subject: Re: standards

[From Rick Marken (920515 14:00)

Chuck Tucker (920515) says:

>I just could not resist this: By what standards do you determine that you
>"like" Ed (or anyone for that matter)?

I don't know. But the fact that we like and dislike anything suggests that control is going on. I think it is very difficult to verbally describe all the perceptual variables that are involved in "liking a person"; let alone the reference levels (standards) for those variables. If you mean what principle perceptions do I have relative to Ed that I feel are close to my currently prevailing refrences for those principles (as I sit here at the keyboard and try to describe them) then I think of things like "family" -- I like the principles I perceive as exemplified in Ed's relationship to his family. I like the principles I perceive in Ed'd interest in and

understanding of PCT. Again, these are just words; you get a better sense of what I'm talking about if you could have my perceptions and my references for those perceptions. Short of that you could do an informal "test" to see what level of the "family" principle I like to perecive, eg. by describing people who exemplify different levels of that principle. I admit, for example, that my "liking" for JFK went way down when I heard that he was regularly unfaithful to his wife. You would have to do a lot of testing to figure out what principle(s) were violated for me -- for example, my liking for JFK would go right back up if I found that this behavior was done with his wife's consent. My own impression is that what is violated (for me) by the JFK infidelity is a reference level for a particular perception of "respect for other people" and not a reference level for "who to sleep with when you're married". But there was some reference for a "standard" (can't we call it a principle -- like it was originally called; this, use of "standard" really confuses me because it sounds like reference level) that was violated.

>I think this is a problem too: This is no "good" reason that anyone should >like anyone else but some of us do.

I think the only reason we do anything is to keep all our perceptions matching their references. Whether that is a good reason or not -- I don't know.

>I think what we need is to submit these notions that we (or you) are putting >in this model to THE TEST?

Agreed. But it is very difficult to test for control of these higher order variables; we can barely describe them.

> Devise an experiment that might be able to determine that
> a person is using a standard with regard to another within the confines >of a particular act (this, by the way, was the assignment to my class this >Spring, no one was able to do it even after reading HPCT all semester).

This is very difficult -- especially if by standard you mean "principle". But it's pretty easy for many other variables. Try getting real close to a stranger; talking real loud during a conversation; use a lot of profanities (if you can -- some of these disturbances are hard to produce because it requires the "disturber" to set their own references to unacceptable levels-- for their own hierarchy). It's really not hard to see variables being controlled -- any time someone acts like something is WRONG there is a perception that is deviating from a reference -- but it's not always easy to name the variable.

I've found (for now) that the discovery of controlled variables is like a Zen exercise; don't try to name stuff; get those words out of your head for a while. Try to just look at the world as variable perceptions; arrangements of objects, relationships between them, etc. Watch how people seem to like certain states of these variables rather than others. Note that sometimes they seem content with things and other times they protest and complain; the protesting and complaining and the "fixing" and the doing are all evidences that something is not as it should be for a person. You just need sharp clinicians like Ed Ford and David Goldstein and the rest to figure out what those controlled perceptions might be.

One of the problems is that most of what people control is too obvious and

too "trivial" so it goes unnoticed. People are not generally running around trying to control the "meaning of life". They are moving things from here to there (moving themselves from here to there); carrying out programs, categorizing (its an "X" -- no, it's a "Y"). If you know someone who makes music (well) you might see if they can imitate the "style" of some well known artist; that's a pretty complex variable (I do a mean Bob Dylan). Control is all around -- maybe the problem is that it's too much around -- we take it for granted. Bill P once said that feedback is like the air we breath; I think this is true of control too. Because it is everywhere, it is invisible, unless you know what to look for (like the answer that's "a blowin' in the wind").

Hasta Luego Rick

Date: Sat May 16, 1992 5:32 am PST
Subject: What CT says about X

[Avery Andrews 920515.1432]
(Bill Powers (920514.0600)

> A Chomksyite evidently proposes "because I can perceive a certain
>structure of relationships in language, that structure produces language."
>The question is thus, "what does control theory have to say about the way
>structure produces language?"

I wouldn't really say this, but that something is producing the structure, and knowing a reasonable amount about the structure ought to help in identifying the something. Actually, in the case of grammatical generations, I suspect that there really are things corresponding to them (ie., for the generalizations that lead people to postulate noun-phrases, there is a noun-phrase detector). I suspect this because grammar does not seem to be real-world interactive in the manner that most things studied in psychology are, so the structures in grammar can't be coming from simple interactions with a complex environment. But that could all be wrong. The real purpose of getting an organized view of grammatical regularities is to get constraints on possible theories of the mechanisms that are actually involved.

Avery.Andrews@anu.edu.au
(currently andrews@csli.stanford.edu)

Date: Sat May 16, 1992 5:57 am PST
Subject: Re: concurrent control in language

Bruce says that

<more than one from of behavioral outputs can accomplish the same
<controlled perception

in speech and some other social/psychological behaviors. This is indeed one of the ways language use differs from many of the CSG behaviors which I've seen demonstrated. Another way that it differs is that at some levels of language use, such as decisions about wording, the target perception itself is neither particularly quantifiable (e.g., I want my hearers/readers to think I am witty) nor is it simple (e.g., I want my hearers/readers to think a) I am

witty, b) I know what I am talking about, c) see the relevance of what I am saying to their own perceived needs, d) think my wording and grammar are acceptable, e) see me as a colleague, or whatever). Somewhow, as we speak or write -- and perhaps when we read and listen -- we edit for the differences between our perceived goals and our perceived (and contructed) meaning associated with the words we are perceiving.

When I think about the levels of control in CSG as well as the configuration of the perceptions, feedback, and other variables, using control theory makes sense as long as I think of it as a metaphor or a verbal model. But when I try to think of all of the variables that I'd have to quantify in order to test the model at a level of granularity similar to the little man (or the baby) I lose control. :->

Perhaps this is an arena where both observations of individual language behaviors (in social settings, pairs, reading-to-write scenarios, etc) and some basic research on what possible variables their are (e.g., standards commonly applied to writing, kinds of choices possible, etc) would make sense. This could well mean applying statistical analysis to observations to find out the range and variation of choices as well as the relative influence each kind of variable seems to have on the language behavior -- and how this influence varies from one indiviudal to another -- before we can try out a formal CSG model with its hierarchy.

What do you think?

Cindy Cochran
Dept. of English
University of Illinois

Date: Sat May 16, 1992 5:59 am PST
Subject: History, Determinism & Butteflies

[from Gary Cziko (920515.2115):

Greg Williams (920515) says:

>My central claim is that the moment-to-moment operation of an individual's
>control structure is NOT MIRACULOUS, but rather a function of its past history
>(perhaps in a probabilistic way -- I have no desire to get into questions
>about "absolute" determinism).

Who could argue with this? How could an individual's control structure be anything but a "function of its past history?"

But what kind of function is it? You've raised the possibility that it is a probabilistic function. But what about a chaotic function? I find chaotic systems particularly intriguing in this respect because they are completely deterministic, and yet in a sense curiously independent of past history since after a while the initial starting point has no bearing on the present state of the system.

A chaotic perspective (with sensitive dependence on initial conditions, a.k.a, the butterfly effect) on the reorganization of control structures would suggest that two indentical (as far as anyone could tell) individuals

placed in identical environments could turn out very differently as the result of reorganization. This is similar to Gould's (_Wonderful Life_) notion of _contingency_ in biological evolution. Play the evolutionary videotape back, he says, and let it run again and you will a very different cast of characters, very likely one in which Homo sapiens has no place.

I don't know what all this means for free will, but at the least it suggests that it wouldn't take much at all to kick things in a new direction. History makes sense after the fact. But makes lousy predictions before. --Gary

P.S. Greg, did I ever send you the Mackay argument for "free will in a mechanistic universe" (of something like that). If so, what do you think of it?

Date: Sat May 16, 1992 8:09 am PST
Subject: Various comments

[from Mary Powers 9205.12,13,14,15,etc.]

Gary:

It's on it's way. We have a box of them. Can we have a copy of your paper? I think Bill and I each thought the other had asked.

Wayne:

???
>Do we experience the energy or the information, or neither?

We experience our perceptions. E and I are high-level, Western, modern constructs. So are time and space. And phenomena. And the relationships between them all.

David:

If a person wants to keep talking about a topic (when you're trying to get him to go up a level) it is probably not "simply wanting to talk about what we are now talking about" - it is itself the resistance. The point is that someone sticks at a level and can't or won't go up because of conflict - unawaredly having at that same level the opposite desire or point of view. When in therapy, i.e. talking in a safe place to someone he can trust, he can allow himself to entertain that conflicting thought, and THEN go up a level and realize what's been going on. It's not splitting attention between lower and higher levels, it's the difficulty of allowing one's attention at the lower level to go to the not-acceptable other side of the conflict that has one stuck at the lower level in the first place. It is very hard to let go of an organization that more or less works, though painful, and to acknowledge the greater pain that inspired that organization originally, and will be experienced again when reorganizing. No matter what kind of mess a person is, he is still a "me", and reorganization and resolution of conflict is in the direction of "not me" (or so it seems, though once one gets there one is still very much "me").

yours of 5/15/92

From many things I've read, the old person probably knows he has cancer or something fatal and doesn't want to end his life as a tool in the hands of the medical-industrial complex. What you need to work on is the children - their reluctance to face the idea that their father is going to die. The prospect of his death is probably a bigger error signal for them than it is for him.

Francisco Arocha:

This goes back to a passing comment you made three or four weeks ago about the "anything goes" philosophy of Kuhn, etc. What he said was that scientific progress can't be judged as approaching closer to the Truth, and that various earlier versions of science continue to be used and are perfectly valid (earth-centered astronomy for surveying, heliocentric for launching spacecraft, etc).

The idea that science evolves towards the Truth is the same notion that pervaded evolution: that life evolved from "lower" to "higher" forms (the highest being us, naturally), presumably more and more perfect (though anyone with back trouble from the upright posture, or anyone who has given birth to a huge-headed baby, might have some doubts). Evolution has been FROM simple to more complex, but is not headed towards any external goal - it is a consequence of internal goals of living things - getting enough to eat, avoiding predators, being attractive enough to find a mate and reproduce, etc. in a world of other living control systems trying to do the same thing and trying to keep even or one jump ahead.

Science, Kuhn says, has also evolved. But not towards the Truth, whatever that is. Its evolution is also FROM some prior state. And the criteria for valuing one scientific scheme over another are internal to (some) humans, and not external. That is what (I think) you construe as "anything goes". What's valuable is up to us, not to some cosmic principle.

Greg Williams (920513)

I think being "'slaves'" of our current control structures conjoined with current environmental disturbances" is a little peculiar. How can one be a "slave" of what one is? We don't HAVE control structures, we ARE them.

"Having" free will is how it feels to be a purposive, living control system.

We spend most of our time satisfying reference signals and resisting disturbances we don't even notice - we breathe, we walk around, we cope with gravity. It's that constant experience from which we derive the sense of free will. If the environment is so constraining that one can't satisfy important reference signals, or if one is in conflict so that satisfying one increases error in another, then one can lose that sense. It may be an illusion, as you suggest, but given the complexities of the control hierarchy and of the environment, it's not likely that it's

possible to determine all the antecedents. Isn't that what chaos theory is all about?

About altering current reference signals. Lower level ones are altered all the time in order to achieve higher level perceptions. Or so the theory goes. And higher level ones can be changed too. It just doesn't seem like "you" are doing it, because consciousness doesn't go up that far. For whatever reason - to protect the stability of the organization?

Avery Andrews

We are at 73 Ridge Place, CR 510 (that's county road) Durango, CO 81301. Send it along.

This is also the business address of the CSG. Keep those conference registrations coming!

Rick, Chuck (9205.15)

Why do we like people, indeed. The real issue to me is why we dislike people. This winter I read Eduardo Galeano's Memory of Fire - a three volume history of the Americas, mainly South and Central (a must read in this half-millenium year of 1992). Pretty brutal. The point here is that it seems "natural" for humans to dislike, fear, and consider subhuman people who are strangers, or different. Often people will like an individual they "get to know" (share reference levels with) and yet continue to dislike other people of the category (Black, Jewish, whatever). We do need to like and be liked, but what about this other reference level?

Mary Powers

Date: Sat May 16, 1992 8:16 am PST
Subject: Language, models, misc

[From Bill Powers (920515.2000)]

Avery (& anyone else):

My mailing address is
73 Ridge Place, CR 510 (that's "County Road 510")
Durango, CO 81301

David Goldstein (920515)

Ann Landers has said, and I agree, that people usually are wrong when they conceal a patient's true condition from the patient. They're really controlling for their own feelings, not those of the other person.

If the older person has gone through some extensive medical testing, it must be clear that there was a reason for it. I don't think it would be too difficult to find out if the person wants to know the bad news. Ask the person. "Do you think anything really serious might be wrong with you?" That ought to be enough of a disturbance to elicit denials if the person

doesn't want to hear about anything bad. A more likely reply would be "I don't know -- nobody around here will tell me a damn thing."

Does anyone really think he or she could visit a beloved relative in a hospital and conceal the knowledge that this relative is dying? Denial exists on both sides of the question.

Greg Williams (920515a) --

RE: Fine kettle of fish..

Good point about the adjunct to HPCT in the form of proposals about free will. HPCT is about control, nothing else. It's not even about consciousness or awareness, much less "volition" in the usual sense. Any remarks I've made on such subjects have come from an attempt to understand personal experience in ways that HPCT doesn't help with. Of course knowing that the vehicle is a hierarchy of control does make more sense of some experiences.

>According to PCT, we are CYBERNETIC beings, behaving/acting in certain >ways because of the history of each of our control structures IN >INTERACTION WITH each of our environments (or more exactly, those >portions of our environments which affect our individual control >structures -- what Maturana terms our "niches").

We are also reorganizing beings. Reorganization is fundamentally a cybernetic process, of course, but it isn't necessarily totally "random" or "statistical" in nature. All that's meant by saying that a process is random is that we know of no algorithm that will predict what it will do next. A random reorganizing system is powerful because it doesn't take anything for granted. But reorganization could be quite systematic in some way that is too subtle, or too advanced, for us to find order in it.

If determinism (of any sort) is an article of faith, then the idea that there may be system behind our own reorganizations will cut no ice: one will say, "Well, whatever that system may be, it must be deterministic and result from interactions between organism and environment." But if one is open-minded, it's not hard to entertain the possibility that reorganization may have a systematic but not deterministic component. After all, until you've found the deterministic links, all things remain possible -- unless you've ruled them out in advance.

I've now heard ouches from two owners of linguistic toes.

Bruce Nevin (920514) --

I said

>>If it seems that there is structure in language, then a model that >>explains this phenomenon should not contain that structure, but only >>components that lead to phenomena which can be seen as having that >>structure.

And you said

>If it seems that there are words in language, then a model that >explains this phenomenon should not contain words, but only components >that lead to phenomena which can be seen as being words.

-- and so on to word dependencies, dependencies on dependencies, etc.

This isn't quite the direction of my thought, although I can't guarantee that there is any direction to my thought. I think you're talking about phenomena, observed order, facts. I'm talking about explaining those phenomena by using a model.

In the case of words, I would wonder "What is a word? What is the system doing when it produces and hears or sees those things we call words?" When I examine words closely, they seem to be just familiar chunks of sound or objects on paper. The meanings they seem to have (for those words that can individually designate meanings) turn out to be ordinary perceptions. So both words and their meanings are ordinary perceptions. The model, therefore, would not explain words in terms of words, but in terms of the way one perception can be used to indicate another, regardless of the classification of the perception. The process of indication, evocation, association, or whatever you want to call it, is what needs modeling, because we already have a start on a model for how ordinary perceptions of different levels depend on other perceptions hierarchically.

When it comes to apparent word dependencies, I don't deny that such dependencies exist phenomenally. But I want to know why they exist and how they CAN exist, in terms of a model. The dependencies themselves are just observations and interpretations. Because words are just ordinary perceptions, the observed dependencies between words must also exist between perceptions of other kinds (although perhaps not exactly the same dependencies). Dependency itself, therefore, is what needs modeling, not any particular dependencies. When you understand dependency itself as a type of controlled perception, probably as an example of a larger type such as relationship or sequence, you will understand not only word dependencies but all kind of dependencies.

By remaining at the level of phenomena, we can only observe and record apparent cooccurrances and dependencies of particular things. We can't explain why they are related as they are. The method of modeling attempts to go beneath that level to the level of underlying operations, looking for operations that could produce both the phenomena and the observed dependencies among them.

Avery Andrews (920515)

I said
>> A Chomksyite evidently proposes "because I can perceive a certain
>>structure of relationships in language, that structure produces
>>language."

> I wouldn't really say this, but that something is producing the
>structure, and knowing a reasonable amount about the structure ought to
>help in identifying the something.

I'll give you the same comment I give to Bruce. The question from the standpoint of modeling is not WHAT structure is perceived in language, but WHAT PERCEIVES STRUCTURE ITSELF. Bruce (with Harris) proposes that the structure we perceive consists of words and their dependencies etc. You propose that the structure can be represented as a program, a network of contingencies. But structures can be of all kinds. What operation of the

brain underlies these symptoms we call structures? Surely, the ability to perceive structure, to alter one's actions so as to create and change structures that are perceived, is a more fundamental aspect of organization than is the ability to perceive and control for a particular example of structure.

>Actually, in the case of grammatical generations, I suspect that there >really are things corresponding to them (ie., for the generalizations >that lead people to postulate noun-phrases, there is a noun-phrase >detector).

You can think of a noun-phrase detector in several ways. The conventional way is to say that there really are such things as noun-phrases in some objective world, and that all we need to do is develop detectors that can respond to them. The CT way is to say that because we have developed perceptual systems that respond to things in terms of noun-phrases, we can create and control the occurrance of noun-phrases.

So in the CT model, a noun phrase exists because it is perceived and controlled; in the same collection of words, something else might have been perceived and controlled instead. But the CT model wouldn't specify noun-phrases: it would look for the kind of operation that underlies the perception of noun phrases; for example, the ability to recognize and reproduce sequences (of any kinds of perceptions).

Also I suggest again that if you can perceive a structure, it is that perception that is controlled by VARYING utterances. I know you're skeptical about the possibility of a closed-loop organization here, but of course if you don't try to invent one that would work, it will remain only a possibility. If, on the other hand, you could find one, that might be rather an important event in linguistics.

> I suspect this because grammar does not seem to be real-world >interactive in the manner that most things studied in psychology are, >so the structures in grammar can't be coming from simple interactions >with a complex environment.

But grammar is real-world interactive during the time it develops. No skill, after it becomes habitual, is real-world interactive to the same extent it is while it's being learned. We reduce principles and reasoning to slogans at the drop of a hat. We reduce slogans to slurred and run-together events even more readily: ISWEARTOTELLLTHETHRUT HANDNOTHINGBUTTHETRUTHSOHELPMEGOD. By the time we're adults, "grammar" is just how we say things; most people say the same things the same way every time, without considering whether it's grammatical or not. When you lose the real-world interactiveness of grammar, you've also lost grammar. If you hear "Every man for itself," the "itself" isn't grammatically wrong, it's wrong because you said the memorized phrase wrong; you made the wrong sound at the end. We no longer think of "Every man for himself" as a sentence in which the words have individual meaning, sexist or otherwise. It's just something you emit under certain circumstances. It's like a shaped grunt.

This is probably a good answer to your objection about treating rules as controlled perceptions. As long as there is a possibility of making mistakes, the rules are perceived and the utterances are adjusted until the right rules are perceived. But once a person has settled on utterances that will reliably fit the rules, the utterances are reduced to phrases and no

longer are treated as having internal structure. Then those utterances are no longer rule-driven. They're just the way you talk.

This would apply to the Harris approach, too.

Cindy Cochrane (920515) --

(Bruce)
<more than one from of behavioral outputs can accomplish the same
<controlled perception

(you)
>in speech and some other social/psychological behaviors. This is
>indeed one of the ways language use differs from many of the CSG
>behaviors which I've seen demonstrated.

A basic concept of CT is that you VARY your actions in order to keep producing the SAME perceived result. You have to do this because the environment keeps changing (and your own actions keep changing) and disturbing the controlled result.

For practical reasons, the disturbances we've been using in experiments are such as to require changing the AMOUNT or DIRECTION of action but not the KIND. So if a disturbance alters the cursor position in a tracking experiment, the handle has to be moved to a different place to keep the cursor in the same place, but you don't have to drop the handle and pick up a microphone or a hammer.

In general, higher systems can control their perceptions not only by varying the amount of reference signal sent to a given set of lower systems, but by changing WHICH LOWER SYSTEMS are provided with reference signals by that higher system. This is changing the kind of behavioral action rather than just the amount. This is tricky to implement in a model when you don't already have a lot of control systems available in the model, which is about where we are.

>Another way that it differs is that at some levels of language use,
>such as decisions about wording, the target perception itself is
>neither particularly quantifiable (e.g., I want my hearers/readers to
>think I am witty) nor is it s simple (e.g., I want my hearers/readers
>to think a) I am witty, b) I know what I am talking about, c) see the
>rlevance of what I am saying to their own perceived needs, d) think my
>wording and grammar are acceptable, e) see me as a colleague, or whatever).

I disagree along two dimensions. First, "witty" is certainly quantifiable by the person doing the controlling and perceiving. Some sayings are wittier than others. Some sayings intended to be witty leave you with a red face because they fall so flat. Wittiness is a perception that varies along a scale from zero to hilarious. It's your call as to where the wittiness falls on this scale, but that's because it's your perception under your control (subject to disturbance).

Second, the HPCT model is anything but "simple." You can have a given act serving many different goals at once; a given goal being satisfied by a changing mix of different lower-level actions. You can have many different goals being served by many different actions, no one of which is under the

exclusive jurisdiction of any one higher-level system. The HPCT model is just about as complex as real people are. Or it's intended to be so.

> Somehow, as we speak or write -- and perhaps when we read and listen
>-- we edit for the differences between our perceived goals and our
>perceived (and contructed) meaning associated with the words we are
>perceiving.

(I edited "Somewho", illustrating that we also edit as we read). I agree with this concept: we're varying our actions to reduce the difference between the words we emit and the meanings we intend them to have until the difference is as small as we can make it. That's the basic CT picture of language production in the domain of meaning.

>when I try to think of all of the variables that I'd have to quantify
>in order to test the model at a level of granularity similar to the
>little man (or the baby) I lose control. :->

```
                                          ^^
Me too. But Rome wasn't built in a day.   OO
                                        (----)
```

As to your suggestion about basic research, I applaud. That's exactly what's needed. The problem, however, is to keep the standards high enough to get what I would admit to be data. Traditional statistical analysis is based on very low standards of acceptance and extremely noisy data. I would rather see less data and higher standards: say, correlations above 0.95 and $p < 0.000001$. This should reduce the literature to a readable size and make its contents worth reading.

I don't think you can get data like that without using a good model. The model implied by most statistical analyses is that behavior is a linear function of inputs: $y = ax + b$. That model is so wrong that the data are very bad.

Penni Sibun (920512) --

RE: looking for ways people correct errors.

>the way to study this is to get lotsa data and look for patterns. you
>can't claim to know what's going on in any particular case, but out of
>the regularities you can develop a set of hypotheses about types of
>errors and conditions under which they occur.

I don't like this approach unless it's done under the right model and with great awareness of the pitfalls of mass studies. If you find that EVERYONE corrects a certain kind of error, that's one thing. But if you find that 80 percent correct it and 20 percent don't, you don't have a scientific fact, because you can't explain why the 20 percent don't. As most data obtained with statistical studies leaves large amounts of unexplained behavior, the custom has grown up of ignoring the counterexamples, and saying "people correct this kind of error" when what you mean is "some people correct this kind of error and some don't." This custom is the reason for the lack of progress generally in the behavioral sciences. Hypotheses are accepted when they ought to be rejected. The variance is blamed on the innate cussedness of organisms rather than on the use of an inadequate model.

>also, because language use just isn't a thing-in-isolation; it's a
>social phenomenon, so you're missing the same things the gb'ers miss
>if youu focus on one person.

I don't like this either (I'm sure I would like you if we met, so don't take this personally). Social phenomena have to have a place to live, and where they live is in individuals. If you understand how each individual deals with the surrounding world (including the other people in it), you can deduce the social phenomena. If you study only mass phenomena, you can say something about similar-sized masses of people, but nothing useful about any particular person. See my article in Wayne Hershberger's _Volitional Action_, in which I show that a mass measure of the characteristics of a population of 4000 simulated people produces a relationship between two variables has to opposite slope to its true form in EVERY INDIVIDUAL.

Working with individuals takes a lot more time and trouble than doing surveys of thousands of people in parallel. But if you test a model over and over against individual behavior, you get a picture of the distribution of characteristics that doesn't generalize a slight preponderance to a universal. You don't throw out any exceptions to the model: you change the model. What you end up with is a hell of a lot more impressive than anything you can get out of any kind of mass measure.

Best to all, Bill P.

Date: Sat May 16, 1992 9:34 am PST
Subject: Little baby

[From: C. Love (920516.1200)]

[To: Joe Lubin (920515.1200)]
Thanks Joe for the suggestion about the multiple low-level iterations to simulate the "transport lag." I had thought about it, so it's nice to see some others have also considered it as something worthwhile. I will keep you posted on how things are going.

[To Rick Marken (920515 10:00)]
> This technique was used by Powers in his two level force control
>model described in his Byte magazine series (I think it was in the July,
>August or September issue, 1979) -- it was the third article in the series.
>It's an excellent article

Thanks Rick for your reference to Bill's work. When I started this "little" project, however, Martin was sure to provide me with much of Bill's work involving his little man project. Thus I have the Byte series articles as well as Bill's "lecture" notes for his proposed course outline. But it was a good reminder to go back and peruse back over it again.

>...this extra lower level calculation is *unnecessary* for the stability of a multi level hierarchy...

>...it's probably a good idea to *put them into* the models -- just for the >sake of realism.

From this, I assume you mean that *although* it is unnecessary, do it anyways? I suppose I must consider just how closely I want to follow biological equivalents.

In any case Rick, I do appreciate your comments and look forward to any others you wish to offer.

[To Bill Powers (920515.1000)]
> It looks as though you've done things with Little Baby a lot like I >have done them with Little Man. Looks good.
Thanks Bill. After I seen your demo, which Martin owns, I got interested in the ideas. The mechanics of my model are quite simplistic still, but I am hoping for better things.

Thanks for the tip on depth perception. I have a question though...

> A tip on depth perception. If the left eye and the right eye both see the
> fingertip and the target, the lateral separation between the two as seen by
> the two eyes will be different if the *distances* are different.

Is this *distance* lateral separation or as you say in the next line, angular separation?

>All angles are measured from a retinal reference point, say the center. The >angular separation of target and finger in the two eyes is...

I can use trigonometry to find the angles difference between the target and finger tip (angular separation) or use trigonometry to find the lateral (X separation between target and finger). The rest seems to make some sense. I'll think on it some more though.

I have been asking a fellow to bring in his vision book so I could look at some different ways of doing this but I haven't done so just yet. My approach was derived strictly from a intuitive and mathematical perspective.

>I think this is a more "physiological" way of perceiving depth than >computing sums of squares.
I knew that there would be better ones but I wanted to continue development for the time being so I used the sum of squares approach. If I don't find anything better in the vision text I will probably *borrow* your idea and implement it in my model. If I do find something better in the text, I will let pass it along. If we are to progress, ideas must be distributed, if feasible.

>...and I will shortly add the control systems to make each eye track the >target independently...
Just wondering Bill, is it physiological to have independent eye tracking? I mean I can't make my eyes do something independent with respect to siting a target (I don't think). When I follow a moving object with my eyes, both eyes follow it, it appears, in the same manner.

>...the distance at which the eyes are converged.

This "convergence depth" you speak of; is this where both eyes siting line intersect, with respect to depth?

>..."we will concentrate on variations that occur *slowly* enough that the >system and its environment never get too far from a steady state >relationship." [BYTE, 197907.P.138]

>The calculated [output] amount of change is multiplied by S2, a *slowing >factor*, and the result is added to the old value of O. We've put a low ->pass filter into the output function, without affecting the steady-state >proportionality constant. The same thing is done for the input fcn." >[BYTE 197907.P.142]

So this is how you account for the delay. Also, I see that you use multiple- level iterations to *smooth* the percolating upwards percept signals [as noted previously by Rick Marken (920515 10:00)]. Quote by Bill is,

>"The outer 2 loops cause the lower-level systems to iterate twice for every >iteration of the higher-level system. This proves to be exceedingly useful, >easy way to stabilize the 2-level system. I have no formal rationale for >why this works..." [BYTE 197908.P.111].

First of all Bill, did you simply discard the first of the two percept outputs or, like I suspect, average the two of them, and then send out this value every second iteration? If you did this then I may have a fair explanation for you...

I think this process serves the same purpose as momentum does to BP. If you watch the error function for BP without training, it is quite irrational; it's very noisy looking and takes much longer to become stabilized, but after adding momentum the error signal drops smoothly and quickly. In your case you are providing a *noise reduced* percept by providing the average of two samples. In the interim (odd interval out), I suppose you provided the previous averaged sample percept.

With the exception of the *slowing factor* you mentioned, I had intended to do what I have just explained. I think two or even a three sample running average would be sufficient. And this would occur between every successive layer. As for the I/O functions, I have though about using a bi -level sigmoid, i.e., centered at origin with range +- 0.5. I have to go back to my control notes to think about amplification issues and closed loop gain though; it's been a while....

What you said about the sequencing, I suppose you had already said in your article, (which I overlooked)

> "... beginning to see that one must view all the variables in a control >system as changing *together*, not one at a time." [BYTE 197907.P.142]

>So strictly speaking, you should evaluate the top level of perception first, >then the next lower, and so on, so every perceptual function responds to >the state of its inputs as of the start of the times slice (not the end of the >previous slice).

Ok, In terms of evaluations this is how it will *flow*. Thanks Bill.

>You can also use the Target position as the reference signal -- same
>difference. In fact you'll want to if you add any higher level system finger
>tip position, set by a higher system concerned with a finger-to-target
>relationship (which needn't be zero distance).

Right! I still think in a very coordinated system where there is always some
focus, i.e., reference point. The same applies here, just that the reference
point is dynamic! I like it anyways!

>A learning mode that can be used when map control is on, gradually
>making the kinesthetic space agree with the visual space.
Sounds like what I'm after!!!! I can't wait to see it!

Bill, do you use any kind of correlation (learning) between the percepts
coming into a given ECM and the output/reference signals coming out of
that ECM?

Well, it appears that I have some changes and modelling to get to on the
little baby. Maybe soon, we'll have a little family and the relatives from
Colorado can come on up to Canada for a visit sometime?!!

Take care all,
Thanks for your advice - it is always greatly appreciated.
Chris.

Date: Sat May 16, 1992 9:48 am PST
Subject: Speech recognition book

[From C.Love (920516.1230)]
[To: T. Cutmore (920514)]
Hi Tim,
Well there are lots of book out there. MIT Press put out a real nice spread
of NN/speech recognition book last fall at NIPS '91. I just couldn't afford them. Anyways, I like Tom Parson text on Speech and Voice Processing. The reference is, Voice and Speech Processing. New York, NY: McGraw-Hill, 1986, 402 pp. It is a basic book. It looks at speech processing from a number of different approaches including HMM, DP, etc. It also look at the issues of isolated and continuous speech recogniton, which are quite different in nature. It also look at linear predicitve coding, LPC, and includes some BASIC programs at the back (written format). I think it was meant as a semester course text since there are questions at the end of each chapter. I any one would like to throw in their *two cents* as to what they like. I, for one, would be interested in listening. Best regards,

Chris Love.

Date: Sat May 16, 1992 2:29 pm PST
Subject: hpct applied to group therapy

To: general CSGnet members
From: David Goldstein
Subject: group therapy based on HPCT

Date: 05/15/92

This is a resend of a message I sent on 5/10/92 plus a few additional thoughts.

I would appreciate some comments on how to apply HPCT to a group therapy situation with adolescents. I am taking a look at how groups are run at the Residential Treatment Center(RTC) where I have beem Clinical Director for the past almost two years. Each resident is offered at least one session of individual and one session of group therapy per week. Other than insisting that a group therapy session take place each week, I have left this aspect of treatment to the consulting clinical staff to do as they see fit. I am beginning to feel that I need to take a more active role in this area.

Some of the problems we are running into with our groups: The groups are often chaotic, more so on the boys side than the girls side. The residents are sometimes stirred up by the group discussion and act out afterwords. The residential living staff complain of having to deal with this. The residential living staff participate in the group therapy and the clinical staff feel powerless to influence how they participate. The worthwhileness of the group therapy is being called into question.

Some issues which occur to me:

What are the distinctive purposes of group therapy? Some of my own thoughts--

To sharpen observational skills as you see your peers interacting with each other.

The levels of perception can provide some observational categories.

Trying to improve skill at reading other people's intentions.

To improve communication skills as you verbally express yourself in the group.

Expressing your intentions clearly as well as other things you want to say.

Being aware of signs that the other person is understanding/not understanding of what you said or are doing.

To learn the method of levels.

To learn more about yourself from the reactions of other people to you.

Receiving comments about your actions and words in an open fashion.

Noticeing people in the group who are like you and unlike you in different ways.

To learn how to be emotionally supportive of and receive support from another person.

Be aware of signs that the other person is feeling better.

Letting other people know when you are feeling better.

To learn how to relate to someone without trying to control them or be controlled by them.

Being sensitive to when you are trying to control another person.

Being sensitive to when another person is trying to control you.

Knowing how to resolve conflicts.

How should the meeting being organized to reach these goals? Some of my own thoughts--

Some easy-to-understand summary of HPCT should be part of what happens. (For example: A person is always trying to get and keep what he/she wants as mucn as possible. A want refers to a desired experience. If the action being taken does not result in the desired experience, a person will change the action being taken so that what is experienced becomes more like the desired experience. Control refers to how closely the actual experience matches the desired experience. When the person is controlling perfectly, the actual and desired experience are the same. This is called zero error signal. The smaller the mismatch (error signal), the better the control is said to be.)

Some statements of the group self-image should be made. This involves stating the purpose of the group clearly.

Participation in the group should be voluntary after the resident has participated in the group for a while.

Some development of group norms(principle level perceptions) should take place.

I would really appreciate any comments you have about basing a group therapy on HPCT ideas. My plan is to write a curriculum for each of the points mentioned above. Regardless of the purpose of the group, I think that the above points should be addressed.

Thanks.

Date: Sat May 16, 1992 4:34 pm PST

Subject: exploring your levels

To: Rick Marken, Mary Powers, other interested CSGnet people
From: David Goldstein
Subject: exercises to help a person explore levels
Date: 05-16-92

Rick asked me if I knew of any exercise a person could use to help a person explore his/her own levels by means of the method of levels.

My first reaction was--No, the only way I know about is to enter into a conversation with another person who already knows how to do it. This is the way that I learned with Bill Powers help. Just like learning to relax with biofeedback equipment, the person learning the method of levels receives some questions/comments from the other person which helps him/her know if he/she is moving in the right direction.

Upon further reflection, I think that the Self-Image Exercise which I have developed may help a person explore his/her own levels. Recently, during one session, I gave a patient the Self-Image Exercise Sheet. The next session she came back with 11 exercise sheets completed. She shared with me her conclusions that as a result of the exercise she discovered three self-images. One she calls Mother, one Child, and one anti-Nancy. Nancy is the name of her mother. The last self-image was a surprise to her. The other two she has recognized from therapy discussions we have had.

I plan to take the information in the 11 exercise sheets and create a set of items which can be used in a Q methodology study with her. It will be interesting to see what results emerge from this study.

Here is the outline of the exercise sheet with one example from her:

Name:_____

Date: 5/9/92

Self-Image Statement: Seeking approval, wanting to please people and be assured of their approval, trying to avoid disapproval.

How Statement: Trying to anticipate what people want, repressing my own wants to avoid conflict, becoming invisible if necessary to avoid disapproval.

Why Statement: Disapproval equated with withholding of love in my childhood. Approval was rarely expressed by my parents, but they tolerated my presence as long as I behaved as they wanted. No attention is better than negative attention.

Contrast Statement: Not caring what other people(important people to me) think about what I do or say.

After Method of levels Discussion: I'm not as in touch with my own feelings and desires as I should be because I've repressed myself in an effort to gain love. This was extreme in my childhood, more moderate now.

Instructions for generating self-image statement: Imagine that you are talking to an actor/actress who will play you in a movie of your life. You are to give him/her general instructions on how to be/behave like you. The instructions can take one of two forms: Be the kind of person who _____. Don't be the kind of person who _____.

My thinking about this instruction is that it results in a person tuning into perceptions about him/herself approximately at the principle level of perceiving.

Instructions for how statement: Give some examples of how the actor/actress can be/behave like the person described in the self-image statement.

Instructions for why statemnt: Explain to the actor/actress what is accmoplished by being/behaving in the way described by the self-image statement.

Instructions for contrast statement: What is the opposite or contrast to being/behaving like the self-image statement.

Instructions for the after method of levels discussion statement: What is your personal opinion/reactions to being/behaving like the self-image statment? What is good and bad about behaving/being this way?

If any of the people on the CSGnet do this exericse, I would appreciate hearing from you about it.

Date: Sat May 16, 1992 6:44 pm PST
Subject: Correction

[From Rick Marken (920516)]

Bill Powers mentioned an article of his on the perils of statistical "modeling". That article is not in Hershberger's "Volitional Action" book (though everyone should read Bill's articles in that book). The article he was referring to is actually in the September/October 1990 issue of the journal "American Behavioral Scientist" (vol 34/number 1). It is called "Control theory and statistical generalizations". It is NOT recommended reading for those who trust the findings of conventional psychology. But it is fun for the rest of us.

Enjoy. Rick

Date: Sun May 17, 1992 1:07 am PST
Subject: clinical example--5

To: interested CSGnet members

From: David Goldstein
Subject: clinical example--5
Date: 5/16/92

I have had one more couple and individual session.

Couple session: The wife was not as angry. She explained that her anger level was decreasing. At the same time she was saying "vicious" things to him but did not feel she wanted to stop at this point. Much of the session involved discussing his parents. She is thinking of him as an abused child and this thought seems to help her forgive him somewhat. She is still vascillating between being angry and wanting him out and forgiving him and working on the relationship. We discussed their different reactions to an incident before they were married. His father asked if he wanted some money for the date. Before given the money, he had to screw in a light bulb. She regarded this as degrading and embarassing. He claims he did not. It was typical of the kind of thing his father would do.

One interesting thought: she married someone like her mother who she has many negative reactions to and he married someone like his father who he has many negative reactions to. Her mother was very demanding, sucked up everything from her but didn't give much back. He has described his wife as demanding, bossy and moody.

In the couples session, each described the incident which I referred to as "a turning point." This incident meant to her that he really regretted, felt sorry for his actions. This incident meant to him that he realized how sad and lonely and empty he would be if he lost his wife and children.

Individual session: He was feeling sad and depressed. Much of the session was spent talking about his wife's negative descriptions of members of his family. This lead to mostly talking about his father. He has learned how to handle his father. The picture of his father was: a big, tall man compared to the patient with a bad temper. The father was very anxious, showed many obsessive-compulsive traits, was very impulsive, and very controlling of others. He was physically abusive when angry. An incident was recalled when he was about 11 or 12: he father told him to go to the store and buy cigarettes. He told his father "no", he shouldn't smoke. His father chased him into the bathroom where the man locked the door. He remembers his father's fist coming through the door and unlocking it. Then he doesn't remember what happened except he was beaten up.

We also talked about his mother. The picture of the mother was: overly involved with her son with sexual overtones, was phycially abusive of him at times. There was an incident in which his mother threw a hammer at him which hit him in the head. There was an incident in which his mother smoked pot with him.
The patient has learned how to handle his father through talking and actions. When his father becomes abusive verbally, he hangs up the telephone or leaves his presence. The patient has distanced himself from his mother.

We went over the history of his relationship with his wife. At one point they had broken up and then later went back together. Each were very sexaully active with many different partners during the breakup.

He will not allow himself to think any positive thoughts about himself. He said he did find comfort in an idea I presented to him: Normal people have multiple self-images which are separately controlled for. This means that he is not all bad. We also examined in further detail the experience of lying on the bed, curled up in a fetal position and crying very hard and long. He had a sense of being alone, sad, in the dark, empty, not alive. I asked him who was making these observations. I suggested that this was an observer self which is above the various self-images.

I also suggested that his actions on this night might have been to relieve his depressed state which he had been in all winter. The fact that he was angry at his wife this particular night is also important I think. When this man was a teenager, he used to flagrantly do things in the house such as have sex, smoke pot, and drink even with his parents in the apartment. He would do it to defy his parents.

From all the discussion of his family, this man grew up in an emmeshed family in which the individual boundaries were violated often and he became used to it. I think this is why he did not become overtly upset during the lightbulb incident. This is a man who has a hard time recognizing when he is crossing boundaries. His wife's intuitions that his actions are somehow connected with his experience withing his family makes sense. He allows people to violate his boundaries, for example, this woman, the babysitter, violated his family boundaries in the beginning and even though he sensed it, sat back passively and allowed it to happen. Another example, there is an incident in which the bahysitter quieted the crying of their firstborn by putting the baby to her breast and allowing him to suck. The wife was shocked and horrified by this but he didn't support her that something was wrong about this picture.

This man has a hard time recognizing and dealing with negative emotional states. When he is depressed, he ignores it and eats, runs, has sex, or works hard to deal with it. When he is angry, he ignores it, withdraws from the person, and then acts out impulsively against the person. He has to learn other ways of dealing with his feelings and moods.

In summary, my thinking of this case so far is that this man has to learn to: (a) not allow other people to violate his boudaries with impunity and to learn when he is violating other people's boundaries. In HPCT terms, he has to become more sensitive when he is having error signals in his self-image systems., (b) sense and take actions when he is feeling sad and angry which are consistent with all his self-image systems., (c) develop an understanding of his different self-image systems all at once, from the observer self point of view which will serve the function of integrating the different self-images., (d) not treat

his wife like her mother treated her which is a big disturbance.

I have been really impressed with his wife. She is intelligent, attractive, and is usually on target with her descriptions of his faimily. His wife has to learn to: (a) not treat him the way his father treated him which is a big disturbance; this will require her to learn some things about herself., (b) make him aware of when he is violating boundaries, (c) make him aware of when he seems sad or angry, (d) be more self-disclosing about herself, for example, her hobbies., (e) deal with the big internal conflict about staying or leaving this marriage; I think she has decided to stay. She would really benefit from seeing someone herself but refuses. I can use the couples session to address some of her individual issues. I can also give her Ed Ford's book to read.

I think that this is a workable marriage. I think they still love each other. They are both committed to the family they have created together. Together, they have to work on the kinds of things I talked about when I wrote about HPCT applied to group therapy. Spending quality time together is the basic means to achieve these goals.

This will be the next to my last post on this clinical example. Hope you have enjoyed it. It is hard to apply HPCT to clinical cases but I think it is doable. In my last post, I will answer any questions which you may have about the case and will give my own reactions to the application I have made of HPCT to the case.

Date: Sun May 17, 1992 11:34 am PST
Subject: PCT & Corrections

from Ed Ford (920517:12:35)

To All..

The other night I had my meeting of local control theorists who are trying to implement these ideas in their jobs. Alan Wright (he attended our last conference and should be at the next one) was recently appointed superintendent of schools for the Arizona Department of Juvenile Corrections. He has been working night and day at the two major lockup institutions, where the toughest juveniles in the state are sent, trying to implement a new program, using PCT as the basis.

In the past, diagnostic teams have decided what the juvenile would be doing and establishing their plans. Juveniles were staffed monthly and told what they were doing wrong. Juveniles never sensed any control as to when they'd get out. It was always kind of vague. All they learned from the staff was how to be critized.

Now, things have changed,
thanks to Alan. First, there are those who know they are getting out (like at age 18) or are going to be transferred to an adult prison. These could care less and continually cause trouble. They have been separated from the rest of the population and are in highly restricted and supervised units.

But for the others, things have changed. Alan and I have been working on the practical applications of PCT to this kind of setting for several years. At Adobe Mountain (the toughest) the juveniles had taken over the place. Alan really tightened the place down. Then each juvenile was asked as they entered the institution what they wanted. The universal answer was to get out.

He'd then ask them what they had to do to get out. He'd explain to those who didn't know. To the acting out juveniles, he'd say "is what you're doing getting you what you want, which is to get out?" Alan has the juveniles working in small teams of 36 with three teachers, each teacher directly responsible for 12 juveniles. The job of the teacher is to help the juvenile work toward getting out, which translates into getting certain tasks done in school and following the standards and rules in the classroom.

The old idea of being in so long (like six months) and then being released has been replaced by the requirement to get the signoff (approval) of each of the juvenile's direct supervisors in education at the school, and the line officer and case manager where he lives, and the person in charge of activity (work or recreation).

Everyone one has to sign off saying he is following the rules and working to his best ability and accomplishing his tasks. Any time they act out and are sent to lock up or an intensive treatment unit for more acting out juveniles, that time doesn't count against their credit for getting out of the facility.

Time is no longer important. Only achieving tasks that reflect increased responsibility will get them out of there. The juvenile is given total control over when he gets out of the facility. He has to accomplish certain goals but he alone has control over how quickly he can get released. Obviously, the more violent the effender's crime, the more responsibility has to be shown over a greater period of time.

It's amazing how the place has settled down. And, it's amazing how quickly acting out juveniles settle down once they learn they have control over when they get out.

Although to you freedom loving control systems on the net, this might not sound like PCT, but within the reality of the juvenile correctional system, asking them what they want and giving them control as to how long they are in a treatment center they don't like has given them a sense of control over their destiny they've never had in the past.

There seems to be less violent and more thoughtful reorganization going on. When they do act out, the supervisors just asked, "Is what you're doing getting you out of here?" or "Do you still want to work at getting out of here?"

Greg - would suggest Standards as one of the topics for the next Closed Loop.

Rick - Thanks for the comments. I'd like to get together with you at the conference and learn a little more about how your spreadsheet works.

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

Date: Sun May 17, 1992 11:40 am PST
Subject: all is all

[From Wayne Hershberger 920516]

(Mary Powers 920516)
>Do we experience the energy or the information, or neither (Wayne asks)?
>We experience our perceptions. E and I are high-level, Western,
>modern constructs. So are time and space. And phenomena. And the
>relationships between them all.

E and I ARE high level constructs, and so is P! As in, "the control of perception."

I believe we would do well to recognize the subtle but terribly important differences between the epistemological implications of control theory and the views of Bishop George Berkeley who claimed that, "to be, is to be perceived" (i.e., "all is perception"). Berkeley was arguing that, of Locke's two substances (mind and matter), only the former exists (i.e., there are no objects, only percepts). That is, Berkeley was claiming that there is no warrant for the existence of Locke's material substance. And Berkeley appears to have been right. But David Hume, extending Berkeley's argument, showed that neither is there any warrant for mental substance, either. It was Kant who later explained that the mind-matter issue is phony epistemology in the first place. Kant noted that we hopelessly PREJUDICE our epistemological deliberations whenever we label APPEARANCES as perceptions (in the mind or brain) and REALITY as material objects or God (or Boss Reality), as had John Locke and Bishop Berkeley. Rather, since we are not supposed to put our conclusions where our premises belong, we must name the two realms (appearances and reality) with neutral terms: phenomena and noumena (i.e., phenomenal appearances and noumenal reality). NOW, what is the nature of these phenomena, these appearances that comprise experience? To what extent are they mental, physical, subjective objective, etc., etc.?

Incidentally, Kant observed that time and space are a priori intuitions that are propaedeutic to recognizing such simple constructs as before/after, or over/under, neither esoteric nor high tech--more, Sesame-Street level.

Warm regards, Wayne

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

Date: Sun May 17, 1992 1:30 pm PST
Subject: modeling

[From Rick Marken (920517)]

Chris Love (920516) asks:

> From this, I assume you mean that *although* it is unnecessary, do it
> anyways? I suppose I must consider just how closely I want to follow
> biological equivalents.

Chris is asking about the necessity of implementing extra low-level loops in a hierarchical control model in order to simulate transport lags.

My goal (in modeling) is to mimic real behavior. It turns out that in the situations where I've used a hierarchical model to simulate behavior obtained in an experiment, no transport lag way necessary -- in the sense that it wasn't needed to make the model work and it made no difference in terms of improving the fit of the model to the data. The later was true only because the experimental situation did not make it easy to detect any benefit from adding transport lags to the model. The model behavior correlated with subject behavior at the .99 level. Adding the transport lag just made no noticeable improvement IN THAT SITUATION. By accident, I discovered an experimental situation that does reveal the fact that human control systems have transport lags. I have described the situation on the net before; the subject does a tracking task with a low gain control system's output acting as the disturbance. Then the subject repeats the task with a replay of the disturbance that had been generated (live) by the opposing control system. The time waveform of the disturbance is the same in both cases -- but the subject always controls better in the first situaiton (with the active disturbance). I was surprised by this finding -- especially because I found that a control model (unlike the subjects) always did exactly the same in both situations (as I had expected the subect to do). It looked like a real problem for the control model. Fortunately (if you like PCT) Bill Powers discovered the answer. You must add a tranport lag to the model (200 msec, I think) replicated the subject data exactly. So in this experimental situation (active vs passive disturbance with same temporal waveform) the transport lag shows up. In most continuous control situations it doesn't.

So, whether or not you put in the transport lag depends on the goals of your modeling efforts. I think the most important goal of all modeling (in psychology) is to build a model that behaves quatitatively like a living system. I think the model should also be true to what we know of the physiology; but not be constrained by it (physiologists can be wrong,too) or pushed by it (so that a lot of unnecessary detail is added before it is needed to make the model work -- for example, I don't think it is necessary to have my models actual generate spikes at varying rates; I just use numbers to represent instantaneous neural firing rate; the fact that this is a simplification may become important when you get into modeling aspects of behavior that might actually depend on the fact that there is a time period between one spike and

another; but right now, for me, it's an unnecessary detail).

I look forward to hearing about your progress on the little baby;
I wish I had the guts to try such an ambitions project. But, as
you can see, I'm happy to kibbitz (that's an english word by now, no?).

Regards Rick

Date: Sun May 17, 1992 2:48 pm PST
Subject: Heims book

From Ken Hacker, May 17, 92:

Right On! to Bruce Nevin's comment about how leaving out accounts of social comformity is not talking about people. Great point for thought.

In the April issue of Technology Review, there is a book review about the book called The Cybernetics Group, written by physicist Steven Heims. Has anyone hear read it and have an opinion about whether it should go on one's CSG reading list?

Thanks. Ken Hacker

Date: Sun May 17, 1992 4:20 pm PST
Subject: Re: Heims book

Heims appeared at the 1991 ASC conference in Amherst on a panel about his book, among other things. He was praised by the likes of Stafford Beer and others. Although I have yet to purchase the book, I plan to. I did go to lunch with Heims and greatly enjoyed our conversation. He has a keen insight into the early cybernetic thinkers, on whose work Powers has built.

O----------
>
| Cliff Joslyn, Cybernetician at Large, 327 Spring St #2 Portland ME 04102 USA
| Systems Science, SUNY Binghamton NASA Goddard Space Flight Center
| cjoslyn@bingsuns.cc.binghamton.edu joslyn@kong.gsfc.nasa.gov
| ^^^^^^ !NOTE NEW EMAIL! ^^^^^^
V All the world is biscuit shaped. . .

Date: Sun May 17, 1992 6:16 pm PST
Subject: Chaotic Freedoms (To & From)

From Greg Williams (920517)

>Gary Cziko (920515.2115)

>Greg Williams (920515) says:

>>My central claim is that the moment-to-moment operation of an individual's
>>control structure is NOT MIRACULOUS, but rather a function of its past history
>>(perhaps in a probabilistic way -- I have no desire to get into questions

>>about "absolute" determinism).

>Who could argue with this? How could an individual's control structure be
>anything but a "function of its past history?"

Anyone who buys into state-determined dynamical models won't argue. Some who don't, will. Some of the latter postulate "miraculous" (non-history-determined) alterations in organismic physiology due to a "self" and/or "God," among other (non-physical?) things. I was just trying to make sure that everyone realizes that such postulations are ADJUNCTS to PCT.

>But what kind of function is it? You've raised the possibility that it is
>a probabilistic function. But what about a chaotic function? I find
>chaotic systems particularly intriguing in this respect because they are
>completely deterministic, and yet in a sense curiously independent of past
>history since after a while the initial starting point has no bearing on
>the present state of the system.

You raise an important point here -- that even though PCT gives no support to the notion of a transcendental "self" capable of (the traditional sort of) "free will" (since an individual's current behavior/acts are due to his/her current control structure, which is a function of the individual's history), the theory also notes, in opposition to Skinner and cohorts, that (in general) SIGNIFICANT contributions to that history are due BOTH to external AND internal events. And even without chaos, the inaccessibility of internal events to external observers makes Skinner's optimism about prediction of (and deliberate precise influence of -- what he termed "control" of) the behavior of OTHER organisms seem greatly exaggerated. Chaos might make Skinner seem VASTLY overoptimistic, but that's just icing on the cake.

So, one can use PCT to argue against the feasibility of one's successfully being DELIBERATELY influenced along certain lines by others. The would-be manipulators will have a tough time because they have access mainly to EXTERNAL events. Nevertheless, if the would-be manipulators have SOME access to one's control structure (maybe just by asking questions of or "getting to know" the person or, at a more sophisticated level, by employing tests for controlled variables), deliberate influence might be considerably more successful.

>P.S. Greg, did I ever send you the Mackay argument for "free will in a
>mechanistic universe" (of something like that). If so, what do you think
>of it?

I did look at it and concluded that the argument hinged on an idiosyncratic notion of free will, namely (if I recall correctly) choice made under conditions of incomplete information relevant to the choice. With regard to the traditional philosophical debates about free will, I think many would grant that MacKay's kind of "free will" exists, but that it is NOT traditional free will (which is NOT "having to guess"). Having a sort of roulette wheel in your head to help make choices does not count as having free will in the Western religious tradition. Being a state-determined dynamical system (even one with probabilistic rules connecting successive states, or a chaotic one) counts as NOT having free will, traditionally. The crux is the notion of a "free agent" which "transcends" (is capable of breaking the rules of) physics (state-determination) by making "absolutely self-willed" (and thereby morally culpable) choices.

It might be enlightening to think about modeling the illusion of (traditional) free will. Dick Robertson has made a start on this in the last (I think) chapter in INTRODUCTION TO MODERN PSYCHOLOGY: THE CONTROL-THEORY VIEW. I doubt that cockroaches harbor the illusion, so I doubt that free will is how it feels to be ANY kind of living control system; I wonder whether monkeys think they can arbitrarily decide whether to move their little finger?

Greg

Date: Sun May 17, 1992 7:01 pm PST
Subject: Vacation

Mary and I will be off Monday morning for a little camping trip around nearby parts of Utah (90 minutes away). Capitol Reef, Bryce, Zion, and home. So if somebody asks me a question and I don't answer, don't worry.

Best Bill P.

Date: Sun May 17, 1992 7:57 pm PST
Subject: Comic relief

[From Rick Marken (920517b)]

Have a nice trip, Bill and Mary.

I will be traveling a bit this week, too, but I do want to get back into the "standards" fray when I have time. Until then, I will try to lighten things up a bit by describing a Gary Larson cartoon that my wife showed me today that seems rather relevant to that conversation:

Picture two strange-looking scientist types looking at some equations on the blackboard. One looks like me, the other looks like Dag. The one who looks like me is pointing at an equation on the board excitedly and saying to the one who looks like Dag:

" Yes, yes, I KNOW that, Sidney...EVERYbody knows that!... But look: Four wrongs SQUARED, minus two wrongs to the fourth power, divided by this formula, DO make a right."

Hasta manana Rick

Date May 18, 1992 4:49 am PST

Date: Mon May 18, 1992 6:24 am PST
Subject: Transport lag modelling

[From: Chris Love (920518.0845)]
[To: Rick Marken (920517)]

Thanks for the quick response Rick. Concerning your views on modelling...

>I think the most important goal of all modeling (in psychology) is to build a
>model that behaves quantitatively like a living system. I think the model

> should also be true to what we know of the physiology; but not be
> constrained by it (physiologists can be wrong,too) or pushed by it (so that
> a lot of unnecessary detail is added before it is needed to make the model
> work.

We believe the same in engineering. In most cases we just want to get it to work first, and worry about whether it's representation fits "the real world" later. But since this area is somewhat new to me, I wanted to get some feelings on how important it was to follow biologically motivated models. Thus, I will use my version of *transport lag* at each level because it is not difficult to implement and it appears to be useful. This means a running average of the signals. I suspect that the difference between two consecutive percept (or reference) signals will not be large (I will have to verify this though). If it is not overly large then the idea of a running average will sufficient to simulate the transport lag. From what Bill said,

[From: Bill Powers (920515.1000)]
> An integral-output model (like yours) is required to compensate for
> transport lags, implicit in the computing cycles and the definition of it
The
> best way would be to do exactly simultaneous calculations, and introduce
> transport lags as shift registers so you can specify the exact lag at each
> level.

From this I understand Bill to mean, - provide the exact outputs with a delay of *n*. I think that the difference in output between, say, *n* and *n+1* will be so negligible that a running average will be sufficient to provide similar behaviour. What do]ou think Rick?

Well, unfortunately today I have to work on my literature survey. This is the other part of my contract with Martin. Actually, it is the *primary* component; this software development is secondary. I personally like working on the software more; it's more fun!
That's all for now,
Take care Rick, Chris

Date: Mon May 18, 1992 10:37 am PST
Subject: Re: A Fine Kettle of Fish

FROM: Gene Boggess (920518)

In reference to: Greg Williams's post of (920515) concerning free will

It seems to me that the discussion of free will is ignoring a distinction that might help clarify things a bit (although perhaps not - who knows?). Basically, I think we all agree that we don't live in (Boss, big-r) Reality; we each live in our own (little-r) reality (because it's all perception, and it's all contextual). And, in reality, we all do have free will because it seems to us that we do (it's a top-level system concept); our attempts (when we are conscious of them) to control our perceptions seem to us to emanate from our self (another top-level concept). It may be that in Reality our actions are deterministically produced, but in the only reality we can know, they are not.

In another post, someone asked whether PCT implied any absolute standards, implying, I believe, that all absolute standards are forbidden by the nature of PCT. However, I think there ARE some absolute standards implied by PCT:

Given that: (1) you cannot control another person (except by force); (2) you cannot, predictably and deterministically, change another person's control systems; and (3) because it's all contextual and it's all perception, we can't be sure that our standards and lower-level reference signals will be appropriate for anyone else, THEREFORE no one should try to impose his or her standards/reference signals on others.

Given that: (1) control systems in good control exist in equilibrium with their environment, and (2) the environment almost always includes other control systems (other people), THEREFORE, to enable people to live with one another with the least conflict, we should try to provide others with the maximum opportunity to develop their own effective control systems.

Given that: (1) control is impossible without feedback, and (2) feedback is information, THEREFORE the more information made available to people, the more effectively they will be able to control their perceptions. (Notice I said "made available" - this doesn't mean we should flood people with too much information for their processing systems to be able to handle, but it does mean that they should have access to "relevant" information [I know; it's ALL relevant, it least potentially, and in some sense].)

Thus, for me, I think I would want the grandfather to be provided with the information about the cancer, because that information could be crucial in enabling him to adjust some of his most important reference signals. Although I understand the fear of the cancer, which is causing his mental conflict, I think he really needs to know (and, subcounsciously, I think he also knows he needs to know). But I wouldn't force him to ask the doctor, if he really resisted, since we don't know all of the preceptions (HIS perceptions, valid and legitimate for HIM) he is controlling for.

- Gene Boggess
Computer Science Dept.
Mississippi State University

Date: Mon May 18, 1992 5:00 pm PST
Subject: Re: Gene on free will & ethics

From Greg Williams (920518)

>Gene Boggess (920518)

>And, in reality, we all do have free will because it seems to us that we do
>(it's a top-level system concept); our attempts (when we are conscious of
>them) to control our perceptions seem to us to emanate from our self (another
>top-level concept). It may be that in Reality our actions are
>deterministically produced, but in the only reality we can know, they are
>not.

I agree that it seems to me that I do have (traditional) free will. I disagree that I necessarily have free will because it seems that I do. At the root of our disagreement is your claim that "in the only reality we can know," our actions are free. I think we can know more than one model of (portions of) Reality, each model corresponding to a different reality. For example, one can know a table naively (not a very good word -- an amazing amount of sophistication goes into knowing something "naively"!) AND know it as physical theory describes it. Similarly, one can know "experientially" (best word I could think of) that one has "free will" (surely a model) AND know that it is an illusion IF one accepts an unaugmented (that is, not including certain types of "miracles") PCT model for nervous-system functioning. Modeling is the Royal Road toward knowing Reality; if what one finds along that Road conflicts with one's (or, especially, others') reality, well, that's what heretics are (sometimes) punished for!

>I think there ARE some absolute standards implied by PCT:

I claim, to the contrary, that any ethical absolutes ("ought" claims) "implied" by PCT MUST be implied by PCT CONJOINED WITH at least one ethical dictum. That is because PCT is not a normative theory, and normatives can only be derived from normative postulates (suitably conjoined with non-normative hypotheticals, of course). In other words, I claim that to derive a standard from PCT, one must PRESUPPOSE a normative stance in addition to PCT. The ultimate justification of the derived standard then is attributable to the presupposed normative stance. The examples Gene gave illustrate this. (In the following critique, I'll simply accept all of Gene's non-normative claims as presumed to be the case.)

>Given that: (1) you cannot control another person (except by force); (2)
>you cannot, predictably and deterministically, change another person's
>control systems; and (3) because it's all contextual and it's all
>perception, we can't be sure that our standards and lower-level reference
>signals will be appropriate for anyone else, THEREFORE no one should try
>to impose his or her standards/reference signals on others.

Here the conclusion does NOT follow from the numbered postulates. For it to follow, there would need to be a fourth postulate which is normative; "it is not good to waste your time" is one such postulate which would serve. But note that the absolutism is only for those who subscribe to the fourth postulate.

>Given that: (1) control systems in good control exist in equilibrium with
>their environment, and (2) the environment almost always includes other
>control systems (other people), THEREFORE, to enable people to live
>with one another with the least conflict, we should try to provide others
>with the maximum opportunity to develop their own effective control systems.

Here there IS an (unnumbered) normative postulate (that it is good to enable people to live with each other with the least conflict), and the conclusion DOES follow. Again, note that the conclusion is NOT absolute for someone who thinks it is good to promote conflict.

>Given that: (1) control is impossible without feedback, and (2) feedback
>is information, THEREFORE the more information made available to people,
>the more effectively they will be able to control their perceptions.
>(Notice I said "made available" - this doesn't mean we should flood
>people with too much information for their processing systems to be able
>to handle, but it does mean that they should have access to "relevant"

>information [I know; it's ALL relevant, it least potentially, and in some >sense].)

Here there is no normative conclusion, so no normative postulate is needed.

I am sympathetic to the notion that PCT can be a useful tool when deciding how to apply one's ethical presumptions. I simply want to make it clear(er) that ethical presumptions are not part of PCT, but added on to it by human living control systems.

Greg

Date: Tue May 19, 1992 8:32 am PST
From: Dag Forssell / MCI ID: 474-2580
Subject: PCT & Corrections

[From Dag Forssell (920520.0930)

Ed Ford (920517.12:35)

What a marvelous posting! Most encouraging.

You show clearly the power of starting with a focus on what people want, instead of a focus on how people behave. You apply disturbances to encourage reorganization over time. You give the delinquents a measure of control they have been denied before.

While your situation is rather extreme, it is not different in kind from many other interactions between parents and their children, business owners and their employees, counselors and their patients.

I am reminded of a newsletter from the Thomas Jefferson Research Center, entitled: What we can learn from Japan's prisons. I believe I sent a copy to Alan Wright last August, but whether I did or not, I shall send you a copy by snail mail.

Again, your results are exciting!

Your years of preparation are paying off in a significant way.

Congratulations!

Dag

Date: Tue May 19, 1992 1:32 pm PST
Subject: misc

[From: Bruce Nevin (Tue 920419 09:00:07)]

(Bill Powers (920514b)) --

>The only reference signals (and perceptions) that can't be changed freely >as required by higher levels are system concepts. And the only reason we >can't vary our reference signals and perceptions at that level with >complete freedom is that there seems to be no place to stand except another

>system concept -- if there is a higher viewpoint, it's impossible to put
>into words or systematize. If there's free will, the only place it can work
>is at the top, because everything else is dependent and interconnected. And
>even at the top, we're free only to be human.

I suggested some time ago a level we might call conversion or (Kuhnian) revolution, though inspiration might be a better term. From this to us giddy point of view we shift from one set of systems concepts to another. It is a point of view that at least some teaching traditions have sought to cultivate, e.g. Sufism.

Rick Marken (920515 14:00)

>Control is all around -- maybe the problem is
>that it's too much around -- we take it for granted. Bill P once said that
>feedback is like the air we breath; I think this is true of control too.
>Because it is everywhere, it is invisible, unless you know what to look
>for (like the answer that's "a blowin' in the wind").

This is what I mean when I say "talk to a fish about water."

(Avery Andrews 920515.1432) --

>Actually, in the case of grammatical
>generations, I suspect that there really are things corresponding to them
>(ie., for the generalizations that lead people to postulate noun-phrases,
>there is a noun-phrase detector).

From my perspective, a noun phrase, verb phrase, etc. are byproducts of something simpler and more basic, even when we limit ourselves to structures in language-as-artifact (rather than considering what control systems do and how they do it, resulting in those structures).

(Cynthia Cochran (Fri, 15 May 1992 18:17:52)) --

Cynthia took this as a springing-off point:

>Bruce says that
>
><more than one from of behavioral outputs can accomplish the same
><controlled perception
>
>in speech and some other social/psychological behaviors. This is indeed

Bill read this as misperception of the basics:

>Bill Powers (920515.2000)]
>
>A basic concept of CT is that you VARY your actions in order to keep
>producing the SAME perceived result. You have to do this because the
>environment keeps changing (and your own actions keep changing) and
>disturbing the controlled result.

Just for the record, what I was saying (Wed 920413 09:17:47) assumes the above and looks for something more. I think Cynthia was responding appropriately to what I actually said, though I emphatically agree with (what I read as) Bill's intent, that you can't get at this "something

more" about control for social conformity without first understanding control in less complex cases.

Here's a replay of what I actually said:

>Speech is different from the control of a pointing finger in a way that
>I think is important for all the social sciences. In the usual case,
>behavioral outputs are incidental byproducts of control. They are not
>themselves controlled. Some other perception is controlled, and the
>behavioral outputs are variable means, whatever it takes in a
>disturbance-prone environment to make the controlled perception match a
>reference perception in memory or imagination. With speech, however
>(and with any conventionalized behavior) the form of the behavioral
>outputs is itself subject to control, concurrently with the perceptions
>the control of which the behavioral outputs are the variable means.
>
>This is possible whenever there is "free" variability that is not
>constrained by the contingencies of control--more than one form of
>behavioral outputs can accomplish the same controlled perception. Then
>choice among alternatives (or in the range of free variability) itself
>is exploited as an aspect of self image, or social standing, or
>relationship to others involved in the transaction, etc.
>
>Even pointing with the finger can have a personal style, or a manner
>associated with a particular community.
>
>To accomplish this, the behavioral outputs involved in effecting control
>of one perception must themselves be monitored and controlled with
>respect to particular choices among their range of free variability.

I'd appreciate thoughts on this. So far, Cynthia's is the only response I've seen.

In simple cases of control, behavioral outputs are byproducts of control. In control of "manner" the form of the behavioral outputs is itself controlled. One controls the perception of the relationship of fingertip to target (basic) and concurrently controls the manner of carrying out the pointing gesture. My boss's boss has positively serpentine mannerisms. If he had enough clout and prestige, I would expect to see others taking on those mannerisms, with little or no awareness of doing so.

Bill, you may be responding to me obliquely, by way of your response to Cynthia, when you refer to practical difficulties of modelling:

>In general, higher systems can control their perceptions not only by
>varying the amount of reference signal sent to a given set of lower
>systems, but by changing WHICH LOWER SYSTEMS are provided with reference
>signals by that higher system. This is changing the kind of behavioral
>action rather than just the amount. This is tricky to implement in a model
>when you don't already have a lot of control systems available in the
>model, which is about where we are.

I recognize the difficulty. This is why we linguists aren't instantly working up models. An enormous number of different perceptions on different levels are under concurrent control in even simple social transactions. These include negotiation or affirmation of agreements

about "subjective" perceptions such as self image and relationship between the parties, and "objective" perceptions such as objects and events, with an intermediate area of things like goals, tasks, roles, etc. (It appears that the perceptions that seem most "objective" are those that are subject to the most firmly institutionalized agreements, which fits well with the notion of intersubjective agreement.)

I have no objection to starting simple. It would be silly and futile for me to object! What I do object to is premature foreclosure.

It is simply not the case that linguists' concern with the form of language behavioral outputs necessarily betrays prejudices inherited from BP behavioral science. (BP: before PCT, natch.) Linguists and linguistics have been remarkably free, even ostentatiously free, from S-R and behaviorist theories of psychology. This is because these theories have been obviously incapable of accounting for language behavioral outputs. It is often claimed that American structuralists were behaviorists. I have argued previously in this forum that this is a canard based on a shallow and partisan reading of the work of Bloomfield and others.

What I am trying to put forward is a way of thinking about the complexity of social transactions, including communication and the use of language, in PCT terms. Only given such a framework is it sensible, I think, to go for simplified situations in which to apply the test for control and in which to start developing models. I am perfectly happy to put lots of the complexity on a shelf. There must first be a shelf on which to put it.

(Bill Powers (920515.2000)) --

>Reorganization is fundamentally a
>cybernetic process, of course, but it isn't necessarily totally "random" or
>"statistical" in nature. All that's meant by saying that a process is
>random is that we know of no algorithm that will predict what it will do
>next. A random reorganizing system is powerful because it doesn't take
>anything for granted. But reorganization could be quite systematic in some
>way that is too subtle, or too advanced, for us to find order in it.

With the e. coli demo you have shown that a random reorg is sufficient. I think you have commented elsewhere that this does not preclude reorganization being guided (controlled). I have some ideas about that (below).

(Dag Forssell (920516-1)) --

>Bill Powers (920512.0930)
>
>>Influences should be thought of as disturbances. That is, you can
>>perform an act that by itself would alter the other's perceptual world
>>if it were the only influence.....
>
>It makes sense to me to see influences as disturbances. Can you see
>information as disturbances also?

I like this a lot.

For Gregory Bateson, the elementary unit of information (or of mind) is a difference that makes a difference, i.e. that is transformed into another difference, such that the sequence of transformations proceeds in a loop.

* No disturbance => no error => no change

* Disturbance => error => change in reference signal
=> change in output of effectors

Deliberate influence is disturbance controlled in a way that is contingent upon behavioral outputs of the one disturbed, as they in turn disturb perceptions that the influencer is controlling.

Define:

<agent> := I, M
I = the influencer
M = the mark (the one influenced)
o.<agent> = behavioral outputs of <agent>
p.<agent> = perceptual input to <agent>
r.<agent> = reference perception in <agent>
e.<agent> = r.<agent> - p.<agent>
E = some perception in the environment, can be o.<agent>

I perceives o.M | p.I != r.I ==> e.I ==> o.I
Behavioral output o.I affects E in some way.
M perceives E | p.M != r.M

M has some choices in the face of disturbance:

(1) p.M != r.M ==> e.M ==> o.M
This is called control.
(2) p'.M = r.M
where p'.M by the imagination connection overrides p.M
This is called ignoring.
(3) p.M = r'.M
where r'.m by the imagination connection overrides r.M
This is called adapting, or being influenced.

The acquisition of a new reference perception need not by by a reorganization process reaching into the random. It may be by choice of a perceptual universe, in memory and imagination, in which the error does not occur.

Choice (3) is attractive e.g. if the emotional value of p.M (or of some imagined opposite, anti-p.M) is greater than that associated with r.M.

Choice (2) is attractive e.g. if e.M is likely to be transient.

(Bill Powers (920515.2000)) --

>I've now heard ouches from two owners of linguistic toes.

Or was that a heel?

You stopped one quote short in your "you said, I said" summary. You left out the one in which I loudly agreed with your sequel in this message. To recap again:

You:

>If it seems that there is structure in language, then a model that
>explains this phenomenon should not contain that structure, but only
>components that lead to phenomena which can be seen as having that
>structure.

Me:

>If it seems that there are words [word dependencies, etc.] in
>language, then a model that explains this phenomenon should not contain
>words [etc], but only components that lead to phenomena which can be
>seen as being words [etc].

Me (missing quote):

>The structure is there. The interpretation of it, or an account of how
>perceptual control systems bring it about, is up to us.

You (apparently ignoring this):

>By remaining at the level of phenomena, we can only observe and record
>apparent cooccurrances and dependencies of particular things. We can't
>explain why they are related as they are. The method of modeling attempts
>to go beneath that level to the level of underlying operations, looking for
>operations that could produce both the phenomena and the observed
>dependencies among them.

I think I'm agreeing with you. You apparently think I'm not. One of us is missing something.

>When
>I examine words closely, they seem to be just familiar chunks of sound or
>objects on paper. The meanings they seem to have (for those words that can
>individually designate meanings) turn out to be ordinary perceptions. So
>both words and their meanings are ordinary perceptions.

Let's be careful here. What about those words that cannot "individually designate meanings?" And of those that can, I have provided evidence in prior posts indicating that the association of words to nonverbal perceptions is not a simple matter, but is "dirtied" by all sorts of historically contingent social conventionalization. For example, what is the nonverbal perception associated with "on" or "by"? Is there just one?

Avery (920515) opined that

>grammar does not seem to be real-world
>interactive in the manner that most things studied in psychology are,
>so the structures in grammar can't be coming from simple interactions
>with a complex environment.

I agree with you, Bill, that

>grammar is real-world interactive during the time it develops.

In Generativist theory, the structure in language springs full-blown from DNA, like Athena from Zeus's headache. The argument from paucity of data says (1) that language is too complex, and (2) what children hear around them exemplifies too little of that complexity, and anyway is too fraught with performance error, for (3) the limited cognitive capacities of infants to make anything of it, so it must be biologically innate. The response is (1) that language is not all that complex (though Generativist descriptions of it are), (2) that what children experience is carefully structured in the conventional frameworks of a Language Acquisition Support System (LASS), and (3) that the cognitive capacities of infants are demonstratedly much much greater than estimated by Piaget and others on whom Chomsky et al. depended. Operator grammar addresses point 1, Bruner addresses point 2, and PCT should have a great deal to say about point 3 in addition to the considerable body of conventional work that has been published on cognitive capacities of infants and children. I want PCT to embrace points 1 and 2 as well, and that is the end to which much of my writing here has been bent.

>No
>skill, after it becomes habitual, is real-world interactive to the same
>extent it is while it's being learned. We reduce principles and reasoning
>to slogans at the drop of a hat. We reduce slogans to slurred and run-
>together events even more readily: ISWEARTOTELLLTHETHRUT
>HANDNOTHINGBUTTHETRUTHSOHELPMEGOD. By the time we're adults, "grammar" is
>just how we say things; most people say the same things the same way every
>time, without considering whether it's grammatical or not. When you lose
>the real-world interactiveness of grammar, you've also lost grammar.

There is an element of truth in this, and it underlies a range of pervasive phenomena of language change encompassing lenitions (e.g. "slurred pronunciation"), cliches, morphemization or grammaticalization (e.g. "building" as a concrete noun alongside the verb form--"he was building that building" vs. "he was swinging that swinging"), and so forth.

However, you underestimate the creative aspect of language use.

>If you
>hear "Every man for itself," the "itself" isn't grammatically wrong, it's
>wrong because you said the memorized phrase wrong; you made the wrong sound
>at the end. We no longer think of "Every man for himself" as a sentence in
>which the words have individual meaning, sexist or otherwise. It's just
>something you emit under certain circumstances. It's like a shaped grunt.

This is just plain naive.

Everyone often says or writes novel sentences that she or he has never said or written before and is unlikely ever to say or write again. This post is filled with examples. So are yours. Sure, there are frozen expressions of all kinds, ranging from familiar quotations like "now is the time for all good men to come to the aid of their party," to cliches like "every man for himself," to idioms like "take the bull by the horns," and so on. But they are by no means all that people produce and hear, even abstracting away errors.

I can't remember when I've said, read, or heard "every man for himself" in the past five years.

>This is probably a good answer to your objection about treating rules as >controlled perceptions. As long as there is a possibility of making >mistakes, the rules are perceived and the utterances are adjusted until the >right rules are perceived. But once a person has settled on utterances that >will reliably fit the rules, the utterances are reduced to phrases and no >longer are treated as having internal structure. Then those utterances are >no longer rule-driven. They're just the way you talk.

This is a reasonable description of the fate of frozen expressions. They become treated almost as complex words. But except for quotations, most of them are partially productive, that is, subject to grammatical operations in some parts but not in others, as in "He really took the bull by the horns," "Take the bull by the horns," "You must take the bull by the horns," "You must learn to be brave and take the proverbial bull by the horns," and so on.

It is interesting that socially "safe" conversation departs little from a relatively narrow range of frozen expressions that are conventional tokens for local definitions of membership. More intimate conversation often involves more creative use of language. A complex issue--there are many forms of communication that do not use language, avoidance of "comfortable" cliches can be socially distancing, etc.

Got to run.

Bruce
bn@bbn.com

Date: Tue May 19, 1992 3:34 pm PST
From: g cziko
EMS: INTERNET / MCI ID: 376-5414
MBX: g-cziko@uiuc.edu

TO: * Dag Forssell / MCI ID: 474-2580
Subject: recommended name of a Systems Theorist

Dag:

I thought you would like to see this note.--Gary
==========

>Date: Tue, 19 May 92 12:38:44 +0200
>X-Ph: V3.12@garcon.cso.uiuc.edu
>From: larsky@jamvax.sunet.se
>To: "g-cziko@uiuc.edu"@kth.sunet.se
>Subject: recommended name of a Systems Theorist
>
>Dear Gary,
>
>Thank you for contacting me and giving me the name of W. Powers to >be included in my list of systems-theorist. I will begin my >introduction to this area by reading your recommended book with the

>title :Introduction to control theory. May-be I later will introduce
>myself in the special network but just now I am afraid of getting too much
>information, we have all experienced what information input overload is.
>If you want to keep in touch with me I remain of my E-mail adress which
>is : larsky@it.hos.se
>
>Greetings from Sweden and Lars

Date: Wed May 20, 1992 4:20 am PST
Subject: standards

[From: Bruce Nevin (Wed 920420 08:03:48)]

One value of the word "standards" is that it emphasizes the social
aspect of principle-level perceptions.

Bruce bn@bbn.com
Date: Wed May 20, 1992 10:11 am PST
From: Dag Forssell / MCI ID: 474-2580
TO: MBX: larsky@it.hos.se
MBX: G-CZIKO@UIUC.EDU
Subject: Kontrollteori

Halsningar fran Californien!

Gary Cziko skickade en kopia av Ditt meddelande i gar, med halsningar
fran "Sweden and Lars." Jag blir litet nyfiken pa vad som menas med
Control Theorist fran Ditt perspektiv. Bill Powers ar en magnifik man som
agnat 35 ar at att beskriva manniskor som "styrsystem." Jag ar
Chalmerist, men har bott har i 25 ar och arbetar inom industri. Nu borjar
jag marknadsfora ett ledarskaps program baserat pa Powers' modell.

Vart elektroniska medium, CSG-l, ar mycket aktivt med en blandning av
social tillampning och detaljerad teknik, foretradesvis simulering. Om
Du har fragor, kommer Du att finna Bill Powers en outtrottlig,
mangfacetterad kampe.

Dag Forssell
23903 Via Flamenco
Valencia, Ca 91355-2808
Phone (805) 254-1195 Fax (805) 254-7956
Internet: 0004742580@MCIMAIL.COM

Date: Wed May 20, 1992 11:14 am PST
Subject: clinical example--finale

To: interested CSGnet people
From: David Goldstein
Subject: reflections on clinical example
Date: 05/20/92

I have made several posts on a case I am seeing now. In the last
post I announced that I was going to stop at this point after
making some final comments. Here they are:

(1) In the case, a major focus has been that I have tried to

understand the motivations for the man's action on the fateful night when he was caught. Rather than state them in "I want" form, I will make a number of factual statements from which "I want" hypotheses could be derived:

(a) The man was angry at his wife that night.
(b) The man did not communicate his feelings/thoughts to his wife this night before they went asleep.
(c) The man had been in a depressed state for several months.
(d) The man comes from an emmeshed family in which boundaries were often violated by his parents. The man developed a passive/aggressive style of a handling angry feelings in himself and his wife.
(e) The babysitter "invited/threatened" the man to join him. She was "An Earth Mother" figure for him and almost like a second wife.
(f) The man did not experience any thoughts along the line "This is wrong."
(g) The man was aware that he might get caught (the squeaky stairs.)
(h) The man has kept this relationship with the babysitter, on and off, for eight years.

(3) The man is relieved that "it is over." I think that he wanted to be caught and bring everything out into the open.

(4) There is a danger here that if the man accepts 100% repsonsibility that nothing will really change in the relationship. In the last individual session I have had with him, I think he is starting to understand this.

(5) They are starting to spend Quality Time together and this is helpful in showing them that they can enjoy each other's company. However, this will not be enough. Each of them has to change in ways that I outlined in the last post or I don't see any reason why there wouldn't be a repetition.

(6) The directions of possible change in marital therapy are the same as the possible kinds of changes which can take place in group therapy which I outlined in a previous post. I am wondering whether it wouldn't be more effective to have a group of couples.

(7) I am pleased with the way things are progressing with this couple. I think we have worked through the initial crisis. Now we have to get down to the hard part.

Date: Wed May 20, 1992 3:47 pm PST
Subject: Free will

From: Gene Boggess (920520)

Ref: Greg Williams (920518)

Greg:

You said:
> I agree that it seems to me that I do have (traditional) free will. I
> disagree that I necessarily have free will because it seems that I do.

"Free will" refers to an abstract concept, not to an observable object; thus, it cannot be pointed to but must be defined. To me, the definition of free will must involve some concept of responsibility, of accepting credit, or blame, for the consequences of one's own choices. In the reality in which we live, credit and blame (both internally and externally assigned) are quite real; these concepts are socially sustained and validated. The next time you have to make an important decision, try to not exercise free will - just stand there and let the inexorable flow of the forces of the universe make the choice for you. Resist all impulses to act of your own free will. Now monitor yourself and see if you perceive any error. My guess is that you will have a huge error signal, and that the psychological impulse to take some action will be overwhelming. So deliberately make a choice, and act on it; then check to see whether you feel responsible for the action you have taken, and whether others also feel that you are responsible. My guess is that, in the cognitive and social reality in which you live, the feeling of responsibility will be quite real, on both sides.

Humans are limited. We exist in a limited place, for a limited time, and have limited cognitive capacity; yet we have to deal with an unlimited universe. So the human organism, to survive, must abstract, chunk, and filter the enormous amouts of potentially significant raw data available in our environment. Perceptually, we can't hear sounds below 20Hz or above 20KHz; we can't see in the infrared or the ultraviolet; we can't feel alpha, beta or gamma radiation hitting our skin. Cognitively, we can't remember the exact pronunciation of each phoneme in the sentence someone has just said to us; we remember the words and the general tone of voice instead. Just think of the length of time it would take our neural processing system to bring all of the sensations up to the level of the cerebral cortex, not to mention the tremendously increased number of neural pathways required, nor the fact that the cortex would become oversaturated with signals in just a fraction of a second. So we live in a world of metaphors and abstractions; we have to, in order to survive.

The implication of all this is that we, quite literally, create the world we live in. There is no "democracy" Out There, just a series of discrete space-time events - events to which we have no direct, unmediated access. There is no "red", just a near-infinite number of discrete visual sensations in hundreds of millions of people - sensations that no others can share. There is no "PCT", just words on paper, electrons in computers, and neural patterns in the minds of several hundred people. But in our world, these terms have meaning and power. There very well may be no "free will" Out There, just a series of discrete space-time events, all pre-determined by the initial state of the universe at the moment of the Big Bang. But in our world - the only world we can know - free will (or rather the feeling of individual responsibility for choosing our actions, which we label "free will") is just as real and meaningful and consequential as any other component of reality, all of which are, necessarily, abstractions from Reality

> At the root of our disagreement is your claim that "in the only reality we
> can know," our actions are free. I think we can know more than one model of
> (portions of) Reality, each model corresponding to a different reality.

The Existentialists are particularly eloquent about the moment of difficult personal choice being the point at which we know we exist (or even, through our choice, define ourselves into existence) because of the personal

angst involved in making the choice. At that moment, all of your models of reality collapse into the one reality in which that choice is the natural - the only - choice that the real you can make. We can abstractly consider other possible realities, but at each moment we can live in only one.

As an aside, I would also be willing to argue that not only can we not know big-R Reality, we can't know little-r reality completely, either. We create it as we act, and thus re-constitute reality every time we make a choice. (I often don't know what I think about a subject until I see what I say about it, and work out my thoughts by expressing a number of ideas and trying them on for size to see how they fit.)

> Modeling is the Royal Road toward knowing Reality; if what one finds along
> that Road conflicts with one's (or, especially, others') reality, well,
> that's what heretics are (sometimes) punished for!

There is no "Royal Road" to KNOWING Reality. Modeling is just one more tool to use in trying to develop useful metaphors to guide our thoughts and actions. It abstracts and regularizes; what you get is not even what you see, much less what is Really Out There (whatever that is).

I said:
>>I think there ARE some absolute standards implied by PCT:

You responded:
> I claim, to the contrary, that any ethical absolutes ("ought" claims)
> "implied" by PCT MUST be implied by PCT CONJOINED WITH at least one ethical
> dictum.

I stated:
>>Given that: (1) control systems in good control exist in equilibrium with
>>their environment, and (2) the environment almost always includes other
>>control systems (other people), THEREFORE, to enable people to live
>>with one another with the least conflict, we should try to provide others
>>with the maximum opportunity to develop their own effective control systems.

You said:
> Here there IS an (unnumbered) normative postulate (that it is good to enable
> people to live with each other with the least conflict), and the conclusion
> DOES follow. Again, note that the conclusion is NOT absolute for someone who
> thinks it is good to promote conflict.

OK; let me elaborate. We are control systems. We evolved (or were created) in such a way that we try to reduce the error signal between an interior reference signal and a sensation, presumably caused by an external event. If the event produces a disturbance, we change our actions in such a way that the error signal is once again reduced to a more desirable level. It is against the nature of our organism to tolerate large error signals; it would be a disturbance, and the organism would act in such a way as to reduce or eliminate the disturbance. Too many disturbances in an environment will swamp the control system and throw the organism into chaos, as the feedback from one action is lost or overwhelmed by the plethora of other signals coming in at the same time; in extreme cases, this can cause the death of the organism. Reduction of conflict is thus necessary for the continued existance of the organism. If people are effective control systems, then they are more likely to live in some sort of equilibrium with their environments, including other people (us, for example). This will reduce conflict and make it more likely that everyone can continue to exist. (I assume continued existence is

a self-evident value, as without it there can be no other values, at least in the reality we are speaking of.) I therefore claim that allowing other people the opportunity to develop as effective control systems is an ethical norm implied by the nature of PCT and by the nature of people as control systems, and is not based on any external ethical consideration.

- Gene

Date: Wed May 20, 1992 7:08 pm PST
Subject: standards

[From Rick Marken (920520 19:00)]

Looks like my distinguished peer, Danny Quayle, has made the issue of standards more appropriate than ever. Now, it seems, the official government position is that these nasty social problems would be solved if everyone would just adopt the right values (standards) -- and guess who's standards those are; the Trobriand islander values? The Napalese values? The Danish values (my personal favorite)? Nope -- Republican values. Thanks for clearing that up, Dan. It makes me proud to be a member of the '60s generation. If only he could claim that it was the result of smoking too much dope.

Bruce Nevin (Wed 920420 08:03:48) says:

>One value of the word "standards" is that it emphasizes the social
>aspect of principle-level perceptions.

I don't like the word "standard" despite this admitted plus. First, the word "standard" can be a synonym for "reference". So it is very confusing to me when people talk about the IMPORTANCE of "standards". It sounds wrong when I think of standards as references because ALL reference signals are important in the model. And it sounds wrong for the same reason when I think of standards as principles. It is no more important to control principles than it is to control intensities.

The PCT model says that we are controlling many levels of perceptual experience simultaneously. Lower level perceptions are controlled in order to control higher order perceptions. The higher level perceptions are in no sense more or less important than lower level perceptions; all perceptions must be brought to their reference levels in order for there to be control at all. So it is just as important to be able to control the position of your torso as it is to be able to control your position in a perceived relationship as it is to control the principle that is satisfied by being in that relationship, etc.

I agree with Bruce that principles often have to do with other people (they involve setting references for reltionships between you and others people, for example). I think these perceptions seem special only because most of our control problems involve attempts to control variables that involve other people (as one would expect since people, being control systems, cannot be controlled and so there will often be large, chronic errors in these systems). It will be very hard to control relationships, programs, principles, etc that involve other people. Since control is generally poor for variables involving other people, our attention (consciousness) will tend to be examining the control systems at this level (it is a kind of postulate of PCT that consciousness tends to move to the level where reorganization is required -- no tests of this that I know of so far; hence, I am talking through my hat).

I am hypothesizing that consciousness (attention) tends to be directed toward control systems involved in the control of variables which involve other people (due to the chronic error that tends to exist in these systems). Better yet, I think we attend to systems involved in the control of variables which involve AT LEAST the RELATIONSHIP between people -- most importantly, relationships between OURSELVES and other people. So my hypothesis is that we attend mostly to systems controlling perceptual variables at the relationship level (level 6 and up). We rarely attend to our control of intensities, sensations, configurations, transitions, events, etc. We do attend to relationships (with the boyfriend/girlfriend), programs (soap opera stories), categories ("he was an sob"), principles (he done her wrong), and system concepts (that was no way for a christian to behave).

I think it does something of a disservice to the PCT model to try to emphasize the importance of one type of perception relative to another. They are ALL important.

If the feeling is that the higher level systems are more important because they determine the goals of the lower level perceptions, then this feeling is incorrect (in terms of the model, anyway). The particular reference level that is selected for a lower level perception depends on the goals of the higher order perception AND on pervailing disturbances which are independent of the goals of the system. So, setting my reference for a principle, like "get control of the center" will result in very different chess moves (relationships) on different occasions; some of those moves may not actually be "good" in terms of other goals (like winning the game) if I just bindly follow the principle.

But you all know that. We just tend to forget it when we are dealing with really "important" principles (the kind that we have been calling standards); principles like "be kind to your neighbor" (even when your neighbor is a nazi who is trying to kill you?). The desire to find the "right" references for our perceptions of principles, etc (ie -- interpersonal perceptions) is strong; and I think it's because consciousness DOES tend to be focussed at these levels. Consciousness is involved in learning and the goal of learning is to try to find the "right" reference settings for perceptions involved in what you are trying to learn to do (to control). If we had more difficulty with the low rather than the higher levels of perceptual control (so that consciousness was always hanging around those levels) we would probably spend all our time trying to figure out the right configurations, sensations, transitions and intensities to experience. Sometimes we do try to figure out the "right" settings for these variables -- like when we are learning a sport or a musical instrument. Of course, even in this case there are no right settings; just the right variables to vary (by changing references) in order to control the higher level variable.

I suggest that unless an individual is conflicted at the principle level then there is no reason to try to direct their consciousness to that level in particular (indeed, if they are conflicted at the principle level then you should try to get them (their consciousness) up to the system concept level. I would say that, as a control theorist, I would try to get a person's consciousness AWAY from thier principle level if there is NO conflict there. Putting consciousness where it does not belong can be quite a problem (at least in theory) because it can start a reorganization that is not necessary. If anyone doubts this, just try moving your consciousness to the perceptual levels that are ordinarily unconsiously controlled in a well learned skill; I

tried this after I had learned to play a two-part invention by heart. I tried to become conscious of what my fingers were doing (the sequence level and some transition and configuration stuff too). Well, the two part invention turned quickly into an N part cacophony.

I think Zen people knew the potential problems of consciousness. My suggestion to people who are doing therapy (on themselves or on others) is to lay off the levels that are not conflicted. And don't assume that a level is conflicted just because it seems like it is important; I bet very few people have any real problems at the principle or system concept level. I bet most people just can't control relationships, programs, sequences, stuff like that. I would not ASSUME that the problem is always principles (it may be intensities -- maybe the person has a boil, not an "attitude").

Another motto -- if it works, don't be conscious of it while it's working.

Best regards Rick

Date: Wed May 20, 1992 9:31 pm PST
Subject: Language, models, misc

[Avery Andrews (920520.2000)]

(Bill Powers 920515.2000)

>>Actually, in the case of grammatical generations, I suspect that there
>>really are things corresponding to them (ie., for the generalizations
>>that lead people to postulate noun-phrases, there is a noun-phrase
>>detector).

>You can think of a noun-phrase detector in several ways. The conventional
>way is to say that there really are such things as noun-phrases in some
>objective world, and that all we need to do is develop detectors that can
>respond to them. The CT way is to say that because we have developed
>perceptual systems that respond to things in terms of noun-phrases, we can
>create and control the occurrance of noun-phrases.

I don't think that current people would say anything different to this. In Situation Semantics, for example, noun-phrases would exist *because* other people respond to them in regular ways.

>Surely the ability to
>perceive structure, to alter one's actions so as to create and change
>structures that are perceived, is a more fundamental aspect of organization
>than is the ability to perceive and control for a particular example of
>structure.

Indeed, but we are still amazingly ignorant about the general nature of the funny kinds of structure that there are out there in languages, like a language called Kayardild in Northern Australia where the words in some noun-phrases can take up to four levels of morphological case-marking, expressing, among other things, the tense and modality of the clause, and certain aspects of subordination. I go for the descriptive schemes (commonly though probably wrongly called `theories') that I think are

most likely to give *sharp* descriptions of these things. Sharp means that I can describe the scheme to a programmar who can go away and implement it without doing any serious linguistic theorizing, (e.g., it is well-defined in fact, even if it doesn't bristle with horrible-looking symbols) & students can then use the resulting system to implement their answers to descriptive problems (this means that it expresses the basic organization of the data in a clear and straightforward manner).

The involvement with descriptive schemes isn't because I don't believe in real modelling, but because I wouldn't have a clue as to how to actually do it in this area in such a way as to do justice to the aspects of grammar that I like to think about. And a model that doen't explain how the various wierdo grammatical structures I'm interested in can arise will just be wrong, so the results of the descriptive scheme project ought to be useful as constraints on model building, when somebody is clever enough to figure out how to actually do it.

Grammar-learning & interaction: children say lots of stuff they've never said before as fluently as adults say things, & I doubt we really loose these skills. I've never denied that grammar-learning is interactional, & suggested various formulations for what the system driving acquisition might be (something like: perceive people making sense. e.g. when people are not perceived as making sense, the grammatical system starts getting Reorganized). Knowing what the grammatical systems of a wide variety of languages are like should help in figuring out what is being Reorganized, & how.

Avery.Andrews@anu.edu.au
(currently andrews@csli.stanford.edu_

Date: Thu May 21, 1992 4:20 am PST
Subject: Free ethics/Absolute will

From Greg Williams (920521)

>Gene Boggess (920520)

> "Free will" refers to an abstract concept, not to an observable object;
>thus, it cannot be pointed to but must be defined.

Agreed -- in fact, there are several models for several kinds of "free will"; I was speaking of what I have termed "traditional" free will (in the Western religious tradition).

>To me, the definition of free will must involve some concept of
>responsibility, of accepting credit, or blame, for the consequences of one's
>own choices. In the reality in which we live, credit and blame (both
>internally and externally assigned) are quite real; these concepts are
>socially sustained and validated.

With "traditional" free will, credit and blame are accorded by the supernatural, as well as one's (natural) peers. Nevertheless, the notion of free will you are putting forth seems close to the "traditional" one. As I pointed out in a post last week, there is certainly a sense in which "I" am responsible -- at least to society -- for my "choices," since it was "my" control structure was intimately involved in "making choices." But I still

maintain that the PCT model gives no support to those who want to say that such "choices" can be solely determined by the "free choice" of "my" "will." This is not to say that ("traditional," or your similar notion of) free will doesn't exist, but rather that PCT models (unaugmented by certain types of "miracles," as defined in my last post) imply that it is an illusion, and point toward a notion of SHARED RESPONSIBILITY for decision-making -- shared by an individual's control structure AND the history of that control structure's "niche."

> The next time you have to make an important decision, try to not exercise
>free will - just stand there and let the inexorable flow of the forces of the
>universe make the choice for you. Resist all impulses to act of your own free
>will. Now monitor yourself and see if you perceive any error. My guess is
>that you will have a huge error signal, and that the psychological impulse to
>take some action will be overwhelming. So deliberately make a choice, and act
>on it; then check to see whether you feel responsible for the action you have
>taken, and whether others also feel that you are responsible. My guess is
>that, in the cognitive and social reality in which you live, the feeling of
>responsibility will be quite real, on both sides.

I have never denied the "experiential reality" of free will. But, like phantom-limb pain, it is a "reality" which conflicts with other models of what is going on. In the case of phantom-limb case, the patient can come to understand the illusory nature of his/her experience. I claim that, similarly, we can come to understand the illusory nature of free will by buying into the (unaugmented) PCT model.

I am certainly not denying that "ideas have consequences." Surely for us it is "all models," and we act on the basis of those models. Many decisions were once made based on the model of a flat earth -- but now many more are based on the model of a round one. Models can conflict with one another, and one cannot always believe in both -- one has to give up one as ill-founded or, in some cases, illusory. This is a different kind of "illusory" than in the package we buy into when we say that it's all perception. In this case, belief in unaugmented PCT is incompatible with belief in free will. But the illusion of free will, which I accept, can be very effective in spurring on certain types of action. On the other hand, so can the PCT-influenced notion that it IS an illusion! And the types of action will probably be somewhat different depending on whether free will is or is not viewed as an illusion. (For example, PCTers should not be tempted to make fun of a welfare mother who blames the Establishment, in part, for her current sad state. They should not say that it is ALL her own fault.)

>We can abstractly consider other possible realities, but at each moment we can
>live in only one.

But the PCT model says that what we do NOW is the (perhaps probabilistic) result of the models we have been holding. The ONE model we now "live" has been engendered by all of those abstract considerations.

>[on ethical absolutes implied by PCT]

>(I assume continued existence is a self-evident value, as without it there
>can be no other values, at least in the reality we are speaking of.) I

>therefore claim that allowing other people the opportunity to develop as
>effective control systems is an ethical norm implied by the nature of PCT and
>by the nature of people as control systems, and is not based on any external
>ethical consideration.

You just set the "external ethical consideration," namely that "continued existence is a self-evident value." Existence of what? You? Other people in general? Some particular other people? Animals, in general or particular? Plants, in general or particular? Ecosystems? The Biosphere?

This is what keeps theologians busy.

No doubt, when I die I have no more opportunity to play the "game" called Life. (Of course, some hypothesize that my "will" will continue to play games of a different sort!) Yet it is not always "self-evident" that my survival is more important than that of, for example, one of my sons. In fact, to indulge in a bit of science fiction not terribly removed from the present, there could come a time when the indefinite survival of those able to afford it will conflict directly with the survival of poverty-stricken newborns, because of burgeoning population pressures. As Warren McCulloch put it, it is appropriate that the prior generations get out of the way to make room for new ideas. McCulloch's ideas on this are challenged by some, as is the idea that human survival is not ALWAYS sacred (even survival of the entire human race, according to poet Robinson Jeffers). So, various positions on even your "self-evident value" have been and continue to be held by seemingly sane individuals in our own society. I won't bother with illustrations from other societies.

"Self-evident" is contextual, as are all other models. And the ethical grounding it (or another normative postulate) provides for a norm is NOT absolute. PCT, alone, does not say, "Thou shalt do this." PCT, taken together with various normative postulates, says, "Thou shalt do VARIOUS things."

Greg

Date: Thu May 21, 1992 6:41 am PST
Subject: ignoring conflict

[From: Bruce Nevin (Thu 920421 09:30:26)]

(Rick Marken (920520 19:00)) --

>My suggestion to people who are doing therapy (on themselves
>or on others) is to lay off the levels that are not conflicted.
>And don't assume that a level is conflicted just because it
>seems like it is important; I bet very few people have any
>real problems at the principle or system concept level. I bet most
>people just can't control relationships, programs, sequences,
>stuff like that. I would not ASSUME that the problem is always
>principles (it may be intensities -- maybe the person has a
>boil, not an "attitude").

I guess you mean to lay off the levels that don't provide a higher vantage point on levels that are conflicted.

It seems appropriate to attend to principles (standards) if people are conflicted at the program, sequence, or category level, such that taking

a point of view through principle perceptions discloses the terms of the conflict.

However, I think there is reason for attending to perceptions at or below the level of conflict. I suggested this to David.

As I argued yesterday and before, I think a common (bandaid) resolution of conflict is to ignore lower-level perceptions that cause error at the level of conflict. One way to ignore a perception may be to substitute a copy of the reference signal by the imagination loop, as I suggested yesterday. Another way seems to have the effect of making areas of the body blank, dark, numb, foggy, armored--people use different metaphors. The character of the perceptions that are being blanked out may provide clues about the error being ignored, and thence clues about the conflict being bandaid-resolved.

In the vipassana practice that I have described previously, after an initial period of attending only to perceptions of the movement of the breath at the nostrils (anapana) to develop the ability to focus and maintain attention--the first 3 days of a 10-day course, the first month of a 3-month course, etc.--one begins to move attention systematically through the body, from one end to the other, area by area. It is very common experience for a given area to seem "dark" or devoid of sensory signals, sometimes for extended periods, yet subsequently a great deal seems to be going on there. You're just sitting still, breathing, and moving your attention from place to place, so there's no evident physical stimulation. In the interim, however, perhaps some emotion-laden imagery or memory has come up to distract you from attending to physical perceptions in the body. Like starlings, if you don't feed them they go away (aniccha). Attending to physical perceptions in this way is a way of not feeding them. Their going away unfed seems to be associated with the "waking up" of areas of the body that had been blanked out. Ignoring perceptions seems to have the cost of turning off sensory inputs. People who do body work (massage, polarity, etc.) are familiar with this.

Some forms of therapy dwell on the content of the emotion-laden imagery and memories. Perhaps this can be useful. I suspect it is useful only when people get in touch with their feelings, not in the sense of their emotional reactiveness, but rather in the sense of awareness of physical perceptions in the body. This is why I challenged the statement, by David's client, that sex with the babysitter was about equivalent to masturbation. I wondered if he was ignoring some perceptions. Like going over that noisy place on the stairs every day. Like his rate of breathing.

Bruce Nevin bn@bbn.com

Date: Thu May 21, 1992 9:14 am PST
Subject: ignoring NON-conflict

[From Rick Marken (920521 10:00)]

Bruce Nevin (Thu 920421 09:30:26)] says:

>I guess you mean to lay off the levels that don't provide a higher

>vantage point on levels that are conflicted.

Yes. I also mean that, if there is NO conflict, then don't try to become conscious of these non-conflicting systems. It's OK to go up a level from a non-conflict. It's like the piano example -- it's ok to be conscious of the fact that I'm playing a two part invention; it's just a bad idea to focus on the systems that are successfully producing the perceptions that are accomplishing this higher level goal.

>It seems appropriate to attend to principles (standards) if people are >conflicted at the program, sequence, or category level, such that taking >a point of view through principle perceptions discloses the terms of the >conflict.

Absolutely!! And it is Ok to go up a level even when there is no conflict; consciousness is just a problem when it is focused on the systems that are currently successfully achieving a higher level goal; like when you think about HOW you manage to keep the car on the road WHILE you are driving (it's perfectly OK-- in terms of control ability-- to think about how you drive when you are not currently controlling the car).

Assuming that the CSG model is right and that we really do control perceptions of principles in order to control system concepts, then I am suggesting that, if you direct someone's consciousness to the principles that they are controlling while they are succesfully controlling a system concept (like being a Christian or a Dodger fan) WHILE thay are controlling that system concept then their control of that system concept will become less skillful. That's OK if there is a conflict at the principle level that prevents control of the system concept; but it's not such a hot idea otherwise (though I think it can be fun; especially if you don't care for the system concept a person is controlling. I think this is what goes on in skillful political debate; get your opponent to look at the principles that they are controlling; suddenly, their ability to defend their system concept deteriorates; not because they see anything WRONG with the principle; just they SEE it [Side not to Greg Williams -- this could be another nefarious application of PCT; if you get real good at directing a person's consiousness to certain levels you could screw up their performance on some task. For example, when you are about to play a game of tennis with your buddy you might ask "Say, are you still turning your wrist on the backswing?". Encorage them to think about this during the game. If they do, you are a sure winner. Actually, people have already discovered this technique; I think Stephen PoOCer wrote about it in a book called Gamesmanship (a funny book, by the way)]).

>However, I think there is reason for attending to perceptions at or >below the level of conflict. I suggested this to David.

I agree with everything you said about this. The main point of my previous post (that you responded to) is that ALL LEVELS OF PERCEPTION ARE EQUALLY IMPORTANT. System concepts are no more important than principles which are no more important than configurations, etc. If you agree with this point then my reference for this principle will be satisfied.

Best regards Rick

Date: Thu May 21, 1992 9:59 am PST
Subject: re.: conflicts

To: Rick, Bruce, interested others
From: David Goldstein
Subject: conflict
Date: 05/21/92

The only problem I have with your therapy suggestion Rick is that it is not always easy or necessary or a good idea to thinking in terms of levels when you are working with a person.

One of the ways you know you are confronting a conflict is when the person is not able to go up a level. The method of levels is taking you no place.

As Bill Powers has suggested to me when doing the method of levels, don't be so concerned with the levels as outlined in the formal theory. Be sensitive to the background perceptions based on what the person is saying and the person's own reactions to what is being said.

A related point to what is being said has to do with a difference I've noticed between Ed Ford and myself in applying HPCT. Ed starts at the systems level and works downward. I start at a lower level and work upward. One advantage of the bottom-to-top strategy is that it avoids what Rick is talking about, namely, directing a person's awareness to levels which "are not broken."

David
goldstein@saturn.glassboro.edu

Date: Thu May 21, 1992 10:34 am PST
Subject: re.: conflicts

[From Rick Marken (920521 11:00)]

David Goldstein says:

>The only problem I have with your therapy suggestion Rick is that
>it is not always easy or necessary or a good idea to thinking in
>terms of levels when you are working with a person.

I AGREE!! I only used specific PCT levels words because there was talk about the importance of "standards" where "standards" were alleged to be principles (in PCT hierarchy terms). In real life, I would not even try for a second to relate a person's conflicts to the proposed PCT levels. All I suggest is that conflicts can occur at at any percpetual level (in theory and in practice) so there is no reason to single out standards (principles) as an important place to look. In fact, the more I think about it the more convinced I become that real conflict have to do with pretty low level percepts, whatever you want to call them; and the resolution to most conflicts just involves seeing that things can be done in sequence or that X does not need to be categorized as a Y or whatever. I think it is rarely necessary to change principles or system concepts (or any high level perceptual reference) to solve

most personal problems. I think this is consistent with the fact that people who hold tranparently idiotic system concepts (from my perspective) can still get along just great in the world. One exquisite example of this is the fellow who wrote my 2 part intentions; J S Bach lived a wonderful life and produced the greatest sequences and configurations of sound ever produced -- and he did it all for the god of martin luther. Silly system concept; great, non-conflicted control system.

Regards Rick

Date: Thu May 21, 1992 11:46 am PST
From: g cziko
EMS: INTERNET / MCI ID: 376-5414
MBX: g-cziko@uiuc.edu

TO: * Dag Forssell / MCI ID: 474-2580
Subject: Re: Kontrollteori

Dag:

I thought I would try to impress you with how English plus fair knowledge of German and CT can be used to make some sense of CT Swedish.

>Halsningar fran Californien!
Greetings from California!
>
>Gary Cziko skickade en kopia av Ditt meddelande i gar, med halsningar
Gary Cziko sent a copy of your message to me, with greetings from

>fran "Sweden and Lars." Jag blir litet nyfiken pa vad som menas med
"Sweden and Lars." I ? ? ? ? ? ?
? ?

>Control Theorist fran Ditt perspektiv. Bill Powers ar en magnifik man som
control theorist from your perspective. Bill Powers is a magnificant man
who

>agnat 35 ar at att beskriva manniskor som "styrsystem." Jag ar
for about 35 years has written much about "cybernetics." I am

>Chalmerist, men har bott har i 25 ar och arbetar inom industri. Nu borjar
a chemist(?), but have now worked for about 25 years in industry. Now I am

>jag marknadsfora ett ledarskaps program baserat pa Powers' modell.
working on marketing a leadership program based on Powers's model.

>Vart elektroniska medium, CSG-l, ar mycket aktivt med en blandning av
This electronic medium, CSG-L, has lots of activity with ? ?
?

>social tillampning och detaljerad teknik, foretradesvis simulering. Om
social interaction(?) and detailed technical ? simulation. If

>Du har fragor, kommer Du att finna Bill Powers en outtrottlig,

you have questions, you will come to find Bill Powers an outstanding,

>mangfacetterad kampe.
multifaceted fight(?).

Not too bad for a first try. But I do miss all the funny little circles you can't get on the e-mail messages.--Gary

Date: Thu May 21, 1992 12:05 pm PST
Subject: Free Will, language...

[From Bill Powers (920521.0800)]

We cut the trip a day short because it turns out that Southeastern Utah is cold and rainy. So is Durango, so maybe we'd better move.

Bryce Canyon is worth an extended look in any weather. It's an amazing example of what nature will do when left in the hands of ordinary physical processes for a few tens of millions of years. Of course they have fences and rules to keep purposive systems from wrecking the place in a couple of years.

Rick Marken --

Actually, THREE LEFTS make a right.

RE: Free Will:

Greg Williams (920517) --

>[Cziko] How could an individual's control structure be
>anything but a "function of its past history?"

>Anyone who buys into state-determined dynamical models won't argue. >Some who don't, will.
>Some of the latter postulate "miraculous" (non-
>history- determined) alterations in organismic physiology due to a >"self" and/or "God," among other (non-physical?) things.

But as Gary said, Chaos ought to make us pause and ask what we mean by "determined." When the behavior of a system for which all parameters and initial conditions are known to 6 decimal places can't be predicted for more than a few hours, the idea of determinism begins to look like an illusion. You can explain present events on the basis of past conditions, but given those past conditions you couldn't have predicted the present events.

I think we can now distinguish between closed-loop determinism and open-loop determinism.

Open-loop determinism is the kind physicists think about. Open-loop determinism can't be demonstrated without introducing multiple chains of integrations that have to remain accurate for long periods of time, and in which the system may encounter chaos-type bifurcations.

Closed-loop determinism is forgiving of changes in initial conditions and

in at least some parameters. It can work in the presence of Chaos. We can predict that a normal driver can keep the car within 2 feet of the intended path and 2 degrees of the intended direction, and that the errors at the end of the trip will be comparable to those at the beginning (that is, if the position is in error by two feet and the direction by two degrees one minute after a trip starts, an hour later the position will still be in error by no more than about two feet and the direction by no more than about two degrees). The systems that determine future conditions the most reliably are closed-loop deterministic systems, systems containing purposes and the means of implementing them.

>Some of the latter [who don't buy into a deterministic system concept]
>postulate "miraculous" (non-history- determined) alterations in
>organismic physiology due to a "self" and/or "God," among other (non-
>physical?) things.

If normal determinism doesn't really work (at least predictively), then to speak of "history-determined" alterations in organismic physiology is almost meaningless. Concepts like "Self" and "God" are no more miraculous in nature than concepts like "input function." They are labels for black boxes whose properties are defined only in terms of what they are understood to accomplish.

After the fact of a purposive action, you can always explain the outcome in terms of antecent events. You can't explain just why just those antecedent events occurred, often in a very long chain involving incidental and unpredicted perturbations and accompany actions that countered their effects.

In order for a Self to create purposeful outcomes of physical processes, it would (according to traditional scientific interpretations) have to forsee the future and adjust the present until the future result matched the intended result. A person using this traditional way of rejecting the Self as a determining entity is thinking, of course, in terms of open-loop determinism. One of the traditional clever ways of "disproving" intention is to say something like "Yeah, but what if a meteorite fell and killed you before you could carry out the intention of opening the door?" This sort of objection assumes open-loop determinism, and makes an intention into a (claimed) infallible prediction. But with closed-loop determinism, intention means only reference level, and does not imply prediction. Accidents of nature don't disprove intention, because a correct understanding of intention does not imply infallible prediction of future disturbances -- or even the ability to resist the effects of every possible disturbance.

The Self can bring about preselected future conditions in spite of normal physical variations that by themselves would lead to a wide range of different future conditions. So the Self acts in a way that is, in traditional terms, miraculous or supernatural.

The traditional notion of open-loop determinism is no longer defensible. So the motivation for rejecting black boxes like the Self or God can no longer be the traditional one.

Gene Boggess (920518) --

Greg William's critique of your proposed CT ethic led you to reframe it,

but I think your whole line of thinking and the exposition of it are admirable and clear. I agree with you that the ethics in question comes down to an analysis of human nature. It isn't that we OUGHT TO avoid conflict; it's that we DO avoid conflict, and the reason we do is simple: those who didn't are no longer with us. Darwin's Hammer. Jesus said that those who live by conflict die by conflict. Smart fellow. Of course he didn't say that ONLY those who live by the sword die by the sword; there's a considerable motive for those who would rather not live by the sword to get rid of the jerks who seem to enjoy it. Maybe it's a good idea to offer the other cheek to be smitten, in case the smiter can be shamed out of violence. But if he smites anyway, the smitee is probably best off, evolution-wise, to say "OK, that's two." Meaning, the third time is going to come out differently. The third time, having been given a chance to prepare, a friend stabs the offender in the back. Unsporting? Dishonorable? No more than insecticide is. Violent? Certainly. You can push any control system too far.

Chris Love (920518) --

>I will use my version of *transport lag* at each level because it is >not difficult to implement and it appears to be useful. This means a >running average of the signals.

A running average isn't a transport lag; it's more like an integration lag. While the output may change more slowly than the input, it starts changing at the same moment the input changes. In a true transport lag, NOTHING AT ALL happens at the output for some time, the lag, after the change in input. A transport lag is observed in listening to a band play 300 feet away in a stadium. You see the conductor's baton going up and down with a transport lag of a few hundred nanoseconds. You hear the music with a transport lag of 1/3 second. If you applied a 1/3-second running average of the music you wouldn't hear much.

To implement a true transport lag, you need to store consecutive values of input in a buffer. At each dt, the contents of the buffer are shifted and the new input is inserted at the beginning. The output is taken from the last (oldest) entry in the buffer. Suppose you want a transport lag of 0.1 second, accurate to 0.01 second. This implies a buffer 10 elements long, and a dt of 0.01 seconds. In practice you wouldn't actually shift the values in the buffer; you'd just use two pointers (one for input, one for output) 10 elements apart, incrementing them modulo 10.

If there is a transport lag in a closed-loop system, negative feedback will become positive at frequencies where half a wavelength is equal to the lag. To prevent the oscillations and still maintain high loop gain, you have to use a smoothing filter (running average) that keeps the feedback from having a loop gain of 1 or greater at frequencies at or above the critical frequency where the 180-degree phase shift occurs.

Bruce Nevin (920519) --

I didn't make my point (discussed later in your post) very well. I was trying to say that where we use "frozen expressions," the machinery for producing language isn't rule driven, but is simply playing back recordings

-- low-level control. But the frozen expressions may AT ONE TIME have been produced by higher-level systems. Certainly, we don't often say or hear "every man for himself." I was just using that as an example of a frozen expression but didn't know what to call it.

In fact I think you make by point better than I did (and I failed to pick up on it), by saying

>This is possible whenever there is "free" variability that is not
>constrained by the contingencies of control--more than one form of
>behavioral outputs can accomplish the same controlled perception. Then
>choice among alternatives (or in the range of free variability) itself
>is exploited as an aspect of self image, or social standing, or
>relationship to others involved in the transaction, etc.

It's precisely when there is more than one way to accomplish the same communication that active control ought to be considered as the model, rather than output generation. The CT model would VARY the choices of output (in imagination or through on-line editing) to produce conformity of the product to reference regularities or rules. When the output can be produced by a fixed rule and permits of only one output format, we should suspect that the forms have become frozen and are being controlled at a lower level. Aren't large parts of even the most creative sentences actually frozen chunks?

I agree with you that we have to tackle simple phenomena first. Along those lines, it would be nice to have some demonstrations of principle as simple as the rubber band demo. Can you (linguists) think of any demonstrations that a CT model could handle but a traditional open-loop model couldn't? The demo could be exceedingly simple and obvious and still serve the purpose of showing the principle -- in fact, the simpler and more obvious the better.

You say

>What I am trying to put forward is a way of thinking about the
>complexity of social transactions, including communication and the use
>of language, in PCT terms.

Every time you do put forward another try at doing this, the result makes more sense and settles nearer to inevitability. All this repetition and going around in circles isn't a waste of time. Often when I come up with an objection, it's not so much an objection to what you mean as to what you can be read as saying, especially by a non-expert in linguistics. When you come back with further explanation, usually the unwanted meanings are gone. This is your reward for being so patient with me -- you're saying what you mean (and I presume refining what you mean) in a more and more lucid way every time we go around. So I can excuse my obtuseness by saying I'm only testing you. Heh heh.

>>When I examine words closely, they seem to be just familiar chunks of
>>sound or objects on paper. The meanings they seem to have (for those
>>words that can individually designate meanings) turn out to be >>ordinary perceptions. So both words and their meanings are ordinary >>perceptions.

> Let's be careful here. What about those words that cannot >"individually designate meanings?" And of those that can, I have >provided evidence in

prior posts indicating that the association of >words to nonverbal perceptions is not a simple matter, but is "dirtied" >by all sorts of historically contingent social conventionalization. >For example, what is the nonverbal perception associated with "on" or >"by"? Is there just one?

I was avoiding words that mean structures of words like relationships among words -- higher levels. My point wasn't that a given word has a given meaning. I was trying to get out of the realm of words and into the more general realm of perceptions, of which words are a subset. By seeing the rules of language as rules of types that we can apply to any perceptions, we can make a try at seeing language as the brain behaving in its natural way, with language as just one outcome. I think there is (in some quarters) too much emphasis on language as something that starts with phonemes and builds up to sentences, and so on. So if a primitive hominid didn't have certain vocal tract structures, it couldn't have had and used language, according to some people. And of course a bird, which can't even say "mama," can't possibly have language.

>I think I'm agreeing with you. You apparently think I'm not. One of >us is missing something.

No doubt. But we don't do too badly at this.

Best to all, Bill P.

Date: Thu May 21, 1992 1:48 pm PST
Subject: Chaos & Determinism

[from Gary Cziko 920521.1615]

Bill Powers (920521.0800) writes:

>I think we can now distinguish between closed-loop determinism and open-
>loop determinism.
>
>Open-loop determinism is the kind physicists think about. Open-loop
>determinism can't be demonstrated without introducing multiple chains of
>integrations that have to remain accurate for long periods of time, and in
>which the system may encounter chaos-type bifurcations.

>Closed-loop determinism is forgiving of changes in initial conditions and
>in at least some parameters. It can work in the presence of Chaos. . . .

I find this distinction very helpful. But aren't there open-loop deterministic systems that are able to fend off chaos, at least to some degree? I'm thinking of things like planetary motion. I understand that to some extent there are chaotic things happening there, but nonetheless the planets stay in their "intended" orbits. How are these open-loop systems able to maintain this consistence of behavior? I realize, of course, that if the planets DIDN'T have just the right position and velocity and direction they wouldn't be there for us to marvel about (and we wouldn't be here on earth marvelling). But given that they are there, how do they manage to hang in there for so long in spite of the unpredictable buffeting they encounter from the passage of neighboring planets, comets, solar wind, etc. Is it just simply that the disturbances

are not great enough, like wind on a heavy pendulum which may move chaotically to some degree but on the average contiues to point downward?

> We can
>predict that a normal driver can keep the car within 2 feet of the intended >path and 2 degrees of the intended direction, and that the errors at the >end of the trip will be comparable to those at the beginning (that is, if >the position is in error by two feet and the direction by two degrees one >minute after a trip starts, an hour later the position will still be in >error by no more than about two feet and the direction by no more than >about two degrees). The systems that determine future conditions the most >reliably are closed-loop deterministic systems, systems containing purposes >and the means of implementing them.

This may impress the car driver, but may have less impact on the railroad engineer. He can bail out of a moving train and the train will no be more than a couple inches (if that much) in error after many hours.

Illinois has been warm and sunny. When are you and Mary moving back here?

--Gary

Date: Thu May 21, 1992 7:30 pm PST
Subject: Determinan & Determinism

From Greg Williams (920521-2)

>Bill Powers (920521.0800)

>But as Gary said, Chaos ought to make us pause and ask what we mean by >"determined." When the behavior of a system for which all parameters and >initial conditions are known to 6 decimal places can't be predicted for >more than a few hours, the idea of determinism begins to look like an >illusion. You can explain present events on the basis of past conditions, >but given those past conditions you couldn't have predicted the present >events.

Bill, I know that you know that I wasn't arguing for a "predictable determinism" in nervous system operation, but just to make it perfectly clear to everyone else:

Dynamical systems models, AKA state-determined dynamical models (of which PCT appears to be a species) can be EITHER deterministic or non-deterministic (i.e., stochastic or even purely random). What distinguishes them from (certain types of) "miraculous" models of the way parts of the world work is that there are rules relating past "states" of the systems to future "states." That is, the basic postulate is that the current "state" of a system is completely dependent on the history of previous "states," via the (possibly random) connecting rules (the "dynamical laws"). Now, for those who buy into dynamical systems, whether deterministic (by which in THIS context is meant NON-RANDOM) OR stochastic OR purely random, and whether open-loop OR closed-loop, the historical determination of the current state of a system remains. That is, taking a PCT circuit as an example dynamical system, the current behavior/actions are (possibly unpredictable even in principle, if you buy into certain interpretations of quantum mechanics, and almost certainly unpredictable in practice, even if chaotic dynamics are not to be found in the

circuit) functions of the system's "state" history.

My point in bringing up the history-determination of current behavior/actions of PCT circuits was to counter claims that PCT supports a notion of strict individual autonomy. In general, dynamical systems which interact with other dynamical systems have "state" time-trajectories which depend on those interactions as well as on the systems' structures themselves, and do NOT have miraculous changes in state, notwithstanding quantum fluctuations which some treat as miraculous in the sense I am speaking of. Even with a "miraculous" q.m. and/or stochastic dynamical laws and/or purely random laws, there is no "Self"-like effector capable of "state" trajectory alterations which are "deliberate" yet not functions of history.

Gary is particularly concerned about the implications of PCT regarding manipulation by others. I've already said that PCT casts considerable doubt on Skinnerian optimism about the potential for success of deliberate manipulation of others -- even without chaos. But at the same time, PCT (unless augmented as per my earlier posts) lends no credence to claims of traditional free-will as exercised by a moral agent.

>Accidents of nature don't disprove intention, because a correct
>understanding of intention does not imply infallible prediction of future
>disturbances -- or even the ability to resist the effects of every possible
>disturbance.

Agreed. Now tell me what intention is in PCT models besides the operation of closed-loop dynamical systems in interaction with their niches?

>I agree with you [Gene B.] that the ethics in question comes down to an
>analysis of human nature. It isn't that we OUGHT TO avoid conflict; it's that
>we DO avoid conflict, and the reason we do is simple: those who didn't are no
>longer with us. Darwin's Hammer.

Models of human nature (and, in particular, PCT models) indeed can help to explain WHY individuals choose the particular ethical systems they adopt. But such models (including PCT models) can't JUSTIFY any particular systems. To say that the models CAN justify some system is to try to get from "is" to "ought." I don't think even conjurers can pretend that.

There are three major points I have tried to make in recent posts.

1. Current PCT models provide no support for traditional notions of a "Self" with "free will." In this claim, I am neutral with respect to claims about "predictive determination."

2. Current PCT models suggest that deliberate manipulations of an individual by others are less likely to succeed than Skinnerians think.

3. PCT models, without added normative postulates, cannot justify any ethical systems.

Greg

Date: Fri May 22, 1992 4:29 am PST

From: Hortideas Publishing / MCI ID: 497-2767

TO: * Dag Forssell / MCI ID: 474-2580
Subject: PL course comments

Hi Dag,

Comments on the materials you sent me...

Cassette Tape Script (intro):

4 - I greatly appreciate your deft steering, right at square one, between the extremes of environmentalism and autonomy. I wish ALL PCTers (both in theorizing and applying) would do the same.

5 - McFarland, Powers, and Clark were preceded by Wiener and his colleagues, who wrote the seminal paper ("Behavior, Purpose, and Teleology," PHILOSOPHY OF SCIENCE 10(1), January 1943, 18-24). I suggest that you take a look at this earliest published statement that intention = negative feedback control.

12 - Have you kept a running percentage on how many rubber-banders have explained what is going on? If so, is it actually only around 10%?

12-13 - Possibly you are making trouble for yourself, unnecessarily, by emphasizing that PL shows "what goes on with one or more persons in an environment... clearly and explicitly." It isn't that you WON'T do this, but some course-takers might be led to expect rules for how to deal with interpersonal situations at a highly detailed level... their minds might be blown, given their expectations, when you discuss the poor predictability of human actions and the inability to set specific rules to cover all situations in general. Perhaps here you could also emphasize that there won't be "cookbook" solutions (and explain why, using the rubber-band experiment for a demonstration of the inappropriateness of rigidity in the face of disturbances).

13-14 - I like the order of presentation of the basic applications (sometimes you say "application") program!

14-15 - You certainly have your work cut out for you on the second and third days! Maybe you are even heading into uncharted territory on Day 3? Good luck... I'm just glad that I'm not the teacher. No criticism here. I'm impressed by the scope your undertaking!! These folks are getting a LOT for their money!!! (And you can quote me on that.)

1. BASIC APPLICATIONS:

2-21 - I would say that one CAN conclude SOMETHING from "behavior alone" (actually, "actions alone" is more appropriate here) but not nearly as much as has been claimed in the past. (Maybe you should talk about the action/behavior distinction in PCT here.)

2-23 - "People are predictable" using PCT perhaps overstates the case a bit? Again, consider the action/behavior distinction here?

2-38 - Excellent!

3-1,2 - Ditto!

3-3 - What's "healthy" competition???

Overall, I am extremely favorably impressed. I suspect that your program will make at least some other management training programs look shoddy and even claptrap. I recommend a fairly structured course evaluation procedure at the end, rather than an unstructured debriefing -- try to get as much info on what you did right and wrong from your star pupils as you can (after all, it is their errors which you are trying to keep small!).

I'd be happy to clarify my comments and/or discuss other aspects of your program at any time.

Greg

P.S. "Focussing" is an acceptable variant spelling of "focusing," and Bill spelled it the former way in that paper. As I said in the "Note on the Text," I tried to make spelling consistent WITHIN each paper, but not AMONG the papers (seems to me Bill spelled it with one "s" in at least one other paper. Your copy of MIND READINGS will be mailed on Monday (our regular day to go to the post office); thanks for the order!

Date: Fri May 22, 1992 8:55 am PST
Subject: "standards" and bugs

[From Rick Marken (920522 08:40)]

The computer here will be down throughout the vacation and I will be out of town until next friday. So if you have any great thoughts about "standards" or other things that I should go off and mull over, you'd better post them today before 15:00 PDT.

One thing that did occur to me that might be worth mentioning is the relevence of this discussion of "standards" to one aspect of the Beer bug discussion of long ago. For me, one of the most interesting "revelations" of the Beer bug discussions was that rather complex output systems can be involved in the control of rather "simple" sensory inputs. For example, beer's bug had a rather complex, largely open loop gait generation system that is really a means for moving the bug so that it could influence (and control) some simple sensory inputs (like the degree of bend in an antenna -- a unidimensional variable). What is interesting to me about this is that an observer of the bug will see some very complext behavior that could be described in all kinds of complex ways (in terms, for example, of spatial variables that are not even perceived by the bug). Yet, the actual "behavior" of the bug (from a PCT point of view) is that it is keeping an intensity input at a reference value.

The relevence of this to "standards"? I think when we look at social behavior we are in a similar position to the observer of Beer's bug. We see all kinds of interesting things happening -- but it is very difficult to see the possibly very simple sensory variables that are being controlled. PCT is an attempt to help us see beyond our interpretations of behavior -- to what behavior is really about; control

of perception. And this requires a special kind of looking (based on hypotheses about what variables might be controlled) and testing (to see if disturbances to the variable are resisted). Just as we can read a lot into the bug's behavior that is not relevent to what the bug is actually doing (controlling) (for example, we see the bug "navigating" and "exploring" in two-space, when, in fact, all it is "doing" is keeping the value of one or two input variables at fixed reference values) so we may also be reading a lot into human behavior that is not relevent to what a person is controlling -- for example, we say a person has "bad manners" or "poor standards" when they eat with their mouth open -- when, in fact, they may just be controlling the amount of pain they feel because they have a toothache.

It is hard to get past our inclination to see behavior as "output". We assume that what we see is what the person is "doing". PCT suggests that we must TRY to get over that inclination (if we want to understand behavior) and take seriously the proposition that what we are seeing (when we see people "behave") is the means by which people are keeping their OWN perceptions matching their own references for these perceptions. We, as observers, CANNOT see what another person is perceiving or trying to perceive. We can only try to get an idea what a person might be trying to perceive by doing the test for the controlled variable.

What seems to an observer as control of a complex principle (at the "wrong" level with respect to the observer) may, in fact, be nothing more than efforts to get from point A to point B in the context of variable disturbances.

I've still got to work up a demo of this. Though Dag Forssell does an excellent job of illustrating this point with his wonderful variations of the "rubberband" demo; extroardiarily complex "behavior" seems to be going on when people are doing nothing more than trying to perceive a simple relationship between configurations -- "knot on dot".

Best regards Rick

Date: Fri May 22, 1992 9:14 am PST
Subject: Miracles, determinism, and free will

[From Bill Powers (920522.0900)]
Interchange between Greg Williams and Gary Cziko:

Greg:
My central claim is that the moment-to-moment operation of an individual's control structure is NOT MIRACULOUS, but rather a function of its past history (perhaps in a probabilistic way -- I have no desire to get into questions about "absolute" determinism).

Gary:
Who could argue with this? How could an individual's control structure be anything but a "function of its past history?"

Greg:
>Anyone who buys into state-determined dynamical models won't argue.
>Some who don't, will. Some of the latter postulate "miraculous" (non-

>history- determined) alterations in organismic physiology due to a
>"self" and/or "God," among other (non-physical?) things. I was just
>trying to make sure that everyone realizes that such postulations are
>ADJUNCTS to PCT.

Point 1:

If a miracle occurs, it becomes part of the history of the affected system. To call something a "miracle" is only to say "I don't understand how that could have happened." To a believer in lineal cause and effect, the unlikely event of the knot between two rubber bands staying nearly stationary while two people wrestle the ends of the rubber bands around is scientifically unexplainable: it is a miracle. The fact that the environment reinforces organisms for doing what is good for themselves is a miracle.

In fact, gravity is a miracle, radioactive decay is a miracle, the constancy of the velocity of light is a miracle, quarks are miracles, the Big Bang is a miracle. There are no physical explanations for such phenomena: they happen or exist, and that is all we can say about them. We can hypothesize causes for such phenomena such as luck or divine intervention, but we still don't understand how the phenomena came to be.

Miracles are things we can't explain. To call them miracles is normal human egotism; it is a subtle claim that nobody will ever understand more than we do. Every generation since the dawn of history has thought it had finally reached the ultimate of human achievement and understanding. This was true, of course. Only it was just a position on a slope.

Point 2:

The concept of a "state-determined system" comes, I believe, from LaGrange. Give me a description of the universe at a given instant, and knowledge of all natural laws, and I will tell you the state of the universe from then on. This is what physical determinism is about, isn't it?

Quantum mechanics put the first nail in the coffin; Chaos screwed the lid down.
Control theory threw the first handful of dirt into the grave of LaGrange.

The problem, as we now know, is that the LaGrangian concept works only backward. Given the present state of the universe and knowledge of all natural laws and intervening events, I could tell you pretty much what the state of the (local) universe was a week ago. But given the state a week ago, I could not have predicted the local state today. That's because of hypersensitivity to initial conditions, which occurs in enough phenomena to make sure that no measurement of initial conditions can be precise enough to distinguish the actual trajectories from the possible trajectories of all variables indefinitely into the future. Looking backward, we can see how the confluence of various forces led to the present state of affairs; the initial conditions must have been within some very narrow range. But picking sets of initial conditions at random from within that narrow range, we would often predict very different outcomes from each set, even though the sets differ only infinitesimally -- or not at all. In the limit, we find that infinitesimal changes of initial conditions can often lead to finitely different outcomes: the famous "bifurcations" of chaos theory, the butterfly effect. If this can happen for some phenomena, there is a finite horizon past which no prediction is even in principle possible. For complex

phenomena, this horizon can be measured in days, or even seconds, depending on the details.

Control theory introduces a type of physical organization that can eliminate all but a narrow range of the outcomes of chaotic and randomly perturbed processes. What determines the outcome is then not "history", but the present organization of the controlling living system. Once the fact of control became established in one part of the physical universe, that part became able to preserve the kind of organization that controls, and passed along from generation to generation this ability -- the ability to predetermine outcomes independently of the laws governing the rest of the universe, the ability to manipulate physical variables so that their lawful effects are those intended by the controlling system.

This phenomenon was not imagined by LaGrange. LaGrange imagined blind natural forces that could produce only the outcomes they produced, so that only one outcome was ever possible. He knew nothing about hypersensitivity to initial conditions (although he could have figured it out by contemplating his mathematics a little further). He certainly did not understand purposive behavior -- in his day, that was strictly a religious or mystical concept in the realm of miracles. To LaGrange, as I understand his ideas, physical determinism was absolute and fixed forever. This idea can no longer be supported.

Control theory provides the final justification for "free will." Free will is not a phenomenon of causation, but a phenomenon of organization. It isn't a very good term for this phenomenon, but it's the best people could do by way of describing their relationship to the nonliving world. Nonliving objects are clearly constrained to behave if and only if, and exactly as, present-time external influences cause them to behave. Within the horizon of prediction, their behavior is explainable by the LaGrangian hypothesis. In fact, history has no influence on them, because one does not need to know the history of the universe to predict the effects of current causes. All that is needed is a description of unchanging natural law, and of the current state of affairs. How that state of affairs came to be is irrelevant.

But moving among these objects and interacting with them are objects of a different kind, which carry purposes within themselves and are thus capable of diverting the flow of cause and effect in the nonliving world to produce consistent and repeatable consequences in a highly "unnatural" way. No longer is there hypersensitivity to initial conditions; in fact, even rather large differences in initial conditions make no significant difference in the outcome, just the opposite of processes in the nonliving world. The effects of disturbances, systematic or random, are systematically counteracted to keep the outcome from straying far from the intended one. Certainly, the effects are sufficient to keep the outcomes far more consistent than they could have been without the intervention of living systems. As a result, the horizon of predictability is extended far beyond the limits of a purely Lagrangian universe. Prediction becomes a matter of understanding intentions. In my case, an intention formed nearly 40 years ago is still determining outcomes, despite four decades of random perturbations and tendencies of natural processes to branch and branch again as they pass natural bifurcation (or multifurcation) points.

The term "free will" does not designate something that exists in an objective universe (or not) and which we then must prove (or disprove) to

be something real. It's an attempt to describe an experience. Control theory, by showing how purpose works, suggests a clearer view of this kind of experience. It supports the idea that this phenomenon is not shared by all physical structures in the same way physical properties like mass and energy are shared. It is something unique to living systems, giving them hegemony, for better or worse, over all nonliving objects, and even over the course of natural -- but nonsentient -- events.

Best, Bill P.

Date: Fri May 22, 1992 10:11 am PST
Subject: Baby Update

[From: C. Love (920522.1200)]
[To: B. Powers (920521)]

Bill is describing transport lag to me and says,
>To implement a true transport lag, you need to store consecutive values of
>input in a buffer. At each dt, the contents of the buffer are shifted and
>the new input is inserted at the beginning. The output is taken from the
>last (oldest) entry in the buffer.

Ok Bill. Sorry about the mistake. It is a easy change to make in the code. I am just wondering about\the smoothness of the output. I have passed the weighted percepts and references both through a sigmoid (+- 0.5 range) and the error signal through another sigmoid (+- 1.0). This gives me a dynamic range for the reference output of about +- 0.5. Do you feel that it would be wiser to subtract the weighted percept sum from the weighted reference sum directly without using sigmoids directly on these summed inputs? Granted the percept output would have to be sigmoided somewhere along the way before it was passed along to the higher elementary control modules (ECMs).

In your earlier (1979) paper you say in the Fig. 14 caption,

>These signals are given quantitative weightings by the S matrix and
>summed in the input function FNI of the system to create the perceptual
>input.

>The error signal is amplified and smoothed by the output function FNO

>with the result....

So does FNI and FNO use some sort of nonlinearity, i.e., sigmoids? I know you treat the reference weights differently, i.e., {-1,0,+1}. Do you sigmoid the internal reference signal though before subtracting it from the internal percept signal? From fig. 14, it does not appear you do this? If you do it for
the percepts then why not do it for the references?

I was talking to one of my colleagues here and he suggested that it may be wise to keep the temporal sum idea while still keeping this pure delay you state is necessary. He suggested that if I had a buffer of length 4, then for the first three trials null output would be transmitted to the reference output pin, but on the fourth shift the temporal summation would "kick-in".

The reason I want this is because it smooths the output , which is what I think you meant when you said,

>To prevent the oscillations and still maintain high loop gain, you have to
>use a smoothing filter (running average) that keeps the feedback from
>having a loop gain of 1 or greater at frequencies at or above the critical
>frequency where the 180-degree phase shift occurs.

Am I correct?

In terms of progress, as you may surmise I have created the necessary routines to create, connect, and evaluate the elementary control system (ECS). Things move quite well when developing in Prograph. I highly recommend it to anyone who has to develop something on this scale.

With everyone's advice I am trying to determine a good "learning" algorithm to use, which will serve my purpose. So if anyone does have some suggestions, don't hesitate - let me know. I appreciate all.

I will keep everyone posted of my progress. By the way Bill, I really can see the usefulness of your "put up your feet and teach the baby" routine that uses the grid system. I like.

Thanks again for everyone's help,
Chris Love.

Date: Fri May 22, 1992 12:04 pm PST
Subject: anything goes

[From Francisco Arocha, 920522, 2:17]

[Mary Powers 9205.12,13,14,15,etc.]

You said:

>This goes back to a passing comment you made three or four weeks
>ago about the "anything goes" philosophy of Kuhn, etc. What he
>said was that scientific progress can't be judged as approaching
>closer to the Truth, and that various earlier versions of science
>continue to be used and are perfectly valid (earth-centered
>astronomy for surveying, heliocentric for launching spacecraft,
>etc).

and

>Science, Kuhn says, has also evolved. But not towards the Truth,
>whatever that is. Its evolution is also FROM some prior state.
>And the criteria for valuing one scientific scheme over another
>are internal to (some) humans, and not external. That is what (I
>think) you construe as "anything goes". What's valuable is up to
>us, not to some cosmic principle.

In my post about the philosophy of science exemplified by the works of people like Kuhn, Feyerabend, and Lakatos I made a

reference to the "anything goes" idea (Feyerabend) that in science there are no criteria for evaluating hypotheses or theories. More specifically, that correspondence with a state of affairs out there is not such a criterion. In other words, that there is no truth. I agree with this if it is meant that there is no ETERNAL truth. Unfortunately, it is meant in the sense that theories or hypotheses must not be evaluated by their correspondence with the external world. That, for instance, earth-centered and helio-centric astronomy are both true. The first was true in its world, centuries ago and that the latter is true now.

Viewing this as coming closer to the Truth presupposes that the Truth is out there, but truth is a property of some propositions, not of the world. Science may no be evolving towards the Truth, but not all "scientific" theories are equally true. The proposition that the earth is the centre of the universe may have been useful (and it may still be), but the point is that it is factually wrong. The universe does not consist of an earth around which the celestial bodies revolve. All this points to the idea that there is an external state of affairs, independent of our wants and wishes, against which we must check our hypotheses and theories. The TRUTH may not exist, but true (and false) propositions (and theories) certainly do.

What I said about the "anything goes" refers also to the idea that there is no objective knowledge and that the criteria for choosing one or another theory must be based on things that are external to science, such as popularity, convention. I acknowledge that those things are influential on which theories are taken to be the best, but there has to be some sort of correspondence with the reality out there for any theory to be credible. After all we don't run surveys or polls to figure out which theory is better. We do experiments. So in a sense there must be true propositions. Otherwise, it become pointless to test our theories.

Science presupposes the existence of an external reality to which our hypotheses and theories, if true, should match. This may not be a simple process of moving closer to an eternal Truth; and most of the time, or at least many times, our hypotheses and theories are wrong. But this is no reason to assume that there is no external reality or that there are many different "worlds" or that science should be "evaluated" by irrational means.

That irrational elements are present in many (probably most) human actions is a matter for factual science to deal with and study. However, from the fact that sometimes scientists behave irrationally, we cannot conclude that science is an irrational process, as Kuhn suggests. No matter how incorrect the positivists were in "reconstructing" science, theirs was a constructive enterprise: they wanted to build, not destroy. Contrarily, most post-positivist philosophies have gained their popularity by attacking positivism without going into the pain of building a new philosophy. When they do try to build something is mostly vague and almost always metaphorical.

If we want our sciences to succeed the way the hard science have, I think, we must be very critical of vague, verbal philosophies, which regardless of some of the truth they have, simply confuse things. This is especially the case for the now fashionable irrationalist philosophies which Ferrater Mora has called "textualism" (i.e., the world is a book). If we, as scientists, are critical of scientific theories because they are vague or metaphorical, we must also be critical of philosophies that are vaguely expressed and metaphorical. There is no distinction in the way science and philosophy should be evaluated.

Regarding Kuhn's book itself, the idea that science is not "the steady, cumulative acquisition of knowledge" but "it is a series of peaceful interludes punctuated by intellectually violent revolutions" (from the back cover of "The structure of scientific revolutions"), is not new. Fifty years before its publication, the french philosopher Gaston Bachelard was talking about "ruptures epistemologiques" in science, by which he meant scientific revolutions.

Again, I would like to draw the attention of philosophically-minded CSGers to the works of Mario Bunge. He has develop philosophical theories of science, which I think are much more important, rigourous, and useful than the current philosophical fashions.

F. CYBN@MusicA.McGill.CA

Date: Fri May 22, 1992 12:22 pm PST
Subject: Autonomy

[From Bill Powers (920522.1100)]

Greg Williams (920521) --

>My point in bringing up the history-determination of current >behavior/actions of PCT circuits was to counter claims that PCT >supports a notion of strict individual autonomy.

Let's go into the meanings we might give to "autonomy." I use the term in a way that has a rather complicated meaning with a basis farther back than current behavioral interactions with the current environment. I'll slip in my hypotheses about the role of control without marking them; I'm sure others will be able to tell what is hypothesis from what is generally-accepted "fact" in this story.

Start with DNA. While the surface appearance is that genetic characteristics are transmitted via the DNA molecule, in fact a lot more passes from generation to generation than just DNA. Much cellular material, as in mitochondria, is passed along with the DNA through the mother's egg; in the lowest orders, the cellular material simply divvies up during the reproductive divisions. Immediately after a new individual is launched, the DNA is in an environment that is continuous with the previous environment, at least locally.

So the biochemical control systems whose reference signals are carried in

DNA can operate right across the boundary between generations. These control systems, finding themselves isolated, begin again building the control systems that build the control systems that build the control systems that constitute the adult organism. The entire milieu interieur (sp?) is regenerated, with whatever changes occurred during the division of genetic material. The continuity proceeds, as people have long suspected, through the mother.

One of the final products of this process is the a set of intrinsic reference signals. These reference signals are the basis of reorganization or learning through which the new organism establishes control in the environment it first and subsequently encounters. The intrinsic reference signals represent the target states of some as yet poorly defined set of variables critical to the survival of the individual. There is no reason to think that the reference signals are identically set from one person to the next, or even that they are all of the same kind. Each individual differs in details of organization at all levels from DNA through cellular through organ-structures through gross bodily structure through neural circuitry. And the mix of intrinsic reference signals will differ from individual to individual.

Intrinsic reference signals are part of a system, probably distributed rather than lumped in one place, that controls for zero intrinsic error. The means of control is blind variation of the organization of the nervous system and the biochemical control systems. Reorganization is driven by intrinsic error, and ceases when intrinsic error drops below some threshold.

As a result, the organism acquires control systems that can maintain perceived aspects of the external world at learned reference levels by means of motor behavior (and at the biochemical level, changes in such things as strength, speed, organ size and activity, and so on). The criterion for acquiring any behavioral control system, and for setting its reference signal to any specific value, is that intrinsic error be maintained at the lowest possible level.

Thus the overriding concern of the reorganizing system, and the purpose for which it causes any behavioral organization to appear, is to control its own basic physical state; to maintain its component variables at endogenously-determined reference levels. It neither knows about nor cares about nor CAN care about any processes external to the body. Everything it causes to be done by way of interacting with an external world is done for the purpose of controlling an internal state. It is therefore completely and absolutely autonomous in its purposes.

It does, to be sure, have a history. But this is not so much a history of antecedent events as it is a history of gradually changing organization. The reorganizing system of one generation is continuous with the reorganizing systems of previous generations: it is the same system, evolving. At the center of this system are reference signals that have not changed in billions of years, having survived even speciation. Reorganizations that preserve these basic reference signals have led to the development of instrumental reference signals and associated control systems, and those have led to still more elaborate control mechanisms, and so on to the various physical forms that life has ultimately adopted -- as a means of preserving the fundamental function, which is to control. And to control is the ultimate meaning of being autonomous.

If the criterion for stopping reorganization is bringing intrinsic variables to their respective reference levels, it follows that only those behavioral control systems will survive reorganization that do entail actual effects of the right kinds on the intrinsic variables.

The effect of any given behavioral act is not determined by the organism: it is determined by the nature of the surrounding world (including the behavioral organization of other organisms in that world). So reorganization can't cease until the actual effects on intrinsic states, via that external world, are correct for maintaining zero intrinsic error.

Thus the organism learns first what variables are critical to perceive in that external world, and second what specific states of those variables are critical to maintain. This process of learning has been going on through geological time, with the appearance of control structures of greater and greater generality, and what we recognize as higher and higher levels of control. As each new level of control appeared, new and more important aspects of the environment became perceivable and came under control by the organism. The actions of the organism adapted themselves to the environment in more and more subtle ways.

The means of action did not change nearly as much as the neural control systems that use actions to control ever more complex variables. A human being and a monkey share nearly identical means of motor action. Both have hands at the ends of jointed limbs; but the human being can accomplish things with its hands that a monkey cannot. This is not because of having an opposable thumb, but because of having higher levels of control. Human beings can do more even with their thumbs cut off than a monkey can do with ten digits.

So we arrive finally at the question of autonomy in the individual human being. Autonomy is clearly not freedom from physical constraints (which include, in the final analysis, social constraints). The environment, not the organism, dictates the effects of any given action. But the environment does not dictate the desired consequences of any action. It is the organism that chooses those consequences, and learns how it must act in order to produce them.

In a hierarchical control system, built, I presume, level by level over the eons and recapitulated in the individual, the lower systems give up their autonomy to the higher systems that manipulate their reference signals. At whatever level is currently the highest, the reference signals are set from within the organism by the process of reorganization; the purpose of choosing a particular setting is to maintain intrinsic error as close to zero as possible -- as the purpose has always been. In order to bring the highest level of perception into a match with this autonomously-set reference signal, the highest control systems must, as usual, be altered to produce actions which are among those that will have the required effects. Now those actions are determined by properties of the existing lower levels as well as by the characteristics of the world external to the organism.

The organism can't choose what properties the external world will have, no matter what the level of perception. Once its lower levels have been built and brought into mutual harmony, the organism has less than a completely free choice even as to the kinds of actions it can produce (without starting again from scratch, which is probably no longer possible in the

adult organism, in the time remaining to it). So the particular behavioral organizations that appear in the adult are shaped by the properties of the world around it and by the properties of its own already-acquired lower levels of control.

However, the highest levels of reference signal remain autonomous and are changed only in service of maintaining the individual organism's mix of intrinsic variables at their unique mix of reference settings. The external world has no influence over that basic requirement. Intrinsic error remains the organism's sole criterion for judging the value of any aspect of its experiences. This is true of all organisms from the amoeba to the human being.

If the highest levels of reference signals are autonomously determined, then the next-to-highest levels of reference signals are varied so as to prevent the environment (as perceived through all the lower levels) from making the highest perceptions depart materially from their reference settings. This means that the next-to-highest levels of perception will also be shaped to meet the requirements of the highest reference signals.

But the next-to-highest reference signals will be determined by what the environment requires, for the highest perceptual signals in general contain effects of uncontrolled elements. To make the net result match what the highest system requires, the reference signals for the controllable parts of the next-to-highest world must be varied, and those variations much be matched to the properties of the lower systems and the external world. The organism can't choose the settings freely, because only certain settings will result in the required perceptions. There may be many alternative settings that will produce the required perception, but there is freedom to choose only among those alternatives, given that the highest reference signal is to be satisfied. All other alternatives are ruled out by properties of the external world.

The general picture is that the environment determines behavior, while the autonomous organism determines consequences of behavior. Given the intended consequences, the environment sets the limits as to what lower-level actions can in fact bring those consequences about.

So we can see where autonomy begins and ends. It is the organism that selects consequences that keep its intrinsic errors as close to zero as possible. It is the environment -- and other organisms in it -- that determines what actions must be produced in order that those consequences be brought about and maintained. The external world sets the stage on which existence is played out. But the reorganizing system writes the play.

And even the reorganizing system is just the product of a deeper control process, at the core of which lies a tiny and unimaginably ancient spark of purpose that makes life different from everything else.

Best, Bill P.

Date: Fri May 22, 1992 1:11 pm PST
Subject: Little Baby model

[From Bill Powers (920522.1330)]

Chris Love (920522) --

> I have passed the weighted percepts and references both through a >sigmoid (+- 0.5 range) and the error signal through another sigmoid >(+- 1.0). This gives me a dynamic range for the reference output of >about +- 0.5. Do you feel that it would be wiser to subtract the >weighted percept sum from the weighted reference sum directly without >using sigmoids directly on these summed inputs?

Did you say what you mean? It sounds as if the error signal and the percept signal are inputs, with the reference signal as output. If so, you'd better draw a diagram to show the organization you're using.

The sigmoid is an interesting idea -- it will put a realistic nonlinearity into the perceptual signal. If the reference signal were derived from previous recordings of the perceptual signal, it too would automatically have the required sigmoid nonlinearity it in; when you generate it directly, that isn't necessary. The loop gain will simply fall off for large error signals, which may well help stability. The controlled variable will be the inverse sigmoid function of the reference signal, when error is zero. Go ahead and try the sigmoid; I'll be interest to see what happens. You might also try one in the output function -- I predict that it will have remarkably little effect.

> So does FNI and FNO use some sort of nonlinearity, i.e., sigmoids?

No, it's a dynamic filter of a very simple kind. Suppose you have a function that generates, basically, y = k*x, a simple amplifier. If you want to put dynamic slowing into it without changing the steady-state proportionality of output to input, you can do it this way:

y := y + (k*x - y)/S (a program step, not an equation)

The parenthesized part is the difference between the current value of y and the target value which is k*y. A fraction 1/S of this difference is added to the old value of y to produce the next value of y for the next iteration. The result will be that on successive iterations, the value of y will approach k*x asymptotically, approximating a curve of the form 1 - exp(-kt). This is like putting in a single-pole exponential filter, which leaves the steady-state amplitude of the output alone, but attenuates an alternating frequency by an amount proportional to frequency.

The optimum value of S, which makes the output reach a steady state in one iteration, is 1 + G, where G is the loop gain of the closed-loop system in which this step is the only filtering step. If you want control systems with a loop gain greater than 1 (negative), you have to use this slowing factor to get a stable system, assuming there are no other lags in the loop. It's a quick and cheap way to stabilize models. See my 1979 article in Psych Rev., "A quantitative analysis of purposive systems," for more details.

Actually, you don't really need the true transport lag to get a reasonably good model. We hardly ever use one in models of tracking behavior, because they add so little to the predictivity that it isn't worth the bother. I didn't use any in the Little Man. But you will need the slowing factor if you don't have an element somewhere in the loop that limits the speed of

change of the variables. Otherwise you'll find that the system is unstable for loop gains greater than 1, which wouldn't give you much control.

A slowing factor in the input function (in addition to one in the output function) can add some realism to a model of tracking behavior, by creating a slight overshoot response to a step disturbance. The "standard" human transfer function used by engineering psychologists uses two lags, one with a longer time constant than the other, as the best model. I didn't use this in the Little Man, either.

The most important thing is to get a model running and play with the parameters to see their effects. That's how I taught myself real control theory. Books don't give much help to the intuition.

A learning algorithm we have used successfully (Tom Bourbon has used it, and owes us a description of it) is based on the E. coli mode of steering.

Suppose you want the system to find its own best output gain (or integration) factor. The criterion for "best" would be some measure of changes in absolute error. Define a delta that can be randomly selected from a range from -1 to 1. This delta, multiplied by a very small number, is added to the current value of the output gain on each iteration. The adaptation algorithm simply chooses a new delta at intervals that are inversely proportional to the rate of change of average absolute error signal (plus a constant to avoid infinite intervals at zero error). If the rate of change of average error is positive (getting worse), a new delta is chosen after a short interval. As the rate of change of average error gets smaller and goes negative, new random deltas are chosen at longer and longer intervals. The result is that changes in a bad direction are quickly followed by a random change of direction, while changes in the right direction are allowed to continue in that direction for a longer time before a random change. The end point will be a series of random changes near the optimum value of the parameter. Or you could decide that when error is below some threshold, the deltas cease to be added. This is a surprisingly efficient process, and it can overcome reasonable local-minimum problems, given time.

I want to try this method for stabilizing the entire Little Man model. I think the key to success will be to find, for each parameter that needs adjustment, some measure of performance that is somewhat independent of the measures used for adjusting the other parameters. This is like a concept of a distributed reorganizing system that monitors many different intrinsic variables and affects local parts of the system accordingly (and appropriately).

>By the way Bill, I really can see the usefulness of your "put up your
>feet and teach the baby" routine that uses the grid system.

I'm getting old, Chris. Did I say that? Are you sure it wasn't someone else? How about a little fuller description for my failing memory?

Date: Fri May 22, 1992 1:14 pm PST
Subject: Re: anything goes

Francisco (direct):

>.............Again, I would like to draw the attention of
>philosophically-minded CSGers to the works of Mario Bunge. He has
>develop philosophical theories of science, which I think are much
>more important, rigourous, and useful than the current philosophical
>fashions.

Could you send me some references of his most important works concerning the philosophy of science?--Gary

Date: Fri May 22, 1992 1:14 pm PST
Subject: Standards; Plasticity of top-level Reference

[Jeff Dooley 920522.1000]

(Bill Powers 920514b)

>The only reference signals (and perceptions) that can't be
>changed freely as required by higher levels are systems concepts.

As you point out, output of higher-order control systems enables the plasticity of the lower-order system reference values. But then there is an apparent problem, illustrated in your comment above, which stems from our apparent inability to go up a level so that systems concepts can have adaptable reference signals too. If a world-view can be considered a systems concept (in the sense that it can organize principles), then how can we explain the possibility that a "world-view" can change (Ptolemy vs. copernicus, etc.).

Difficulties like these have surfaced in philosophy of science recently surrounding attempts to provide a normative epistemology within the scope of a purely scientific (naturalistic) study of how we come to know anything. This problem boils down to how we can derive norms or prescriptive standards from an apparently descriptive account of epistemology.

I think that some of the work of Larry Laudan may help clarify these questions as they may pertain to system concept reference adjustment.

In Laudan's _Science and Values_, (U. Cal. Press, 1984) he proposes a circular causal loop in which AIMS, THEORIES, and METHODS are all recursively interdependent and plastic as a function of incremental changes among one another over time and practice. An important point in Laudan's model (he calls it "the reticulated model of scientific rationality") is that the thing is grounded in practice. One way this grounding is achieved is through the actual realizability of aims through methods. Methods are in turn constrained by theories while the theories are justified by the methods. The thing is entirely circular and intended to self-correct over time as each vertex of the triangle impinges upon each other one. New perceptions are introduced into the system as practice continues and are either harmonious or anomalous. These perceptions occasion either assimilation of the system concept (world view) in case they are harmonious or accommodation of the world view if they are anomalous. (They may also be irrelevant and have no impact). Though Laudan doesn't invoke any explicitly control-theory language, it seems that his model could be described as an n-order control system for the derivation of scientific norms. It also is an explanation for the rationality of scientific change and the rational role of norms and values fueling such change.

My question here is simply: can we let systems concepts have plastic reference values as a function of all the other-order reference changes at any point on the analog continuum of experience? This sort of ties the reference hierarchy into a closed loop with all the other reference levels at the same time.

jeff dooley dooley@well.sf.ca.us

Date: Fri May 22, 1992 2:07 pm PST
From: Dag Forssell / MCI ID: 474-2580
TO: Gary (Ems)
Subject: Kontrollteori

Halsningar fran Californien!

Gary Cziko skickade en kopia av Ditt meddelande i gar, med halsningar
Gary Cziko sent a copy of your message yesterday, with greetings

fran "Sweden and Lars." Jag blir litet nyfiken pa vad som menas med
from "Sweden and Lars." I get a little curious about what is meant by

Control Theorist fran Ditt perspektiv. Bill Powers ar en magnifik man som
Control Theorist from your perspective. Bill Powers is a magnificent man who

agnat 35 ar at att beskriva manniskor som "styrsystem." Jag ar
has devoted 35 years to describing people as "steering systems." I am

Chalmerist, men har bott har i 25 ar och arbetar inom industri. Nu borjar
See below, but have lived here for 25 years and work in industry. Now, I begin

jag marknadsfora ett ledarskaps program baserat pa Powers' modell.
to market o leadership program based on Powers' model.

Vart elektroniska medium, CSG-l, ar mycket aktivt med en blandning av
Our electronic medium, CSG-l, is very active with a mixture of

social tillampning och detaljerad teknik, foretradesvis simulering. Om
social application and detailed technology, preferably simulation. If

Du har fragor, kommer Du att finna Bill Powers en outtrottlig,
you have questions, you will find Bill Powers an indefatigable,

mangfacetterad kampe.
multifaceted fighter.

-Fighter is too strong; hard worker, one who struggles: CHAMPion.

In one of the many addresses in the header, was the sequence "kth."
This I take to stand for "Kungliga Tekniska Hogskolan". I graduated
from the other University of technology in Sweden: "Chalmers Tekniska
Hogskola", thus: "Chalmerist", and peer.

Thought you would get a kick out of it. You did good! No reply yet.

Live well. Dag

Date: Fri May 22, 1992 2:17 pm PST
Subject: Willfulness

From Greg Williams (920522-2)

>From Bill Powers (920522.0900)

You're not willfully misreading me, are you?!?!

>If a miracle occurs, it becomes part of the history of the affected system.

Do your PCT models contain non-history-determined events (what I've been calling "miracles"? I've always thought they didn't.

>To call something a "miracle" is only to say "I don't understand how that could have happened."

I have defined what I am calling a "miracle": non-history-determined events. I claim that PCT models don't include such "miracles." PCT models include plenty of "miracles" in your sense, as does the natural universe at large. So what? How does your concept of "miracles" bear on my claim? I don't even need to call non-history-determined events "miraculous" to make my point, which depends on the nature of PCT models. Are they history-determined or not? If they are history-determined, then the models offer no support for those who claim that (at least some of) an individual's choices can be deliberate and also completely autonomous. I admit that autonomous choices might be made in a random way (perhaps employing quantum mechanical effects) by processes internal to an organism modeled using PCT -- but such choices are random, and don't correspond to traditional notions of free will. Otherwise, PCT models (as far as I can tell) produce time histories of behavior/actions which depend on their histories, BUT NOT NECESSARILY IN A STRICTLY NON-RANDOM WAY (for instance, randomness could come from the dynamics of the niche). In any case, what happens NOW in a PCT model depends on its interactions with its niche, and so what happens NOW depends NOT ONLY on the organism modeled, but on its environment.

>The concept of a "state-determined system" comes, I believe, from LaGrange.
>Give me a description of the universe at a given instant, and knowledge of
>all natural laws, and I will tell you the state of the universe from then
>on. This is what physical determinism is about, isn't it?

Since Lagrange, the idea has been broadened to include systems with randomness in their state transition (dynamical) laws. I am using the notion in this broader-than-Lagrangian-predictive-determinism sense. One more time: I AM NOT ARGUING FOR PREDICTIVE DETERMINISM OF PCT MODELS. My problem is with claims that PCT models give any support to absolute autonomy of organisms.

>Control theory introduces a type of physical organization that can
>eliminate all but a narrow range of the outcomes of chaotic and randomly
>perturbed processes. What determines the outcome is then not "history", but
>the present organization of the controlling living system.

Are you missing my point on purpose? (Tee hee!) If PCT models are (non-random

OR random OR stochastic) dynamical systems -- and, to date, I haven't met one that isn't -- then the organizations, at each point in time, of those models depend on their history (for the last time, POSSIBLY WITH RANDOMNESS IN THAT DEPENDENCY), and that history includes interactions between the modeled nervous system and its niche.

>Once the fact of control became established in one part of the physical
>universe, that part became able to preserve the kind of organization that
>controls, and passed along from generation to generation this ability -- the
>ability to predetermine outcomes independently of the laws governing the rest
>of the universe, the ability to manipulate physical variables so that their
>lawful effects are those intended by the controlling system.

As I have observed them, PCT models do not "predetermine outcomes independently of the laws governing the rest of the universe." To the contrary, those laws (first and foremost, CAUSALITY, which claims that the future does not influence the past) are presumed in PCT models. Aren't they? Did I catch a whiff of that dread opposite of the law of causality, namely TELEOLOGY? No -- I never have smelled even a hint of teleology in PCT, just TELEONOMY. PCT models show how intention can be physically realized via closed-loop organizations.

PCT models determine behavioral outcomes in accordance with their histories. What they DON'T do is autonomously determine the actions necessary to achieve those behavioral outcomes. Neither do they autonomously determine their own current control structure, in general. Anyone looking to pin all the "blame" on
somebody for what that somebody does had better look elsewhere for a supportive model. And so should anyone looking to pin all the "blame" on somebody's niche for what that somebody does. PCT models SHARE the blame between somebody and his/her niche.

>To LaGrange, as I understand his ideas, physical determinism was absolute and
>fixed forever. This idea can no longer be supported.

Predictive determinism is dead. But its demise wasn't needed to make the point that it seems (to me) several netters want to make: Success in making deliberate manipulations of living control systems is more difficult than Skinnerians think.

>Control theory provides the final justification for "free will." Free will
>is not a phenomenon of causation, but a phenomenon of organization. It
>isn't a very good term for this phenomenon, but it's the best people could
>do by way of describing their relationship to the nonliving world.

Why not "intention" instead? Go ask a theologian whether PCT models justify "free will." I bet the theologian will say that's not what "free will" is about. PCT models show how intention is a natural phenomenon of (certain kinds of) organizations. I still think PCT models include no organization giving rise to traditional "free will."

>Prediction becomes a matter of understanding intentions. In my case, an
>intention formed nearly 40 years ago is still determining outcomes, despite
>four decades of random perturbations and tendencies of natural processes to
>branch and branch again as they pass natural bifurcation (or multifurcation)
>points.

(An aside.) As I've noted a few times before, this begins to sound ominous to some who would prefer to not be manipulated by others. The Test in the hands of would-be manipulators might aid them greatly. (Maybe we should all start refusing to participate in polls? As Steve Earle has noted, "Just because you ain't paranoid don't mean they ain't out to get ya.")

>The term "free will" does not designate something that exists in an
>objective universe (or not) and which we then must prove (or disprove) to
>be something real. It's an attempt to describe an experience. Control
>theory, by showing how purpose works, suggests a clearer view of this kind
>of experience.

Yes. PCT models suggest that the experience of (traditional) free will is an illusion.

Greg

P.S. With good weather here (eat your heart out, Bill), between work on the house (we topped out today and had a tree ceremony!) and answering your posts, when will I have time to look at the Little Man. The weather folks say rain on Sunday, but they're usually wrong. Seems they are limited in their ability to specify behavioral outcomes.

Date: Fri May 22, 1992 2:19 pm PST
Subject: PCT-autonomy: HOORAY!

From Greg Williams (920522 - 3)

>Bill Powers (920522.1100) on Autonomy

You could have saved me a lot of keystrokes...

No sooner do I send a complaint accusing you of misreading me on free will and determinism than I receive a beautifully composed essay on PCT and autonomy, with which I am in full agreement. Teleonomy all the way down! No (traditional)
free will. Thanks!! Maybe I'll get to the Little Man soon, after all!

>So we can see where autonomy begins and ends. It is the organism that
>selects consequences that keep its intrinsic errors as close to zero as
>possible. It is the environment -- and other organisms in it -- that
>determines what actions must be produced in order that those consequences
>be brought about and maintained. The external world sets the stage on which
>existence is played out. But the reorganizing system writes the play.

>And even the reorganizing system is just the product of a deeper control
>process, at the core of which lies a tiny and unimaginably ancient spark
>of purpose that makes life different from everything else.

A vision of the life process as beautiful as any I've found in traditional religions!

Greg

Date: Fri May 22, 1992 2:20 pm PST

Subject: Standards; Plasticity of top-level Reference

[From Rick Marken (920522 14:30)]

Jeff Dooley (920522.1000) says:

>(Bill Powers 920514b)

>>The only reference signals (and perceptions) that can't be
>>changed freely as required by higher levels are systems concepts.

> If a world-view can be considered a
>systems concept (in the sense that it can organize
>principles), then how can we explain the possibility that a
>"world-view" can change (Ptolemy vs. copernicus, etc.).

Two possibilities: 1) ptolomy vs copernicus is not a system concept level difference. They could just be two different models (programs?) that are possible components of a "science" system concept. If one of the principles of this concept is "best fit to data" and "simplest" then you adopt copernicus. If it's "best bit to data" using existing assumptions" (epicycles) then its ptolemy. 2) ptolemy vs copernicus are system concepts. Whichever you believe (have a reference for) is then a result of previous reorganizations (random selection of system level references triggered by intrinsic error). So people would only adopt one if doing so happens to produce the least intrinsic error. Intrinsic error might result from observed discrepencies between model predictions and observations (if the person cared about such things) or something else (desire to differ from the church?).

>I think that some of the work of Larry Laudan may help clarify
>these questions as they may pertain to system concept reference adjustment.

>My question here is simply: can we let systems concepts have
>plastic reference values as a function of all the other-order
>reference changes at any point on the analog continuum of
>experience? This sort of ties the reference hierarchy into a
>closed loop with all the other reference levels at the same time.

If this is what Laudan is suggesting then I think it might help if he hooked up these ideas in a working model. My guess is that he could quickly eliminate this hypothesis; if lower level outputs influenced the reference signals of systems that determine the reference signals of the system that determines the output then it seems like there could be problems; like positive feedback type effects. But maybe it could work? It just doesn't seem to make much sense in terms of the architecture of the PCT model as it currently stands. Perhaps if you gave a quantitative analysis or functional diagram of Lauden's suggestion it would clarify his point.

By the way, the references for the highest order systems (in the PCT model) can be varied -- but not as a means of controlling some higher order variable. So changes in the references for the top level variabls cannot be SYSTEMATIC (as are the changes in reference for the tension in my muscles which varies to maintain the position of my outstretched arm even while someone is pressing down on my

hand with variable force). The top level references are varied UNSYSTEMATICALLY by the reorganization system -- and they stop changing once intrinsic error disappears. This aspect of the model seems to me to give a good account of why people adopt certain system concepts rather than others -- its BY CHANCE. People become christians because by adopting that reference they have been able to reduce intrinsic errors that result from fighting with their family. People become dodger fans because, by adopting this reference they reduce the intrinsic error that comes from the stares generated by wearing an oakland A's jersey at Dodger stadium. People believe in Copernicus because everyone else does (it looks to me like the sun goes around the earth -- but I'll never tell). I like the fact that system concepts (in control theory) are not selected systematically; and this goes for the "control theory system concept too (to the extent that it is a system concept). There is, really, no good reason to adopt it except that it feels good. Of course, part of that good feeling comes from accounting for data to within .001%. So it seems like the only scientifically RIGHT thing to do. So why aren't social scientists moving to PCT in droves? Because it doesn't feel good to them; they are happy with their own system concepts and they don't even know that there is a system concept to change (if system concepts really are the TOP of the hierarchy and control theory and ptolomy and copernicus are system concepts then it seems to me that we would not even be able to see that there is a CHOICE. I think a case can be made for the fact that "real" system concepts are invisible to consciousness -- you can take the point of view of that system concept, put you shouldn't be able to see it AS an optional point of view.).

Regards Rick

Date: Fri May 22, 1992 6:06 pm PST
Subject: control and consciousness

[From Rick Marken (920522 19:00)]

Well, it looks like the machine is still up so let me make one quick observation. I think there has been some confusion about what control theory says about control and what it says about the processes that might influence control -- ie. consciousness. This confusion becomes particularly acute in the discussion of standards (principles) where there is talk about "setting appropriate standards" and such. When we talk like this, WHO do we imagine to be "setting the appropriate standards"? The hierarchical control model says it is the "higher order systems". The references for principles are AUTOMATICALLY set by the systems controlling system concepts. The references for programs are automatically set by the systems controlling system concepts and/or principles, etc. The point is that all this varying of lower level references to control higher level perceptions is carried out smoothly and automatically by the control hierarchy. I hate to point this out again, but this process is nicely illustrated by my hierarchical spreadsheet and (in a less abstract manner) by Bill's "Little Man". So ordinarily, there is no external "agent" (other than the control hierarchy itself) that sets references -- and varies them -- and this varying happens automatically. This means that principles are varied AUTOMATICALLY to control system concepts; if the system concept control systems need to vary the honesty principle reference to preserve the perception of the system concept, it DOES it; that's YOU doing it -- but there is no choice going on; no conscious decision to be a little more or lest

honest in this particular situation. It just happens (just as you muscles tense automatically to control the position of your limb).

When we talk about a person "setting appropriate standards" I think we are talking about a phenomenon that is external to the PCT hierarchy. Subjectively, when I talk to myself and say "maybe I should try X" it is my consciousness that is doing this. Consciousness is like something that hovers over the hierarchy and tinkers with it occasionally; at least, consciousness is what CAN tinker with the hierarchy.

I think we know a hell of a lot LESS about how consciousness works (in terms of phenomena and models) then we know about how hierarchical control works. But I do think that consciousness (the feeling of having to choose -- "should I do X or Y"?) only comes up when there is some degree of internal conflict or lack of "output functions" that can be used to control the required perceptions. When you are in control you are rarely conscious of it -- unless you make some effort to notice how well your hierarchy is working. When there is a failure of control (due to conflict, lack of skill or insuperable disturbance) then consciouness is there. As I said in an earlier post -- moving consciousness to systems that don't need attending to can create more problems than it might solve.

Best regards Rick

Date: Fri May 22, 1992 9:32 pm PST
Subject: Interesting Book

Has anyone heard of, read, or have opinions about this book:

Chapman, D (1991) Vision, Instruction and Action, MIT Press

Penni Sibun put me onto it, & it seems quite interesting on the basis of a quick scan.

Avery.Andrews@anu.edu.au (currently andrews@csli.stanford.edu)

Date: Sat May 23, 1992 8:11 am PST
Subject: Plasticity; history determined; miracles

[From Bill Powers (920523.0800)]

Jeff Dooley (920523) --

>As you point out, output of higher-order control systems
>enables the plasticity of the lower-order system reference
>values.

"Plasticity" doesn't suggest the right image to me -- maybe "fluidity" would work better. The output of a higher system doesn't just "enable" lower reference signals to change (that implies that once such change is enabled, something else does the actual changing). The lower reference signals, in the HPCT model, are DETERMINED by the higher level outputs, in real time. They are simply the sum of the outputs of all the higher systems contributing to them. Keep in mind that changing a reference signal does

not change its kind or meaning: it changes its amount along the scale from zero to maximum.

>If a world-view can be considered a systems concept (in the sense that >it can organize principles), then how can we explain the possibility >that a "world-view" can change (Ptolemy vs. copernicus, etc.).

Rick Marken has given one good answer: through reorganization, meaning trial and error. There may be a constraint in that we can't set reference signals for perceptions we don't know how to perceive. So perhaps the first thing that has to happen before we can control for a new system concept is that we must learn to perceive it -- grasp some constellation of principles and their subordinate programs AS AN ENTITY. This, too, requires reorganization. Once we can perceive such an entity, such as physics, we can perceive it in various states: rigorous physics, intuitive physics. Then we can select a reference state for it according, as Rick says, to the state that strikes us as the most pleasing. "Feeling good" about the system concept may sound somewhat too trivial -- perhaps the idea of "aesthetic judgment" would fit the surroundings better.

>Difficulties like these have surfaced in philosophy of science recently >surrounding attempts to provide a normative epistemology within the >scope of a purely scientific (naturalistic) study of how we come to >know anything. This problem boils down to how we can derive norms or >prescriptive standards from an apparently descriptive account of >epistemology.

The basic difficulty is in the idea of a "normative" epistemology, which implies that there's one boss system concept to which "good science" OUGHT to conform. Even to speak of a normative epistemology is to beg the question as to whether the epistemology an individual can know is the real, objective epistemology or simply a private concept of epistemology with no provable relationship to reality. You can't have (basically social) norms for science without having already subscribed to some form of realism, or at least having proposed a model.

Laudan's idea of " ... the actual realizability of aims through methods" does, as you say, sound like an approximation to control theory. It also (later in your description) sounds like Piaget, which is OK. But I find philosophers of science difficult to take, most of the time. Their main mode of argument seems to be the flat statement of fact, which they seem to hope will evoke some sort of resonance in the reader. I would rather see philosophies of science grow out of the practice of science, and the propositions to be cast in terms of evidence and reasoned justification rather than resting on some unspoken sense of rightness. The footing feels awfully mushy to me. But then I get sort of mushy sometimes, too.

>My question here is simply: can we let systems concepts have plastic >reference values as a function of all the other-order reference changes >at any point on the analog continuum of experience?

I agree with Rick Marken's comment on this: you're proposing an architecture very different from that of control theory. What would the connections look like in a model where one reference value "is a function of" all other reference value changes? What function, in particular, are you talking about? What would happen, then, to the idea that reference signals are set by the outputs of higher systems? It seems to me that you

have some problems here if you want to relate this idea to control theory.

It does seem that the subjective meaning of a single perception somehow depends on other perceptions, but this doesn't mean that the perceptions themselves (as individual signals) are interrelated in this way. It means that whatever apprehends a given collection of signals can make sense of them only as a complete set and not in isolation. This could be nothing more than a higher level input function.

Greg Williams (920522-2) --

I'm not deliberately misreading you, but perhaps I'm misreading you. To me, "history-determined" doesn't seem to mean what it means to you. I don't think that "history," which is simply a record of what we remember of the past, has any influence on anything (although our memories of it, which exist in present time, may have some influence on what we do next). Even the FACT that something happened in the past has no physical effects NOW. All physical effects, I assume, occur in present time, as one variable influences another coexisting variable. The past can't affect the present any more than the future can. This doesn't keep us from tracing a course of events through time, but all that happens in imagination.

I've heard this concept of influences of history before, but from the same people I heard that history makes no difference. A calculation of voltage from resistance times current, for example, doesn't depend on how a particular resistor happened to be manufactured, or on what is supplying the current, or on what the current was prior to the measurement. The main thing that the LaGrangian concept says to me is that history DOESN'T matter. It doesn't matter how the universe got into its present condition -- by what paths the current variables came to be in their present states. Only the states of the variables NOW, and the laws connecting them, matter in determining their next states. And when the next states occur, there is a new NOW. All real phenomena and interactions occur NOW. That is what differential equations are all about.

With stochastic and chaotic phenomena as parts of the real universe, we really can't tell how the world got into its present state, not in any great detail. A succession of bifurcations and stochastic variations leaves the causes of present states of variables unknowable (in general) except through memory; anyway, the causes are immaterial to what will happen next. There are not only alternative futures that could grow out of the present, but alternative pasts that could have led to the present. To say that a process HAS a history is not to say that history "determined" the process.

>I have defined what I am calling a "miracle": non-history-determined
>events.

Again, we're at odds over the usage of a term. Perhaps yours is better. If one believes in miracles, then a scientific explanation of what happened is rejected in principle, without even needing to understand the explanation: the essence of a miracle, as some people view it, is that it is unconnected to any natural process. If you can make a connection, it wasn't a miracle. Of course the validity of miracles is always evaluated in terms of PRESENT understanding of natural phenomena.

I don't believe that anything that happens is "unnatural" or "supernatural." By my definitions, if it happens, it's part of the natural

universe. To say that something occurs miraculously is, as I remarked, only to say that you don't understand how it could have occurred. So the less you understand of nature, the more miracles there are.

One possible explanation of any event is that a supernatural being caused it. As long as you have no better explanation, that's as good a one as any. Except that I would claim that when we finally understand how the event was brought about, we will understand the supernatural being, and then it will simply be a natural being, like any other, although perhaps smarter and more capable than usual. We define "natural" and "supernatural" in relation to ourselves. Remember Arthur C. Clark's dictum: a product of any sufficiently advanced civilization will appear to work by magic. Maybe there's an advanced civilization messing around with us. Anything's possible, until you eliminate some of the possibilities. Why limit the playing field?

For me, the question isn't whether miracles could occur, by anyone's definition, but what is the best explanation of what we observe? As long as you can't put on a convincing demonstration of your explanation, you haven't ruled out anything, even magic. There's no need to settle once and for all on any one mode of explanation, although with experience you will probably try certain modes of explanation before others. Scientific purism is a strait-jacket. If I'd been a scientific purist I never would have doubted lineal causation.

It's perfectly possible that there are entities which exist in space and time, but are purely causal in nature: that is, they are generators of phenomena, but are not generated by phenomena external to themselves. This would not make them all-powerful or omniscient, for their scopes of action might be quite limited. As far as anyone knows, such an entity produced the Big Bang. It's scope of action was to produce, literally out of nothing, all the particles and antiparticles of which the universe was made. This general sort of entity would seem to have the basic ability we associate with creativity or consciousness; such an entity would have a scope of action limited to nervous systems, or at least living control systems.

On the other hand, such entities may not exist at all. Basically, who cares? We're a long way from needing that answer now. Looking for short-cuts to ultimate answers has never done science much good.

As to stochastic determinism, all I can say is that this sounds like an oxymoron to me. To invoke "chance" as a causative agent seems no better to me than invoking miracles. If there's randomness in a dependency, what kind of dependency is left? Sometimes there's a dependency and sometimes there isn't. Does that average out to lawful dependency? It seems to me that you're talking about the fundamental flaw behind statistical analysis, not about science. I claim that a statistical regularity is only a real regularity seen dimly and through the eyes of the wrong model. Clearly, nobody will offer me a job in quantum mechanics.

>Did I catch a whiff of that dread opposite of the law of causality,
>namely TELEOLOGY? No -- I never have smelled even a hint of teleology
>in PCT, just TELEONOMY.

This distinction is a straw man that grew out of thinking that purpose could only exist if the future affected the present, while happily accepting that the past can affect the present. I don't think that

"causality" is any better than teleology: it's magic, too. All interactions occur in present time. Causality doesn't come into it.

Finally: I tend to treat ideas like God and miracles and the like as evidence of experiences that people have been trying to explain. It's a mistake, I think, simply to dismiss the theories because one doesn't agree with them. Behind the theories is something that people have run across that puzzles them. The real challenge is to try to discover what that is, and to see if a better explanation can be found without discarding the phenomenon.

I'm glad you went through all those extra keystrokes before my missive on autonomy. I don't know anyone else who not only publishes my work so beautifully and with such unstinting labor, but who forces me to think deeply about things I would otherwise dismiss with a passing word. Don't quit.

Best to all, Bill P.

Date: Sat May 23, 1992 11:34 am PST
Subject: Re: Autonomy

[from Gary Cziko 920523.1418]

Bill Powers (920522.1100) expressed:

>So we arrive finally at the question of autonomy in the individual human >being. Autonomy is clearly not freedom from physical constraints (which >include, in the final analysis, social constraints). The environment, not >the organism, dictates the effects of any given action. But the environment >does not dictate the desired consequences of any action. It is the organism >that chooses those consequences, and learns how it must act in order to >produce them.

This gives me a whole new appreciation of what the H (hierarchy) in HCPT is about. I realize now that the environment (and the other organisms in it) DO determine certain perceptions, but only the LOWER ones in the hierarchy. If the living control system is successful, the environment will not determine what the PERCEPTIONS THAT COUNT will be (those higher in the hierarchy).

If you are my disturbance pulling on your end of the rubber band, your action WILL determine some of my perceptions, namely my perception of where my hand is. But it will not determine where the knot of the rubber band is (unless of course you disturb beyond my capacity to control). Of course, if I want to control BOTH perceptions (where my hand is and where the knot is), then I've got conflict and will have to reorganize.

Keep at it, Bill. It's so fulfilling to see my understanding of HPCT continue to grow. I hope that others on CSGnet are having the same experience.--Gary

Date: Sat May 23, 1992 7:30 pm PST
Subject: Autonomy and control

[From Bill Powers (920323.2000)]

Gary Cziko (920323.1410) --

>If you are my disturbance pulling on your end of the rubber band, your
>action WILL determine some of my perceptions, namely my perception of
>where my hand is. But it will not determine where the knot of the
>rubber band is (unless of course you disturb beyond my capacity to control).

Right. The level where disturbances can affect the controlled variable is the level where your behavior (actions) depends on what the environment is doing. At that level, your behavior (and your perceptions of it) are not autonomous. But by giving up autonomous control over hand position, you gain autonomous control over a consequence of hand position -- the position of the knot.

The environment, however, can be more complicated than that: it may be that control of a consequence of position of the knot might also be disturbed in some way, so that you have to give up control of the knot's position in order to control something else. For example, you might be trying to make the knot stay over a randomly moving target. Now you can't control either your hand position or the position of the knot. You have to let the target determine where the knot is to be, in order to maintain the relationship between the knot and the target. You have to let your hand move according to the disturbance put in by the person at the other end of the rubber bands. Now your autonomy applies only to the control of a relationship, and neither to control of knot position or hand position.

Here's an even more complicated example:

In the 70s, when I was thinking up games (like Trippples), I devised a rubber-band game that was played on a wooden board. The board was divided into three parts by two lines parallel with the ends of the board, which was about 24 inches long and 12 inches wide. In the middle part, which was only about 4 inches deep, were four holes. Two of the holes were labeled "A" and the other two "B", corresponding to players A and B, alternating A,B,A,B across the width of the board.

The rubber bands were connected to a puck with a hole in it in which a marble was placed. The puck could slide over the holes in the board, but if it was centered, the marble could drop in. So the first aspect of the game was for the players to move their ends of the rubber bands to try to get the marble to drop in their own hole for a score (then they put the marble back in the puck and started again from a neutral position).

The other aspect of the game came about from the fact that at the free ends of the rubber bands were two more pucks; the players moved their ends of the rubber bands by sliding those pucks around. Those end pucks also had holes in them into which marbles were put. In each player's part of the board, there were more holes. If the marble in the puck you were holding fell into one of the holes on your side, the OTHER player got the score for that hole.

So each player had to control for three things: the position of his own

end-puck (don't let the marble drop in a hole on your side); the position of the other player's end-puck (try to influence the other player to drop the marble at that end into a hole that scores for you); and the position of the center puck (try to drop the center marble into a hole that scores for you and keep it out of a hole that scores for the other person).

This was, of course, a game of conflict between autonomous systems. But it was also a game of inner conflict, because as you maneuvered your puck around trying to get the other person to give you a point, you could either try to get the center puck over one of your holes, or maneuver the other person into moving the other end puck over a hole at the other end of the board -- and all the time not move your own puck directly over a hole on your side. You had to decide what to give up control of and what to control; you also had to guess whether the other person was concentrating on maneuvering you to drop your marble in a hole at your end, or trying to get the center marble in a scoring hole.

If you paid too much attention to avoiding holes on your side, the other person could pretend to maneuver you toward a hole so that when you counteracted that move, the center puck would be over the other person's scoring hole. If you paid attention to getting the center puck over one of your holes or keeping it away from the other's scoring hole, the other person could put in a disturbance that would make you drop the marble at your end into a hole. And of course you were trying to do the same thing to the other person.

This turned out to be a very funny game; also very difficult and sometimes so fast it was right at the limit of what was possible. Your attention was darting all over the board, and you could feel things clashing inside as you tried to avoid dangers and also make a score. I never tried to market it. If anybody wants to try, pay me a chunk of whatever you get for it -- I'm saving up for a new computer.

As you control for variables of higher and higher level, the aspects of the environment that can introduce disturbances become less "concrete" and more "abstract." Think of playing the piano in front of an audience. The piano isn't going to disturb you, and you're used to controlling against the key resistances and so on. But if someone in the front row pulls out a newspaper and starts reading it, this may disturb some very high-level variables and make it difficult to put the appropriate expression into your playing. Or suppose you're in the middle of a passionate exposition of control theory, and you notice that the other person is stifling a yawn. If you try to control for rapport or understanding, you're going to have to change your style of delivery in some way. And it's the properties of the other person that determine what you must do to maintain control of what you're trying to do at the higher level.

Happy Memorial Day (somehow that doesn't sound right)

Bill P.

Date: Sun May 24, 1992 6:25 am PST
Subject: Re: Bill & Gary on Autonomy

From Greg Williams (920524)

>Bill Powers (920523.0800)

>It doesn't matter how the universe got into its present condition
>-- by what paths the current variables came to be in their present states.
>Only the states of the variables NOW, and the laws connecting them, matter
>in determining their next states. And when the next states occur, there is
>a new NOW. All real phenomena and interactions occur NOW. That is what
>differential equations are all about.

It matters to the folks who have pitched the idea of what I've been referring to as "traditional" free will (which is where this thread started). They claim that an individual has a "transcendental self" (their term) which can make "free deliberate choices," where "free" means completely independent of anything besides the "self" and "deliberate" means "not-random." So models of how behavior/action happens which don't include such things -- such as what I've termed "unaugmented" PCT models -- provide no basis for a belief in such things. For the "traditional" free will believers, it is important that at least some of an individual's current behavior NOT be subject to influence of EITHER past OR present events outside the "self," otherwise the "self" would not able to be absolutely autonomous. In PCT models, rules (in some cases, perhaps with "random" components) are postulated connecting the present state with the previous state (differential equations, for time-continuous models), and there are also rules connecting the present form of the governing differential equations with previous forms (with reorganization, the differential equations are non-time-stationary). Further, all of those rules, in general, are non-autonomous; that is, they have forcing functions (think of these as boundary conditions which change with time) set by the niche, not by the "self" (even if it is claimed that a whole PCT model is a model of a "self"). The trajectory of states describing the evolution of behavior/action through time will be different, in general, if the forcing functions are altered.

PCT models imply that an individual's current control structure is the result of past interactions between the (changing-over-time) control structure and its niche, and NOT the result SOLELY of an autonomous dynamics of the control structure (which would mean that when reorganization stopped, in any particular case, would be independent of the niche, denying the very reason for reorganization). And PCT models imply that the current behavior/action is the "automatic" result of the current control structure and the current forcing functions. Again, I agree with you: PCT models say that at any moment, the environment (via forcing functions) influences action, but cannot influence behavior (that is, which goals are set); PCT models say that at any moment, the existing control structure sets the goals and thereby influences action; and PCT models say that the existing control structure is the result of the past interactions between the evolving control structure and the niche.

>For me, the question isn't whether miracles could occur, by anyone's
>definition, but what is the best explanation of what we observe? As long as
>you can't put on a convincing demonstration of your explanation, you
>haven't ruled out anything, even magic. There's no need to settle once and
>for all on any one mode of explanation, although with experience you will
>probably try certain modes of explanation before others.

Right on. My only complaint is with people who claim that PCT supports

"traditional" free will -- absolute autonomy. They can believe what they want, but I, for one, won't let them get away with claiming to support that belief with a model which doesn't support it.

>It's perfectly possible that there are entities which exist in space and
>time, but are purely causal in nature: that is, they are generators of
>phenomena, but are not generated by phenomena external to themselves.

Sure it's possible. But no such entities are to be found in PCT models. (Although PCT models COULD be augmented, as I have said before, to include such entities.)

>On the other hand, such entities may not exist at all. Basically, who
>cares? We're a long way from needing that answer now. Looking for short-
>cuts to ultimate answers has never done science much good.

Again, right on. In all this, I've been attempting to head-off such shortcuts. It is tempting, but premature, to claim support for religion in scientific ideas.

>Gary Cziko

>I realize now that the environment (and the other organisms in it)
>DO determine certain perceptions, but only the LOWER ones in the hierarchy.
>If the living control system is successful, the environment will not
>determine what the PERCEPTIONS THAT COUNT will be (those higher in the
>hierarchy).

I don't understand this claim. As I understand the HPCT model, at any given moment the "relevant" or "interactive" environment (what I have been calling the "niche") cannot SOLELY determine ANY perceptions in the hierarchy -- rather, the perceptions at ALL levels are determined (possibly with a random component -- sorry Bill; maybe "are set" sounds better?) by the niche AND the current structure of the hierarchy. How could it be any different at the top of the hierarchy? Surely you aren't saying that the top level is a solipsist and compares its reference signals (about system concepts?) with COMPLETELY MADE-UP perceptual signals. Bill mentioned physiological homeostasis variables ("intrinsic reference signals"); are you alluding to these as somehow "not determined" by the environment? If so, you should be claiming that the REFERENCE SIGNALS, not the PERCEPTIONS, are "not determined." BTW, intrinsic reference signals ARE determined (sorry again, Bill; set!) evolutionarily by organism-environment interactions (not, of course by the environment ALONE). And intrinsic perceptions (i.e., of core body temperature?) are set from moment to moment by the current hierarchy and the niche, acting conjointly.

Greg

Date: Sun May 24, 1992 9:08 am PST
Subject: Free will

[From Bill Powers (920524.0900)]

Greg Williams (920523) --

>It [how the universe got to the way it is] matters to the folks who

>have pitched the idea of what I've been referring to as "traditional"
>free will (which is where this thread started). They claim that an
>individual has a "transcendental self" (their term) which can make
>"free deliberate choices," where "free" means completely independent of
>anything besides the "self" and "deliberate" means "not-random."

When "these folks" (?) claim that free deliberate choices are completely independent of anything besides the self, do they include independence from the choices that are available to be made? That is, if you're presented with the choice of eating a dish of Jello and eating a dish of spaghetti, are you free to choose to eat a cheese sandwich, even if there isn't one? The very concept of "a choice" (however it's made) seems to imply some extant alternatives among which to choose -- in other words, a world apart from the chooser over which the chooser does not have total control, which the chooser did not choose to exist. If you have total control over the universe, no choices need be made.

Second difficulty: how would anyone show that a "deliberate" act was "non-random?" As you present the idea, this would be impossible to demonstrate. If an act is "non-random," then it seems that it must be related in some systematic way to surrounding conditions. In other words, the proof of non-randomness is to describe the algorithm or rule or influence that generated the choice. If "deliberate" means "non-random," it means acting for a reason: to achieve a goal, or in obedience to a describable rule. But then the goal or the rule determined the choice, and it was not free. The same procedure that shows that a choice was non-random shows that it was not free (I will show later that there's a flaw in this argument).

If you're fairly describing the best thought of those who support the concept of free will, it's hard to understand how "they" could have ignored such elementary difficulties. If, on the other hand, you're picking the lowest common denominator to represent that point of view, it isn't hard at all, nor are you presenting the view fairly. Why should we bother with analyzing the least competent way of presenting a viewpoint? The only interesting challenge would be the one presented by the most thorough and competent thinkers who speak of free will.

We have to avoid falling into the same tactics that were used in denying purposiveness. One of the arguments against purposiveness was that the future can't affect the present. This was a very insulting argument, because it assumed that those who accepted purposiveness were so stupid that they had overlooked this elementary problem. Instead of using that counterargument, the opponents of purposiveness should have thought "I'm talking with a person who is as intelligent as I am, so whatever that person means by purposiveness, he or she wouldn't mean to say that the future affects the present. What, then, might be a meaning that wouldn't entail this silly assertion?"

Even if a particular proponent doesn't see problems with the idea of purpose, the challenge is still to discover how that idea MIGHT be defended by someone who IS competent. Until you can say that even when you give it your best effort you can't find a defense, you have no basis for rejecting the idea. You're just defending your own idea, the same as the other person.

So, what could "these folks" be referring to in speaking of free will that might have some validity to it? I don't mean the stupidest of them; I mean

the smartest of them, those over whom we do not have any intellectual edge.

There is one way to reconcile a "deliberate" act with a "non-random" act, and still leave the act free. Behind the concept of randomness is the assumption that there is no orderliness in a random event. But given a change in a variable that does not seem related to anything else, it is still possible that we simply don't recognize the order that is there. A "deliberate" choice for a change in that variable, a choice that is still not systematically related to any prior conditions, would have, as far as anyone can tell, the character of a random choice. Even if there is order behind the choice, if that orderliness is not of the kind that is a recognizeable relationship to prior events it would be indistinguishable from a random choice.

At this point one has to be cautious lest we make the same logical error made by the opponents of purposiveness. Those opponents used their premise to support the conclusion. The premise was that ALL OUTCOMES ARE CAUSED BY EXTERNAL EVENTS. The reasoning went something like this.

A series of actions leads through time to a future outcome. The only way those actions could come about would be as a result of some external event. To say that an action is caused by a purpose is to say that its intended outcome caused the action. Therefore the intended outcome is to the action as a cause is to its effect. But in this case, the cause occurs after the effect, which is impossible.

The error is to assume that purpose works as cause and effect work. The correct solution is to see that a purpose is not a prior cause, but a blueprint defining, in present time, a state of affairs that is to be brought about. The action is based not on the end-point, but on the present difference between the blueprint and the actual state of affairs. Action continues until the difference disappears.

If the opponents of purposiveness had thought this way, they would have invented control theory, thus showing the proponents that they were right even if the proponents at hand had no idea of how to defend their own belief. But of course if they had thought that way, the "opponents" of purposiveness wouldn't have been opponents in the first place; they would simply have taken the concept under advisement, given it their best effort, and discovered that it was in fact defensible. No adversary procedure would have been necessary.

On other other hand, if the opponents had given it their best effort and failed, they would then have had some basis for claiming that the concept of purpose was flawed -- or at least not tenable for the present. They would then have had every justification for opposing the proponents.

Now we have a similar situation. I have proposed that there can be a basis for choice that is not dependent on prior events, a basis that can't be expressed as a cause-effect chain or an algoritm or a rule. A choice made on that basis would be indistinguishable from a random choice. One possible, but erroneous, counterargument would be to say that if a choice has any basis, that basis must have been some external influence. That is the very question we are investigating: are there choices that are made without any external influence? So the counterargument uses the desired (negative) answer to prove itself.

There is another error to avoid, also made by the opponents of purposiveness. Another kind of counterargument was to describe instances in which the acts intended to create a certain outcome failed to do so, because of unpredictable large disturbances occurring after the act but before the outcome. In the same vein, opponents of purpose cited such things as the eyeblink reflex to prove that blinking the eye occurred whether one felt an intention to blink or not. These instances were taken as counterexamples that disproved the existence of purpose.

But purpose does not have to apply to every single act in order to exist. If a cosmic ray hits a motor nerve, the muscles may cause a limb to twitch and knock a glass of water to the floor. This is certainly not a purposive outcome. But under other circumstances it might have been. The existence of purpose is not disproven by demonstrating instances of nonpurposive outcomes.

In the same way, the existence of free will is not disproven by demonstrating instances of rule-based or automatic behavior presumably generated by automatic machinery. To say that free will exists and has effects is not to say that nothing else exists and has effects in the same organism.

I have proposed the possible locus of free will to be the process of reorganization. I would rule out reason, thought, emotion, principles, and system concepts as evidence of free will, because we can understand such things as being the product of learned computations in a physical brain obedient to physical properties of matter. I would also rule out as free will any truly random processes in the reorganizing system, because there can be automatically disorderly (or at least chaotic) processes at work as well, as in E. coli. But I can't rule out the possibility that there is an uncaused but orderly influence at work during the process of reorganization that can say, for no reason whatsoever, "Let's try it this way." Nor can I rule out the possibility that this kind of orderly influence accounts for ALL of the apparently random process of reorganization. It is impossible to distinguish an uncaused but orderly process from a truly random one, as long as we know of no ordering scheme that will account for the apparent randomness.

Quite likely, my conclusion about free will would not satisfy many of those who already believe in it. I don't particularly care about that. True Believers don't listen to reasoned arguments anyway, nor are they interested in risking any real investigation of belief that might turn out "wrong." If one already knows the answer, why go looking for it again?

A lot of arguments like these result from the way people use words without really examining what they mean. Consider the word "deliberate" in "deliberate choice." The derivation of this word implies deliberation -- that is, rational consideration of alternatives and the arrival at a logical conclusion. That would certainly NOT be a characteristic of free will, because then circumstances and logic predetermine the outcome. But what most people mean by "deliberate" is "on purpose," which entails nothing more than that sense of willed urging that something shall come to pass. As few people have understood purpose, it's not surprising that they are rather inarticulate when it comes to talking about purpose. But this doesn't mean that they're just tossing empty words around. They're trying to describe an experience without the vocabulary or system concept needed to give order to the description.

We'll get furthest by trying to understand and articulate those indescribable experiences rather than by rejecting the way people try to talk about them.

Best, Bill P.

Date: Sun May 24, 1992 11:16 am PST
Subject: Introducing Bruce Abbott

[From Wayne Hershberger 920524]

Rick Marken (920522 08:40)]
>We, as observers, CANNOT see what another person is perceiving
>or trying to perceive.

We can not see what the driver of an automobile is doing by watching the driver but we can see what he is doing, about as well as he can himself, by looking out through the windshield. As long as we have the same type of visual system it is possible to observe first hand what another is doing visually. This is not to say that one can directly experience what another is trying to perceive, but even that is not as inaccessible as behaviorists suggest, as you well know--you wrote a mind-reading program.

****
Regarding the interesting free will "controversy," I noted a point of agreement between Bill Powers and Greg Williams: A timeless (i.e., fixed, not free) autonomous reference signal. Hence, it was not surprising when Greg embraced Bill's "ancient spark" metaphor.

Bill Powers (920522.1100)]
>At the center of this system are reference signals that have not
>changed in billions of years, having survived even speciation
>.... a tiny and unimaginably ancient spark of purpose that makes
>life different from everything else.
****

Bill, Bruce Abbott, a prospective CSG member I met at this month's MPA convention in Chicago, will be contacting you about copies of your Armdemo2 and Demo1 Demo2 that I gave him when he attended my MPA presentation (Conation and Control: Where the Terms Response and Stimulus Are All Wrong). Bruce is using a 360 and having some difficulty with the graphics, but I'll let him explain the problem (it's minor). What is more important is that Bruce is a traditionally trained (his mentor's mentor was my mentor: C. C. Perkins, Jr.) experimental psychologist (Skinner-box/learning theory) with an non-traditional outlook. He read your 73 book years ago (also some Ashby, Simon, etc.) and the ideas have been percolating on his back burners ever since. In addition to being an open-minded "behaviorist," Bruce is a self-taught computer programmer and hardware manufacturer: he sells a

computer interface for skinner boxes he calls ParaPort. I have encouraged Bruce to subscribe to CSGnet, and I hope you will do likewise. I am sure he would be fascinated with your ongoing conversation with Chris Love, and with all sorts of posts, particularly such posts as Rick's recent essay: Three Blind Men. I other words, I believe that Bruce, while contributing a unique voice to our conversation will fit right in, and I hope we make him feel welcome--not that he won't be expected take his licks like the rest of us.

Warm regards, Wayne

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

Date: Sun May 24, 1992 11:45 am PST
Subject: Free will (more); Meeting info

[From Bill Powers (920524.1230)]

Greg Williams (920524) --

I omitted one point that flitted through the space between my ears and out again.

Your main point is that the PCT model doesn't contain any basis for assuming a self with a free will. I think I can dispute this point. You say,

>PCT models imply that an individual's current control structure is the
>result of past interactions between the (changing-over-time) control
>structure and its niche, and NOT the result SOLELY of an autonomous
>dynamics of the control structure (which would mean that when
>reorganization stopped, in any particular case, would be independent of
>the niche, denying the very reason for reorganization).

The implication here seems to be that the current control structure is some REGULAR function of past interactions with a niche. But reorganization occurs precisely when interactions with the niche lead to loss of control -- that is, when the current regularities in the interactions are insufficient to preserve control.

The reorganizing system is effective because the changes it institutes do NOT depend in any regular way on the current organization or the current niche. The whole point is to break out of the conflict or the circle or the failure -- the local minimum -- by trying something NEW. So the idea of tracing the current organization backward, while all right in a general sense, is wrong if it implies any predictable course of development. Reorganization breaks the cause-effect chain.

Reorganization -- that is, the actual output effect of the process -- is

independent (save for the frequency with which reorganizations occur) of any prior causes. The outcome of reorganization, to be sure, has to be such that intrinsic error is corrected; if it's not, reorganization simply continues. But there are uncountable ways of reorganizing that would result in correcting intrinsic error, so that result is not a constraint on any particular act of reorganization. In fact, one episode of reorganization is just as likely to make matters worse as it is to make them better (unless, of course, there is an unsuspected systematic component in it). The statistics of reorganization are very different from the statistics of stochastic -- but on the average systematic -- casuation. Reorganization will work even when the changes it produces show no trend at all in any direction.

Also, we mustn't forget that what makes reorganization effective are not the individual reorganizational events, but the selection effects that terminate reorganization. All that is required is the existence of something that can say "There! That feels better." Or, of course, something that says "Oh, no! Reorganize!" In fact, we could accept a mechanical randomness generator that actually does the reorganizing acts, and limit free will to the single act of triggering a reorganization. The "awareness" part of free will would then superimpose its judgments of what is acceptable and what is not on auytomatic judgments about such things as body temperature and state of nourishment. Thus free will could select for outcomes acceptable to it simply by causing reorganizations until the result is acceptable. The grounds for acceptability need have nothing to do with the niche.

By the way, for free will to exist, it isn't necessary for it to be the SOLE cause of anything. If it's simply A cause of SOME things, that's enough to throw the monkey-wrench into the causal gears.

Best, Bill P.
Date: Mon May 25, 1992 12:46 am PST
From: larsky MBX: larsky@jamvax.sunet.se
TO: * Dag Forssell / MCI ID: 474-2580
TO: 0004742580
EMS: INTERNET / MCI ID: 376-5414
MBX: "0004742580@mcimail.com"@kth.sunet.se

Subject: arbetslöshet o fattigdom

Hejsan Dag:

Kul att höra ifrån en svensk i Carlifonien. Förmodligen tjänar du väl en massa pengar där och lever det goda livet. Själv är jag arbetslös sedan min doktorsexamen i Systemvetenskap o Informatik och helt utan pengar men har gott om studieskulder.

Jag försöker alltså att bli docent , det enda som återstår för en doktor som ingen vill ha på arbetsmarknaden. För den sakens skull skriver jag på en bok om systemteori som förhoppningsvis skall bli en standardlärobok på de systemvetenskapliga linjerna. (där anställer man studenter som lärare, inte doktorer) Vilken av Powers böcker skall jag börja med att läsa? Jag skulle föredra den som är mest "allmänbildningsbetonad". Kan du rekommendera någon?

Hälsningar från det fattiga sverige o Lars Skyttner

Date: Mon May 25, 1992 3:34 am PST
Subject: Open letter to Bill P. on Arm 2

From Greg & Pat Williams (920525)

In a word, Bill, your Arm Demo 2.0 is IMPRESSIVE!

We put the program through its paces (or rather, pointings) yesterday (yes, it rained!) and found that the arm trajectories after correction of kinesthetic space are very much like empirical data -- much, much more so than in version 1 (visual endpoint control). For some start/end point combinations, the "in" vs. "out" trajectories are still a bit farther apart than the data show, but this is a minor difficulty apparently related to head and eye motion which can be fixed later, no doubt. Overall, the model is eminently reasonable from the points of view of development, perceptual adaptation, and studies of people with proprioceptive neuropathies (Bill, tomorrow we're mailing you a paper on the latter which you should find quite supportive -- G. Ghez, et al., "Roles of Proprioceptive Input in the Programming of Arm Trajectories," COLD SPRING HARBOR SYMPOSIUM ON QUANTITATIVE BIOLOGY 55, 1990, 837-847). It is truly remarkable for a model to meet the data in detail AND make sense in a general way. Congratulations!

That said, we are assuming that the documentation as it stands is for us beta testers only, and that it will be improved for reviewers and users. We think you should start off with a detailed explanation of the model (incorporating material like that in some of your CSGnet posts of last month). We're available as editors if you're game.

It would be nifty to be able to explain the idiosyncracies of particular trajectories (i.e., why that one curves convexly, not concavely, but has a little concavity at the far, not the near, end, etc.) via an examination of the arm geometry constraints/corrected kinesthetic space. It would literally blow Bizzi's mind, we suspect, if you could say (in advance, for any reasonable set of endpoints) how a trajectory would be idiosyncratic, and why. But that is for the future.

More integer arithmetic might be welcome to those without coprocessors. Yep, it is a pain in the posterior, but it ran only 1/10th real time on Pat's 16MHz 386SX. (A lovely 1/3 real time on my lowly 12 MHz 286 -- but with a souped up IIT 287!) Again, for the future. "Real scientists are patient."

Why don't the visual and mapped modes work well together? How do you plan to make them cooperative (or at least coexistent)? We assume that this will involve some fundamental postulates about whether vision just notes the end points or "helps" continuously. There are reports of more experiments on this besides the paper we are sending tomorrow.

If SCIENCE doesn't like this, BIOLOGICAL CYBERNETICS should!

Your labors have indeed been well-directed! We'd say "point on," but it would be a lousy pun....

Greg & Pat

Date: Mon May 25, 1992 5:24 am PST
Subject: PCT "free will"

From Greg Williams (920525)

>Bill Powers (920524.0900)

Memorial Day is a wonderful occasion for discussing whether history always matters to organisms, isn't it?

Is sending a 15KB retort to somebody with little time to reply as low a blow as picking on "least common denominators"? BTM, I think you underestimate the intelligence of (at least some) folks who believe in traditional free will; in particular, I think they are smart enough to understand that what you say PCT models offer as "free will" isn't what they want. Many people who believe in traditional free will are, no doubt, quite intelligent -- they just happen to believe something which doesn't accord with (unaugmented) PCT models. But some seem to overenthusiastically neglect that fact. My original problem with THOSE folks was with their saying that PCT models support their belief. You have reached the same conclusion: there is no traditional free will in PCT models. So much for that.

But you also raise some other interesting issues.

>When "these folks" (?) claim that free deliberate choices are completely
>independent of anything besides the self, do they include independence from
>the choices that are available to be made? That is, if you're presented
>with the choice of eating a dish of Jello and eating a dish of spaghetti,
>are you free to choose to eat a cheese sandwich, even if there isn't one?
>The very concept of "a choice" (however it's made) seems to imply some
>extant alternatives among which to choose -- in other words, a world apart
>from the chooser over which the chooser does not have total control, which
>the chooser did not choose to exist. If you have total control over the
>universe, no choices need be made.

It might be better to ask the folks, but I'll attempt an answer. The "independence" is taken to refer to the choosing itself, given that the alternatives are provided. The "self" is supposed to be the sole arbiter of eating Jello or spaghetti. All of the "possibility" conditions are assumed to have been taken care of, too -- that is, there are no physical constraints preventing eating Jello or sphagetti.

>Second difficulty: how would anyone show that a "deliberate" act was "non-
>random?" As you present the idea, this would be impossible to demonstrate.

I'm not sure that it is possible, to SHOW. I think the Believers rely on how it FEELS. And, of course, there is Doctrine in the background: God wouldn't be "fair" if he judged you on the basis of "randomly" caused acts, yet He does judge you on the basis of some of your acts -- those that are "non-random." I can't justify this reasoning scientifically, because some of its terms are not scientific (including "random," which I think is basically a religious notion when applied to physics (but not when used in pure mathematics) -- but that is another, and very long, story).

>If you're fairly describing the best thought of those who support the
>concept of free will, it's hard to understand how "they" could have ignored
>such elementary difficulties.

It's hard for YOU to understand, because you want to avoid ultimate rcourse to "God's mysteries"! You (me, too -- I'm not picking on you) are a bit too hubristic for these folks, who would deny the ultimate naturalization of everything.

>The only interesting challenge would be the one presented by the most thorough
>and competent thinkers who speak of free will.

I have tried to fairly characterize traditional free will. Perhaps some of them could be persuaded by the (in other respects) fertility of PCT models to switch to believing in a different kind of "free will" which IS supported by the models. I have no problems with that.

>I have proposed the possible locus of free will to be the process of
>reorganization. I would rule out reason, thought, emotion, principles, and
>system concepts as evidence of free will, because we can understand such
>things as being the product of learned computations in a physical brain
>obedient to physical properties of matter. I would also rule out as free
>will any truly random processes in the reorganizing system, because there
>can be automatically disorderly (or at least chaotic) processes at work as
>well, as in E. coli.

Listen well, ye believers in traditional free will!

>But I can't rule out the possibility that there is an uncaused but orderly
>influence at work during the process of reorganization that can say, for no
>reason whatsoever, "Let's try it this way."

Right. You COULD put such an "influence" ANYWHERE in a PCT model, like Maxwell's demon, altering, say, a perceptual signal here, a reference level there, and so forth. Why restrict the demon to reorganization only? If you want to augment, well, then AUGMENT!

>Nor can I rule out the possibility that this kind of orderly influence
>accounts for ALL of the apparently random process of reorganization.

Or for any apparently random "fluctuations" in on-going, moment-to-moment control.

Why you don't put the demon everywhere is because you think it is unnecessary. So why is it necessary in reorganization? So far, it looks like the only reason is to preserve a notion of "free will." Are there any non-polemical reasons?

>Quite likely, my conclusion about free will would not satisfy many of those
>who already believe in it. I don't particularly care about that. True
>Believers don't listen to reasoned arguments anyway, nor are they
>interested in risking any real investigation of belief that might turn out
>"wrong." If one already knows the answer, why go looking for it again?

Again, my original point was that True Believers shouldn't claim that PCT models bolster their belief.

>Bill Powers (920524.1230)

>Your main point is that the PCT model doesn't contain any basis for
>assuming a self with a free will. I think I can dispute this point.

Let's just keep in mind throughout the following that you are taliking about (shall we call it?) PCT "free will," not traditional free will. No sense in giving the traditional True Believers even a teeny ledge to hold onto!

>You say,

>>[GW:]
>>PCT models imply that an individual's current control structure is the
>>result of past interactions between the (changing-over-time) control
>>structure and its niche, and NOT the result SOLELY of an autonomous
>>dynamics of the control structure (which would mean that when
>>reorganization stopped, in any particular case, would be independent of
>>the niche, denying the very reason for reorganization).

>The implication here seems to be that the current control structure is some
>REGULAR function of past interactions with a niche.

Yes, indeed -- but not necessarily a non-random-appearing function.

>But reorganization occurs precisely when interactions with the niche lead to
>loss of control -- that is, when the current regularities in the interactions
>are insufficient to preserve control.

That's when reorganization STARTS. What about when it STOPS?

>The reorganizing system is effective because the changes it institutes do
>NOT depend in any regular way on the current organization or the current
>niche. The whole point is to break out of the conflict or the circle or the
>failure -- the local minimum -- by trying something NEW. So the idea of
>tracing the current organization backward, while all right in a general
>sense, is wrong if it implies any predictable course of development.
>Reorganization breaks the cause-effect chain.

If "cause-effect" = predictably deterministic, sure.

>Reorganization -- that is, the actual output effect of the process -- is
>independent (save for the frequency with which reorganizations occur) of
>any prior causes.

Well, independent of conditions existing at the START of reorganization.

>The outcome of reorganization, to be sure, has to be such that intrinsic
>error is corrected; if it's not, reorganization simply continues. But there
>are uncountable ways of reorganizing that would result in correcting
intrinsic
>error, so that result is not a constraint on any particular act of
>reorganization. In fact, one episode of reorganization is just as likely to
>make matters worse as it is to make them better (unless, of course, there is
>an unsuspected systematic component in it). The statistics of reorganization
>are very different from the statistics of stochastic -- but on the average
>systematic -- casuation. Reorganization will work even when the changes it
>produces show no trend at all in any direction.

DURING reorganization, the process is a continuous unpredictable altering

coupled with testing to see if the problem has been solved (if it has, then the process stops). This IS stochastic, if one recasts randomness as apparent unbiasedness. And in practice, if not in principle, the TRAJECTORY of the process is unpredictable. But the OUTCOME of the process might be much more predictable -- after all, it is "(this particular) problem is solved." Knowing the problem (as posed by the prior-to-reorganizing) control structure AND the niche, in concert, would allow some predictability of the outcome.

>Also, we mustn't forget that what makes reorganization effective are not
>the individual reorganizational events, but the selection effects that
>terminate reorganization. All that is required is the existence of
>something that can say "There! That feels better." Or, of course, something
>that says "Oh, no! Reorganize!" In fact, we could accept a mechanical
>randomness generator that actually does the reorganizing acts, and limit
>free will to the single act of triggering a reorganization. The "awareness"
>part of free will would then superimpose its judgments of what is
>acceptable and what is not on auytomatic judgments about such things as
>body temperature and state of nourishment. Thus free will could select for
>outcomes acceptable to it simply by causing reorganizations until the
>result is acceptable. The grounds for acceptability need have nothing to do
>with the niche.

Fine, except that the awareness, if not a solipsist, has to base its constructed judgments on some inputs from the niche. But that isn't a major issue. You've provided a basis for PCT "free will" which I think is basically not controversial from MY point of view. It is another question altogether from the point of view of anyone who believes in traditional free will and wants to support that belief with PCT.

I hope those who believe in traditional free will and are looking for support for their belief have made this far: to PCT "free will," straight from the fountainhead. Sorry, folks. But don't feel too bad -- you can always augment (wherever you feel necessary)! Of course, PCT models augmented to have traditional free will seem spurious to many PCTers, including, given the above, the instigator of PCT. That ought to mean something to those who value authority!!

Hurriedly,

Greg

P.S. to Mary -- was I the tentative "student"? I'll check air fares/availability tomorrow.

Date: Mon May 25, 1992 6:39 am PST
Subject: Littlebaby...

[From: C.Love (920525.1000)]
[To: B. Powers (920522.1300)]

Earlier (920522) I wrote to Bill saying,
>>This gives me a dynamic range for the reference output of about +-0.5.

This caused some confusion since Bill replied with,

> Did you say what you mean? It sounds as if the error signal and the percept

>signal are inputs, with the reference signal as output.

Sorry Bill. The description was somewhat misleading now that I have reread it. It is sometimes *tricky* to describe the output signals/reference signals since they are they same except for the weighting factors inbetween the various layers. That is to say, a output signal from a higher elementary control module (ECM) as it propagates itself to lower modules becomes weighted and then *becomes* a reference input to a lower ECM.

In my previous description I mistakenly called an ECM output a reference signal. This was wrong (in the context) so I think this is where the confusion
crept into the discussion.

To be quite clear, percepts are inputs. Reference signals are inputs. The difference between these two, called the error signal, is the output (aside from filtering, amplification, transport lag,etc.).

I like your adaptation scheme Bill. I have worked through it and have rewritten it in a pseudo algorithm form and have one suggestion to point (5):

>.....Suppose you want the system to find its own best output gain (or
> integration) factor. The criterion for "best" would be some measure of
>changes in absolute error.

>.....(1) Define a delta that can be randomly selected from a range from
-
>1 to 1.
>.....(2) Multiply this "delta" by a very small number and add it to the
>.....current value of the output gain on each iteration.
>.....(3) The adaptation algorithm simply chooses a new delta at intervals
>.....that are inversely proportional to the rate of change of average
>.....ABSOLUTE error signal (plus a constant to avoid infinite intervals at
>.....zero error), i.e., If the rate of change of average error is positive
>.....(getting worse), (Fig. 1 (a)) choose a new delta (after a short
>.....interval).
>.....(5) As the rate of change of average error gets smaller and goes
>.....negative , *new* random deltas are chosen at longer and longer
>.....intervals.

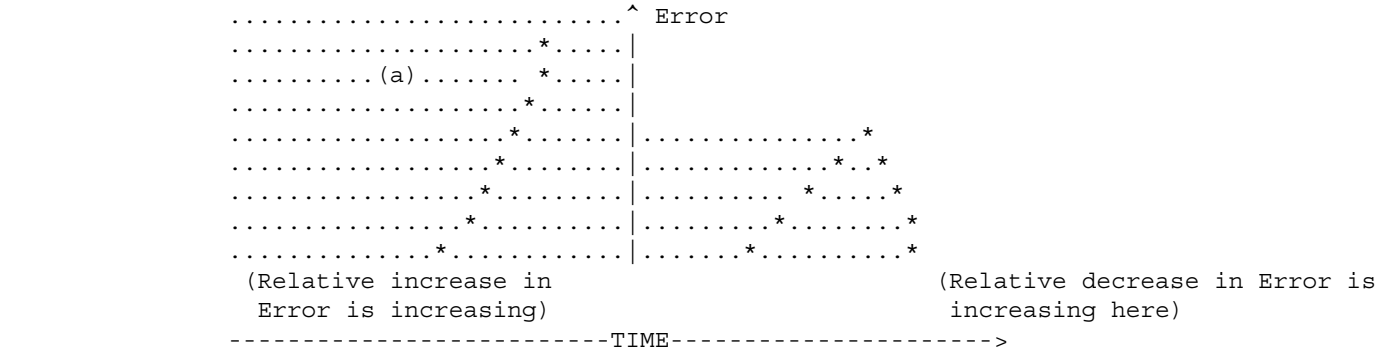


Fig. 1 Error-time plots. (a) Error graph shows error increasing with time with a larger incremental rate,i.e., rate of error is accelaerating. (b) Error

graph shows error is increasing at first then changing sign and decreasing at a larger incremental rate, i.e., rate of error is decelerating.

My suggestion is that if the 'rate of change' of error is decreasing over time using a particular random number then this random value is good and you may want to do one of two things:
(i) Leave it (since it is working correctly).
(ii) Enhance its value.

First, if you choose to follow step (5), choosing the random deltas at longer intervals makes sense, BUT the basic essence of my following suggestion is - if it's working don't touch it! So here it is....

>...changes in the right direction are allowed to continue in that direction for
> a *longer time* before a random change.

Why do you *have* to choose a *new* delta if the error slope is negative, i.e. the error is decreasing, especially if it is doing so more quickly at each interval; the rate of change of the *decrease* in error is increasing??? (refer to Fig. 1b) Unless you are worried about overshoot?

I realize that step (3) does this somewhat using the inverse proportionally, i.e, if the error slope is large (indicating a large changes) make the random number selection less frequent and vice versa. But what I'm suggesting is 8simply to try to *optimize* this characteristic.

>This is a surprisingly efficient process..
Ok. Maybe this is even faster. Is it important to be faster in adapting? I suppose it depends on what your application is, right?

Anyways Bill - it's only a thought I had while reading your mail.

Last thing.
You asked what I meant by,
>>"put up your feet and teach the baby" routine that uses the grid system.
Well I was referring to your,
>7. A learning mode that can be used when map control is on, gradually making the >kinesthetic space agree with the visual space. A random target positioning mode is >available so you can turn learning on with a cleared map, go away and read a >book for a couple of hours, and come back to find the map all adapted (1331 cells).

Well, have to go and implement some of these changes...
Talk to you guys later.
Chris.

Date: Mon May 25, 1992 1:22 pm PST
Subject: creative counseling

from Ed Ford (920525:14:20)

Mary - Thanks for your tip about no seat availability on some flights getting out of Durango on Sunday. I got the last seat on the Sunday noon flight out of Durango to Phoenix.

David, et all....

David (920521) says "a difference I've noticed between Ed Ford and myself in applying HPCT. Ed starts at the systems level and works downward. I start at a lower level and work upward.

This is not quite true in my case. I see counseling similar to creative writing. When writing, I watch ideas pop out of my mind, as if I had little to do with creating them. I just think about the area where I'm curious or trying to work out a thought and out something comes. It just pops up and there it is. Bottom line is that I take advantage of my reorganization system and let it work for me, like creative people do.

Counseling involves using the reorganization system, the same creative process. I don't start at systems concepts and work down any more than I start at a lower level and work up. I begin my session by talking with my clients (what else is there to do) about what they want, where they see their problem, a little bit about their life. I have in mind the major areas of importance in PCT that are applicable, such as priorities, values and beliefs, standards, decisions, various areas of perception, our actions, wants and goals, and stuff. Then I watch myself take certain directions, primarily areas in the clients' lives where both harmony and conflicts exist.

Actually, David, I think our difference is this: you tend to analyze, to think things through in a logical way, to be more analytical; I tend to just watch where I go, to let ideas come out of my mind, not constrict my mind but to let it creatively seek various paths to take. When I occasionally find my self uncomfortable with where I am or what I'm saying, then another idea pops into my mind and if it makes sense and is compatible with what I want, I go in that direction. That isn't to say that I don't have an overall structure in the way in which I work or that I don't think about what I'm saying. I'm thinking all the time, but it's within the creative process. PCT has given me a delightful structure, and I've added my own way of understanding and creative process within the boundaries of PCT.

I basically look for various areas such as where there may be two incompatible goals, or for goals they've established but over which they have little or no control accomplishing, and I also have them look at how they've structured their worlds and get them to evaluate the structure they've created.

Typical areas of conflict are where a job demands enormous time, a spouse and children need time, extended members of the family such as sick or lonely parents, physical activities (time at spa) or intellectual activities (time on CSG net). Here are all kinds of areas with interrelated and sometimes highly conflicting standards, decisions to be made, and the way the various systems have been prioritized. It is impossible for an outsider to know all the various areas of importance, their strength and priorities at any one time, the

varying standards and how their all interconnected within the total network of the person with whom you are dealing. That's why I think it's best to teach a person how to work out their own internal conflicts cause only they know what is really going on. All I know is my own created perceptions of what I think is going on.

I think a big mistake can be made if a person looks at PCT in terms of an individual area of concern and tries to analyze an area in isolation from other areas. Again, there's so much going on. I can't think of a single area of importance to me that isn't tied into lots of other areas of greater and/or lesser importance. Hester, my children, my various jobs, my health, my faith, my friends, CSGnet, things around the house, all kinds of other things as well. These are all very interrelated areas, all with various priorities, depending on the time constraints and other areas of importance. To look for the single or major reason or cause for what people do within their network of reference levels is rather misleading. There seems to me to be too much interrelatedness within our structure of our values and beliefs, how we've prioritized them at any one time, and all the various standards we've set. Added to this is how all of the above can be conflicting with various disturbances becomes apparent to us when we are attempting to control in various areas.

The most important thing I've learned from Control Theory is that I'll never understand another living control system, and they'll never understand me. To quote Clint Eastwood (one of my very favorite actors) "A man has got to know his limitations!" When living control systems come to me seeking help with various conflicts their having, I see my goal as a teacher. My job is not to figure out why they do what they do. Rather, it is to help them build confidence in their ability to deal with their internal worlds by teaching them effective and efficient ways of resolving their conflicts and establishing harmony within their worlds. Control Theory has given me more help in this area than anything else I've learned.

Rick..

I think that directing a person's awareness to levels (or areas) which aren't broken can be very productive. Obviously, if people are doing well in one or more areas, but their belief-in-self system isn't, then having them reflect on what they're doing well can be most helpful in rebuilding confidence. It is best to build from strength, not weakness. Also, sometimes it is best to build more strength in areas of success before attending to weaker areas. Again, as I was saying above, you just have to fuss around and help the client determine which is the best way to go. It isn't best to set hard and fast rules where you have so much going on.

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

Date: Mon May 25, 1992 9:17 pm PST
From: Dag Forssell / MCI ID: 474-2580
TO: Lars MBX: larsky@it.hos.se
Subject: arbetsl|shet
Message-Id: 45920526051754/0004742580NA3EM

[Fr}n Dag Forssell (920525.22:15)]

Hej Lars!

Du har sett f|r mycket amerikansk film/TV. Jag har bara haft en betald dag de senaste 15 m}naderna. Jag har investerat min tid i mitt program.

I dag, M}ndag {r det Memorial day, en helgdag h{r. I morgon skall jag be Gary Cziko skicka ett par saker till Dig. Men f|rst vill jag be Dig ber{tta litet om Dina planer. Vad {r systemvetenskap och informatik? (Lustigt svenskt ord)! Talar vi om Elteknik, servosystem, datorer, produktionsteknik eller samh{llskunskap? Till vem riktar sig Din bok? Gymnasiet? KTH? Om KTH: Maskin, EE, ?? N{r jag tittar noga efter, ser jag att Du skriver systemvetenskaplig linje. Det har jag aldrig h|rt talas om. (Jag ser kth i Din adress; vad betyder det? Var i geografin finns Du?)

Vad {r det Du {r intresserad av att l{ra Dig av Control Systems Group? Jag vill g{rna rekommendera med litet kunskap om vad Du vill uppn}.

S} en $64 fr}ga, helt seri|s: Vad styr ett styrsystem? I kortfattad teknisk detalj.

Du anv{nder }{| med klammer och linje. Skriver Du det eller {r det automatiskt fr}n Din terminal? Hur ser stora }{| ut?

L}t mig f|rresten g{rna veta om Du redan f}tt information om oss av Gary.

Till min tisdag.

Dag Forssell
23903 Via Flamenco
Valencia, Ca 91355-2808
Phone (805) 254-1195 Fax (805) 254-7956
Internet: 0004742580@MCIMAIL.COM

Ditt senaste kom fr}n en annan adress: larsky@jamvax.sunet.se
Vilken f|redras?

L}t mig n[mna att jag t{nker anlita Gary d{rf|r att han beh|ver inte betala porto f|r att skicka l}nga document.

Date: Tue May 26, 1992 3:14 am PST
Subject: Re.: creative counseling

To: Ed Ford
From: David Goldstein
Subject: Re.: Creative Counseling
Date: 05/25/92

Ed, I based my statement on how you do therapy from your book "Freedom From Stress." The impression I receive from this book is that you work in a top-to-bottom fashion. You ask people what is important to them and then ask them to rank order these areas. If

marriage is one of these areas, you ask them questions for finding out the principle level perceptions and goals for this area. Then you ask them questions for finding out the program level perceptions to achieve each principle level perception.

In your post you say that you do not work this way and went on to describe how you do work. Your therapy principle level generalizations are, based on your post: Be creative and spontaneous. Be a teacher, teach them about HPCT. Be sensitive to signs of conflict and harmony and focus on these areas. Encourage people to believe that they can solve their problems. At this level of generality, I would be surpised if we really differed.

At a more specific level you say: "To look for the single or major reason or cause for what people do within their network of reference levels is rather misleading." In the clinical example I described on CSGnet, I did try to understand the experiences of the man during the night when he was caught with the babysitter. Is this what you mean when you say I am analytic? Does HPCT not teach us to look for controlled variables? I did look at his specific actions and tried to identify what experiences were being controlled by them. So, here is where we may differ.

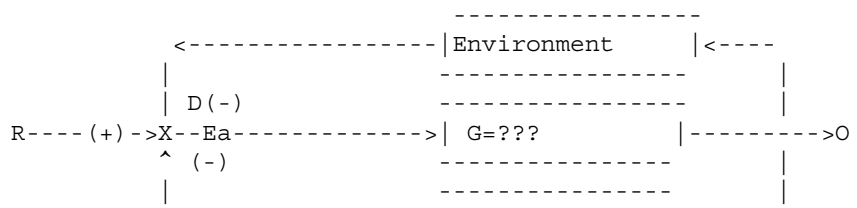
I must admit that it is not easy to do this in a clinical setting as is obvious from the example. But if HPCT has anything unique to say to therapists, it is: Identify controlled variables by means of the method of levels and the test for the controlled variable. In a clinical situation this is much harder than in an experimental situation. If we give up doing this, I am not sure of how HPCT therapy is really any different from other therapies out there.

I know that when you ask people " What do you want?" in the exploration phase of your counseling and when you ask people "Is it working?" in your evaluation phase, that you are moving in the direction of finding controlled variables. Maybe I simple go further in this direction through the explicit use of the method of levels. Asking people questions like you do certainly disturbs them and invites awareness to what is going on inside them.

Date: Tue May 26, 1992 8:14 am PST
Subject: Model concepts

Hi Bill:
I would like to say ask a few things about the elementary control module (ECM).

With respect to the basic closed loop diagram:



** (D) disturbance, actually the weighted perceptual inputs, are acting as negative feedback, therefore we should multiply this by (-1) before adding it into the summer (or subtract it in the appropriate way).

** The (X) is the summer/comparator junction.

** The (R) reference is the weighted perceptual inputs.

** Ea is the Error signal.

** What is G??? (I can only think that it is FNO + any other internal functions working on the error signal {This was deduced from matching the structure of the ECM to that of the basic negative feedback model})

** Now, since I cannot see what (H) is, I assume it does not exist as I have drawn it but, exists as the environment (as drawn).

** Output (O) is what is sent out of the ECM, whether it be to other ECMs or to the external environment.

This all started when I sat down and tried to model the steady-state solution. Then I began to think about it and realized that I didn't really know what (G) was. I mean, after that of course, you have to abe able to recognize its form, convert it to the (s) domain, solve for the partial fractions solution, or whatever method, and finally use the inverse Laplace transforms to get your steady-state solution, given what ever type input you provide (Type 1 -step, Type 2 -ramp, etc.). I think these are basic questions Bill, but I would like to have them clear in my mind.

Another reason why I started this general analysis is because I didn't know how to analyze "1+G", in order to determine the optimum "S" in your dynamic slowing equation!!! So I'm in a bit of a position...

I have your 1978 paper, "Quantatative analysis of purposive systems: Some spadework at the foundations of scientific pyschology", Pyschological Review, pg. 417-435, 1978.
I have started to review it.

Well, that's all. Thank-goodness.
Hope to hear back from you soon,
Chris.

Date: Tue May 26, 1992 6:32 pm PST
Subject: Baby; Arm; therapy

[From Bill Powers (920526.1500)]

If anyone has trouble getting a Sunday flight out of Durango (Aug. 2), contact Mary immediately. If we know how may people would like to leave on Sunday, maybe we can get an airline to lay on a larger plane.

Chris Love (920523) --

The random reorganizing method is certain less efficient than just going the right way and keeping on in that direction. However, the relationship between a change in error and a change in gain (or whatever the criterion is) isn't necessarily single-valued -- raising gain may lower error up to a point, and then make it increase again.

The point of using the random method is to have a single simple principle applicable to all kinds of optimizations, with the least amount of "smarts" possible. Intelligent methods of reorganization can easily be devised for specific applications, but in modeling behavior we then have to ask how the intelligent method got organized, where it gets the information it uses, and what perceptual and computational equipment it needs (and where that came from). A realistic reorganizing system has to be able to work before there is any organization at all, so it can't rely on higher functions that will appear only later (as a RESULT of reorganization), and it can't have any knowledge of what "makes sense" in the current environment. It's too easy for an adult human being to think of optimization methods that use logic, computation, and information about the objective situation. It's harder to think of a method when all those advanced capabilities are ruled out.

Why not write yourself a little program to play with random control? All you need to do is set a dot moving at constant speed on the screen, and arrange it so every time you tap the space bar the angle of travel changes at random. Put a little circle up as a target, then steer the dot to the circle by tapping the space bar! You'll be amazed at how easy it is.

Once you have this working, you can replace the display with a single dot that moves along a one-dimensional line toward a target, and position the visible dot according to the radial distance between the invisible dot and the invisible target in the now-invisible two-dimensional display. So invisibly, there will be a dot moving at constant speed and variable direction toward a target located in two dimensions, but what you will see shows only the radius along a single line. You will still be able to get the dot to the target in two dimensions, even though all you're seeing is a one-dimensional display of the radial distance. This little demonstration is worth pondering at length.

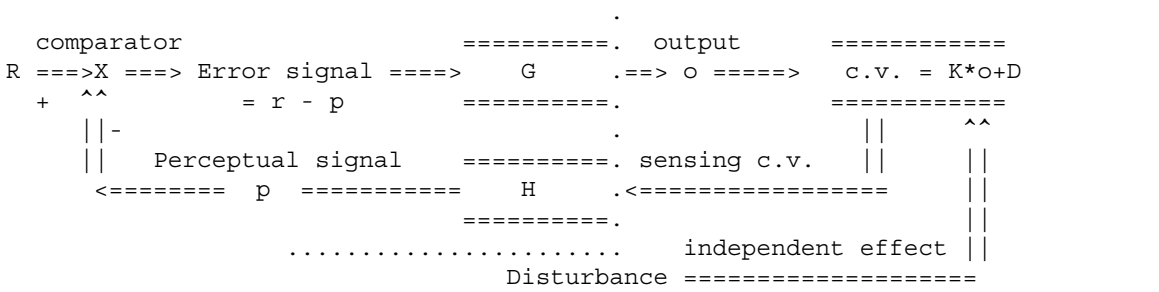
Thanks for the clarification on "putting your feet up." Now I remember.

--- (920526) --

Judging from the way you laid out that control-system diagram, you must have got it from some standard text on control engineering. If so, that "H" would probably refer to some sort of INTERNAL feedback connection, which we don't need (as you suspected). Or else it's meant to represent the perceptual function. In any case there isn't any indication of the controlled variable in the system! The "O" is the control engineer's concept of output: it has some sort of effect in the external world useful to somebody else. This form of the diagram is very limiting because it doesn't allow seeing the effects of action on the controlled variable along

with effects of independent disturbances. Also the role of the perceptual function gets completely lost. How the heck does the environment have a DIRECT effect on the comparator (X), without going through a perceptual function? I think the diagram is just too confused to use.

Here's a rearrangement that might make things clearer:



Now G is the output gain (FNO) and H is the perceptual function (FNI). The dots separate the control system from its environment; everything to the right and below the dots is environment. The disturbance acts on the controlled variable, not the comparator. The role of sensing is now explicit, and there's a place for the generic disturbance to act on the controlled variable outside the system.

Suppose both G and H are simple multipliers, and that the state of the controlled variable (c.v.) is K times the measure of output. Then the steady-state loop gain is just GKH. The optimum slowing factor to put in G is S = 1 + GKH. So the output variable o would be computed on each iteration as

o = o + (G*error - o)/(1 + GKH)

You can use a larger value of slowing factor to get a gradual approach to steady state.

Note that "loop gain" means the product of all multipliers encountered in one trip around the loop. Starting at the comparator you encounter G on the way to the output, K associated with the controlled variable, and H in the perceptual or input function: G*K*H. The loop gain is actually negative because the perceptual signal is subtracted at the comparator, putting in a hidden factor of -1. For computing the optimum S, ignore the -1.

In your servo texts, G and H are probably meant to be complex functions -- Laplace transforms. Ignore all that -- our way is easier and works just as well in simple cases. If you analyse most systems properly, they're all simple cases. Only the environment is complicated.

Ed Ford and David Goldstein (920525) --

Ed, that was a beautiful exposition of what I think of as HPCT therapy. If you understand how control systems work, you don't need any formal "method." Even the method of levels isn't a formal method -- it's just a way of bringing out the fact that one level of goals serves another of higher level. When you really SEE your client as organized to control at many levels, the way into the case, I should think, would practically define itself.

I can say, in defense of David, that there is merit in following some systematic approach, too. This is particularly true if you hope to teach this concept of therapy to others, particularly beginners. Not all people who do therapy are going to be as creative as Ed and David (and David is a lot more creative than Ed gives him credit for). I'm a firm believer (well, not totally soft anyway) in bottom-up teaching: do it this way and get right results; understand it later. A set procedure can provide a focus; later on, you will see (if the teaching is done right) that this is only one of many ways to get the same result. Of course too much teaching stops with the set procedure, but we know that principles have to be learned, too, don't we?

The moral: there is more than one style of HPCT therapy that will work.

Greg Williams (920524++) --

To change the relationship of "in" and "out" curves, play with the integration factors in the "S" list of parameters for Shoulder y and Elbow.

Thanks much for the nice word about Arm v2. I will take you up on the editing offer -- if you can pull together a coherent account of the arm from past posts, that will give us the nucleus of the article. I will then add intro and other material, and make some diagrams with PictureThis that you can check and edit. I'm hoping that you will take it upon yourself to provide links to the literature with commentaries. This will be, of course, a joint article.

Your OK of the Schwartz letter means it will go in the mail tomorrow.

Best, Bill P.

Date: Wed May 27, 1992 12:06 am PST
Subject: Meeting airline tickets

[From Mary Powers]
While United is booked (but check) for August 2 from durango to denver,

Date: Wed May 27, 1992 12:06 am PST
Subject: airplanes & meeting

[From Mary Powers (920526.1930)]

CSG Conference Update

The transportation picture is not as bleak as I thought. There are seats available on flights out of Durango August 2 on Continental - though United is almost fully booked.

One of the students for whom we were waiving fees will not be coming, so a spot is available for someone else. Graduate or undergraduate student, in case I was ambiguous about that.

The registration deadline is July 13, the last possible date before I nail down things like how many beds at Fort Lewis College. If you are planning to come I would appreciate hearing a

lot sooner than that, however. And it would be wise to make your reservations soon, if flying.

If you are driving, let me know and I will send maps.

Mary P.

Notes from Bill:

Gary Cziko's paper for Educational Researcher is superb. I have a feeling that our horizons are going to expand rapidly when it appears.

Speaking of which, Gary and Hugh and Ed, I presume you saw the news about Schmidt resigning as president of Yale to start a chain of "innovate" private schools. If he's looking for some new approaches to education --- why not PCT?

Date: Wed May 27, 1992 12:07 am PST
Subject: Reorganization project

When I get back home, I'm contemplating programing the following little Reorganization project, whose purpose is to illustrate Reorganization in the simplest possible setting (to see if I really understand how it's supposed to work). Comments welcome.

Astro is supposed to learn to go to a point in free space and stay there. Distance from the goal-point will constitute intrinsic error, driving reorganization. When it succeeds, it is started up again in a random position, in the state it attained on success.

Effectors: fore and aft thrusters.

Sensors: distance to desired location. (1 line)
velocity to/from " (2 lines)

Hardwired will be an ECS for maintaining a given positive velocity towards the goal point (so thrust-reversal on overshoot will be an automatic consequence). The distance circuit therefore needs only to measure absolute separation from the goal point.

What astro has to figure out is how to vary the velocity reference level w.r.t. the distance from the goal point so as not to overshoot, which it will do by altering the parameters of a distance -> velocity ref. level function.

The space of available position -> ref. level functions will be cx+b, b positive, so the astro will start in a situation where it overshoots wildly. The intrinsic error computation should then reward slower veloc. reference levels when near to the goal, since these will result in more time spent near this region, optimizing at b=0.

A further elaboration is to introduce another intrinsic error for thruster-use (fuel consumption), and to allow c to be altered by Reorg., to see if it manages to balance fuel-consumption vs. travel time.

Then there is the possibility of allowing irrelevant information,
to influence the velocity ref. level (such as, say acceleration),
& seeing if it manages to learn to ignore this (by setting its weighting
factor to zero).

Avery.Andrews@anu.edu.au

Date: Wed May 27, 1992 12:10 am PST
Subject: A CALL FOR HELP

From Tom Bourbon [052792 -- 0:46]

I need copies of books and computer demonstrations on PCT. In July, I will present material on PCT at a workshop-conference, Comparative Approaches to Cognitive Science 92, in France. The organizers have requested that participants bring copies of books they have written or edited. Since I have done neither, I would like to use material from others -- Wayne Hershberger, Bill Powers, Clark McPhail, Ed. If you can provide a copy of anything you wrote or edited, please send it to me. (I have several extra copies of the ABS issue on PCT and will take them, so Rick is off the hook.)

Also, please send the latest versions of any demonstrations or modeling projects that you would like me to show. I will have my own over-the-shoulder 386 DOS machine, and the organizers will provide a room with a DOS machine and a Mac. (I have zero experience with Macs, so please make it self-explanatory if you can. Chris Love, do you have anything far enough along to show, on LITTLE BABY?)

Please send all material, by the second week in June, to my new address (as of 1 June):

Division of Neurosurgery E-17
University of Texas Medical Branch
Galveston, Texas 77550

I hope some of you can kick in and help the lone representative of PCT. (Actually, Andy Papanicolaou, my colleague in Galveston, will arrive for a few days during the second week of the meeting. Like the cavalry riding to the rescue!)

Tom Bourbon <TBourbon@SFAustin.BitNet>
Dept. of Psychology
Stephen F. Austin State Univ.
Nacogdoches, TX 75962 Ph. (409)568-4402

Date: Wed May 27, 1992 4:29 am PST
Subject: book

[From Wayne Hershberger 920527]

(Tom Bourbon 920527)
>In July, I will present material on PCT at a
>workshop-conference, Comparative Approaches to Cognitive Science
>92, in France. I need copies of books and computer
>demonstrations on PCT.

I am honored to provide you a copy of Volitional Action: Conation

and Control. I will mail one today to the following address

Dr. T. Bourbon
Division of Neurosurgery E-17
University of Texas Medical Branch
Galveston, Texas 77550

I have been thinking about you and the conference this summer--admiration and envy mostly, but, also, Joyce and I are leaving for France next week, and I was hoping to be present in Aix-en-Provence for your presentation; alas, it didn't work out. However, you can be sure that we'll be with you there in spirit, and eager to debrief you upon your return.

Give my regards to Andy. Warm regards, Wayne

Date: Wed May 27, 1992 5:43 am PST
Subject: PCT concepts and France

[From: Chris Love (920527.1000)]
[To: Bill Powers (920526)]

Thanks Bill for "clearing up" the model details. And yes, I was using a basic engineering control theory text, "Modern Control Systems", Richard C. Dorf. I got a little confused from what I was taught and what you are doing, but your explanation was very clear and I feel my understanding has improved a lot.

I see your point about the random reorganization method also. I never thought to consider that,
> raising gain may lower error up to a point, and *then* make it
>increase again.

I will also give the random dot experiment a try. Should be easy enough to implement in Prograph.

I wish I could attend your PCT conference because I know I would really enjoy it. I will try to plan for it next year. I didn't know about it soon enough.

[To : Tom Bourbon (920527)]
Hi Tom,
I donot yet have my Little baby up and running. What I do have though is an early Beta version of him. The intent of this version was to get the environment up and running and to check the angle relationships in the arms, etc.. I suppose it was, more or less, a debugging version. The entire graphical display is functional with all the buttons and stuff. The baby does not use any PCT in this version. What it does use is three *rules* to change the arm angles.

The first rule says that if the target is to the left of the finger tip then swing the arm (at the shoulder) left (and vice versa).

The second rule says if the target is above the fingertip then move the arm (at the shoulder) up (and vice versa).

The last rule says if the target is further out (depth) than the fingertip then bend the elbow outwards (and vice versa).

As you might imagine, these rules are hardly sufficient to correctly map the space but it does work well in *nice* regions, i.e., don't put the target above his head or at his toes!

This version has been compiled and is executable. I'm sorry that my PCT version is not running yet. Hopefully soon!

Since this Beta version does not use PCT (but is interesting all the same) I will leave it up to you whether you would like to show it. I suppose it could be used to show the idea of what I'm doing and give an impression of what it will look like. What I could do though is also provide a few 8.5X11 page pictures of the *proposed* structure of the current model???

Let me know what you think of this. If you feel it would not be warranted to discuss, no problem (no offence taken - it is kind of early in the development cycle still).

Thanks for the offer, Chris.

Date: Wed May 27, 1992 6:23 am PST
Subject: Arm "Ballistics"

[from Gary Cziko 920527.0845]

To Bill Powers & Greg Williams:

The arm demo sounds like it has the potential to stir up lots of problems for the motor control people. I'm anxious to get this on the fileserver so CSGnetters can play with it.

It just so happens I've been looking at the "manual control" chapter of a text on engineering psychology (Wickens, 1992, who just happens to be the advisor of the psychology grad student living next door). Research on "discrete movement" is reviewed concerning a stylus in the hand which is moved from a start to a target. As Wickens summarizes (pp. 448-449):

"Two important characteristics of this pattern are apparent: (1) The general form of the movement is that of an exponential approach to the taret, with an initial high-velocity approach followed by a smooth, final, "homing" phase. In the earliest research in this area, Woodworth (1899) distinguished between these two phases, labeling the first the _initial ballistic_ and the second _current control_. (2) The velocity profile of the movement shown in Figure 11.3b reveals that control is not continuous but appears to consist of a number of discrete corrections, each involving an acceleration and a deceleration."

So, two questions come to mind: (1) is this an accurate description of the movement involved? and (2) if so, does the Arm demo behave similarly?

By the way, I've come up with what I feel is neat little demo of the closed loop nature of apparently open-loop behaviors. I'll show it at Durango. I

wonder if anybody else has thought of pulling on elastic bands attached to Michael Jordan's wrists as he makes a shot at the basket (maybe the Knicks should have tried this; they tried just about everything else).

Reference:

Wickens, C D. (1992). _Engineering psychology and human performance_. (2nd ed.). New York: Harper Collins.

Gary A. Cziko Telephone: (217) 333-4382

Date: Wed May 27, 1992 7:01 am PST
From: Dag Forssell / MCI ID: 474-2580
TO: Lars MBX: larsky@jamvax.sunet.se
Subject: Kontrollteori

Jag skickade det h{r p} m}ndag till den adress Du angav i Ditt f|rsta brev. Du tycks anv{nda tv}. H{r {r en upprepning f|r s{kerhets skull.

[Fr}n Dag Forssell (920525.22:15)]

Hej Lars!

Du har sett f|r mycket amerikansk film/TV. Jag har bara haft en betald dag de senaste 15 m}naderna. Jag har investerat min tid i mitt program.

I dag, M}ndag {r det Memorial day, en helgdag h{r. I morgon skall jag be Gary Cziko skicka ett par saker till Dig. Men f|rst vill jag be Dig ber{tta litet om Dina planer. Vad {r systemvetenskap och informatik? (Lustigt svenskt ord)! Talar vi om Elteknik, servosystem, datorer, produktionsteknik eller samh{llskunskap? Till vem riktar sig Din bok? Gymnasiet? KTH? Om KTH: Maskin, EE, ?? N{r jag tittar noga efter, ser jag att Du skriver systemvetenskaplig linje. Det har jag aldrig h|rt talas om. (Jag ser kth i Din adress; vad betyder det? Var i geografin finns Du?)

Vad {r det Du {r intresserad av att l{ra Dig av Control Systems Group? Jag vill g{rna rekommendera med litet kunskap om vad Du vill uppn}.

S} en $64 fr}ga, helt seri|s: Vad styr ett styrsystem? I kortfattad teknisk detalj.

Du anv{nder }{| med klammer och linje. Skriver Du det eller {r det automatiskt fr}n Din terminal? Hur ser stora }{| ut?

L}t mig f|rresten g{rna veta om Du redan f}tt information om oss av Gary.

Till min tisdag.

Dag

Date: Wed May 27, 1992 7:09 am PST
From: Dag Forssell / MCI ID: 474-2580
TO: Gary MBX: G-CZIKO@UIUC.EDU
Subject: Your paper

[From Dag (920527)]

Gary, I have been waiting for your paper. I conclude that my request got overlooked since I asked for the present starter package at the same time. May I please have a copy?

Heard from Lars. Have asked him for some detail so I can make recommendation that makes sense. He plans to write a book on "Systems science". Fresh doctorate in same. Do you want me to forward our correspondence for your enjoyment? There is a linguist in all of us???!!!

Best, Dag

Date: Wed May 27, 1992 7:00 am PST
Subject: Arm control

I am very much interested in the current discussion on arm control. The reason is that we have built a behaviour-based robot arm controller which enables a five degrees of freedom robot arm to grasp a rolling ball using 2D vision input from a hand wrist camera. (The approach and technical details of this work are described in a technical paper: Asteroth et al. "Tracking and Grasping of Moving Objects - A Behaviour-Based Approach", GMD Working Paper No. 603, December 1991).

The basic idea is to have individual behaviours which interact through a subsumption-like architecture controlling the robot arm. Each behaviour is triggered by particular aspects of the visual input (e.g., light intensity at an edge of the image triggers the orientation of the robot - base and wrist joints are acting; a centered light spot triggers the robot arm to approach the rolling ball - shoulder and elbow joints are acting; a camera image having many white pixels - due to a ball very close to the wrist camera - triggers a grasp reflex). All behaviours operate in parallel. No apriori assumptions are made about velocity and direction of the ball.

It would be very interesting to us to have a more "natural model" of decomposition into different behaviours. At the moment, our decomposition is based on our intuition. Additional, it would be interesting to find out whether it is possible to describe such a model of behavioural organisation using a system theory point of view in order to compare the properties and the performance level of our system to other "classical" robot arm controllers.

Looking forward receiving your comments.

Uwe Schnepf
AI Research Division
German National Research Center
for Computer Science (GMD)
Schloss Birlinghoven
P.O. Box 1316
5205 Sankt Augustin 1
Germany

e-mail: usc@gmdzi.uucp
usc@gmdzi.gmd.de
phone: +49-2241-142704
fax: +49-2241-142618

Date: Wed May 27, 1992 11:26 am PST
Subject: Arm model; scholarship

[From Bill Powers (920527.1000)]

Uwe Schnepf (920527) --

Welcome to the speaking population on the net, Uwe!

>I am very much interested in the current discussion on arm control.

Good. The schedule for release of the model, roughly, goes like this. Greg Williams is evaluating it now, and preparing some preliminary material for an article on it, which Greg and I will submit to Science (and if they don't take it, Behavioral Cybernetics). The article should be submitted by the early part of July. I will be busy working up figures and adding a few little refinements to the model, as well as working on the writing, until then.

After the article is submitted, I will release the Arm Demo Version 2.0 or 2.1 for general consumption. The release will include an abbreviated writeup without figures, summarized instructions, and complete C- language source code with all special header files and object files needed to compile it under Borland Turbo C version 2.0 or later. The target platform is an IBM compatible 286, 386, or 486 machine using Hercules (720 x 348), EGA (640 x 350) or VGA (640 x 480) graphics. I'd love to do a version for other machines, but don't have the hardware (or expertise). The shareware asking price will be $100 US. This crass commercialism is due to the fact that I have no institutional or grant support for my work: everything comes out of my pocket, which is currently empty.

I think you may find the model to be useful in your work, although it is designed as an exploration of how the real human system is organized and not just as a way to get a particular job done. As you no doubt realize, it uses a hierarchical structure rather than a subsumption structure. It uses binocular vision, which is easy to do in a simulation but much harder to implement in hardware. Its motor actions are created by torques applied at three frictionless joints. Its visual inputs and joints are positioned in the anatomically realistic way (I'm not sure how the system would work with the eyes mounted on the wrist!). Also, in my model there is no "triggering" of any motor behaviors; all actions are smooth and the variables involved in their control are continuous. Of course I realize that if grasping is involved, there has to be timing of the initiation of various actions that are appropriate only after certain control actions are complete.

A general question, to you and any other experts in robotics who are listening in.

I need a way of simulating the movement of jointed masses that could be extended to a model of the complete body. Intuition tells me that there must be a simpler way to do this than by solving LaGrangian expressions involving potential and kinetic energy. Doing it this way with a body having four multiply-jointed appendages, a bending and twisting trunk, and two swivels at the neck is far beyond my mathematical abilities.

In a simulation, it's easy to apply forces to a single mass and integrate the resulting angular and lineal accelerations to produce

velocities and positions. The mathematics gets complex but the simulation doesn't have to use analytical expressions: it just has to make the body move realistically, for a short time, under applied forces. The integrations don't have to be very accurate when control is involved; any little errors are simply equivalent to small control errors, which are self-correcting; in effect, initial conditions are re- established every iteration.

I'm stuck, however, when it comes to doing this with even two masses joined by a hinge or ball joint. I can't figure out how to express the effect of the joint on constraining the motions of the masses. I know that there are analytical ways to do this (I'm using one in the arm model), but that's what I'm trying to avoid. I'm really trying to set up an analog-computer statement of the problem, so the simulation itself can solve the equations by "acting them out." I'm sure someone has done this. I'm not getting very far trying to do it by myself. HELP!

If we had a way of setting up such a simulation, this would quickly provide a basic structure to which control systems could be added, for a simulation of far more than just one simple moving arm. The basic organization of Arm v2. will work just as well for legs and a neck as for arms. The control-system part is relatively easy to implement, at least approximately. The hardest part is simulating the physics of the environment -- and as far as the control systems are concered, the environment includes the body, which is just a way of converting outputs into sensory signals.

I think we're close to being able to do realistic simulations of whole-organism behavior. This one little (!) problem is all that stands in the way. Anybody?

Joel Judd has claimed the remaining student scholarship for the meeting. If he has to relinquish it (by the first of July, please) we'll immediately notify everyone so someone else can have it. Couldn't think of a better recipient.

Best to all, Bill P.

Date: Wed May 27, 1992 12:01 pm PST
Subject: PCT & counseling

from Ed Ford (920527:10:55)

David Goldstein(920525)

>The impression I receive from this book (my book, Freedom From Stress)
>is that you work in a top-to-bottom fashion. You ask people what is
>important to them and then ask them to rank order these areas.

This is one of the techniques I use to help people organize their internal systems so they know what's going on and can make choices of action accordingly. It certainly isn't THE way nor the only way I deal with others. In Chapter 7 of FFS (Reorganization: The Mind's Repair Kit) Page 96, lines 20 through 31, sort of encapsulates my ideas on the creative process.

>..if HPCT has anything unique to say to therapists, it is: Identify

>controlled variables by means of the method of levels and the test for
>the controlled variable.

I don't really think PCT actually says anything in particular. The greater the understanding one has of how the whole system works, the more creative a therapist can be in coming up with all kinds of ways to teach people how to deal with themselves. Certainly the ideas in PCT provide the creative mind various ways to more efficiently help others produce harmony within their own worlds. One of the keys to helping others is, as you suggest above, the whole concept of controlled variables, and how they can be used within the counseling session. However, I believe there is a lot of intriguing ideas that flow from PCT that therapists can use, the controlled variable being one of the more important. And finally, I agree with you. I really don't think that we are that far apart in our thinking. It's just that we come from such completely different backgrounds and ways of perceiving that our approaches are bound to have their own unique differences.

Rick -

Congratulations on "Mind Readings: Experimental Studies of Purpose". The book looks great. Greg Williams did a great job on the publishing end. Bill's foreword was most helpful, especially for people like myself, in explaining the whole need and understanding of modeling. You should be very proud.

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

Date: Wed May 27, 1992 2:13 pm PST
From: g cziko
EMS: INTERNET / MCI ID: 376-5414
MBX: g-cziko@uiuc.edu

TO: * Dag Forssell / MCI ID: 474-2580
Subject: Re: Your paper

Dag (direct):

>Gary, I have been waiting for your paper. I conclude that my request got
>overlooked since I asked for the present starter package at the same time.
>May I please have a copy?

I'm not sure if I sent it or not, probably not, so I'll send it today (will go out tomorrow).

>Heard from Lars. Have asked him for some detail so I can make rec
>ommendation that makes sense. He plans
>to write a book on "Systems science". Fresh doctorate in same. Do you want me
>to forward our correspondence for your enjoyment? There is a linguist in all
of
>us???

Thanks for the offer, but I have enough language homework trying to keep my French, German and Spanish alive. The first one was fun to decipher, but that's about all I care to do in Swedish for now.--Gary

P.S. Ray Olsen, a graduate student here in computer science and on CSGnet, is American but has spent considerable time in Sweden and I understand he is quite fluent. You might want to copy him. His address is olsen@suna0.cs.uiuc.edu.--Gary

Date: Wed May 27, 1992 2:38 pm PST
Subject: Bunge references

[Francisco Arocha]

RE: References by Bunge

The following is a post sent to Gary Cziko listing some of the most important publications by Mario Bunge.

Gary:

The most important ideas of Bunge are summarized in his Treatise on Basic Philosophy. Although the title may suggest a broader scope, this work is devoted to the foundations of factual sciences (Bunge classifies the sciences into formal and factual; formal being mathematics and logic and factual being the rest). The treatise is composed of the following books:

Treatise on basic philosophy: Vol. 1. Semantics: Part I: Sense and reference. Boston: D. Reidel (1974).

Treatise on basic philosophy: Vol. 2. Semantics: Part II: Interpretation and truth. Dordrecht, Holland: D. Reidel (1974).

Treatise on basic philosophy: Vol. 3. Ontology: Part I: The furniture of the world. Dordrecht, Holland: D. Reidel (1977).

Treatise on basic philosophy: Vol. 4. Ontology: Part II: A world of systems. Dordrecht, Holland: D. Reidel (1977).

Treatise on basic philosophy: Vol. 5. Epistemology: Exploring the world. Dordrecht, Holland: D. Reidel (1983).

Treatise on basic philosophy: Vol. 6. Epistemology: Understanding the world. Dordrecht, Holland: D. Reidel (1983).

Treatise on basic philosophy: Vol. 7. Epistemology: Philosophy of Science and Technology: Part I. Formal and physical sciences. Dordrecht, Holland: D. Reidel (1985).

Treatise on basic philosophy: Vol. 7. Epistemology: Philosophy of Science and Technology: Part II. Life science, social science, and technology. Dordrecht, Holland: D. Reidel (1985).

Treatise on basic philosophy: Vol. 8. Ethics, the good and the right. Dordrecht, Holland: D. Reidel (1989).

There is also the methodological book

Scientific research, Berlin: Springer-Verlag (1967).

and two volumes on his work:

Agassi, J. & Cohen, R. (1981). Scientific philosophy today: Essays in honor of Mario Bunge. Boston/Dordrecht: D. Reidel.

Weingartner, P. & Dorn, G. J. W. (1990). Studies on Mario Bunge's treatise. Amsterdam: Rodopi.

A great deal of his work is devoted to the philosophy of physics (he was a professor of theoretical physics):

Foundations of physics. Berlin: Springer-Verlag (1967).

Problem in the foundations of physics. New York: Springer-Verlag (1971).

Philosophy of physics. Dordrecht: D. Reidel. (1973).

Many of th ideas in the epistemological books are supported in the first four volumes, those comprising the semantics and the ontology. So, reading these first is, I think, important for understanding the rest. Not that the other books are difficult to read, Bunge's prose is very clear; but that the concepts used in the books 5 and on are presented in the first four.

Date: Wed May 27, 1992 6:58 pm PST
Subject: Re: anything goes (Re: Science & Truth)

Dear Francesco,

There is no absolute truth.

There are no true propositions, except relative to a set of observations (empirical "truths") or formal conventions (analytical or conventional "truths"). To attempt to discuss the truth/falsity of propositions outside of at least one of these contexts is meaningless.

The formal conventions tell us absolutely nothing about the world -- only about the consequences of our own (constructed) systems of rules. Observations are drastically impoverished images of the world beyond our measuring devices -- they do not mirror that world, they cannot capture all aspects of that world, and we never know exactly what it is that we are measuring (only that we can replicate the measurements and make good use of the results to act on them.)

This is not to say that models cannot be evaluated (e.g. relative to some specified pragmatic criteria -- how well they allow us to understand or predict phenomena of interest to us and/or to influence the world in accordance with our desires).

Van Fraassen's The Scientific Image more or less heads in this direction, as do other pragmatist conceptions of science (see also Munevar, Radical Epistemology). These approaches are equally far from the irrationalisms of "postmodern" "textualisms" (which assume that all relations between scientific models and the external world are constructed by the scientist, and hence are

completely subjective) and the various realisms (which assume some (knowable and finite) relationship between scientific models and the world beyond our instruments).

Peter Cariani

Date: Thu May 28, 1992 4:23 am PST
Subject: ga ga over GA

[From: Bruce Nevin (Thu 920428 08:13:10)]

Gilbert Syswerda passed this from John Holland on to the machine-learning list yesterday:

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

On an entirely different matter some of you may be interested in an upcoming Bill Moyers program that will be aired the evening of June 9. It concerns the Santa Fe Institute and the construction of predictive models. It involves Murray Gell-Mann, Marcus Feldman and me plus representatives from Brookings and World Resources Institute (we're in a consortium funded by the MacArthur Foundation). You also may want to know that the SCIENTIFIC AMERICAN article on Genetic Algorithms will appear in the July issue (which usually shows up after the middle of June).

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

Bruce
bn@bbn.com

Date: Thu May 28, 1992 9:20 am PST
Subject: PCT bibliography update

From Greg Williams (920528)

I plan to start my annual update of the PCT bibliography very soon (for distribution at the CSG meeting). I would appreciate receiving any PCT-related materials of recent vintage from netters for inclusion in the bibliography and/or filing in the CSG archive. Please send published papers, books, unpublished manuscripts, dissertations, and pointers to such.

Thank you!

Greg Williams, CSG Archive, 460 Black Lick Rd., Gravel Switch, KY 40328 U.S.A.

Date: Fri May 29, 1992 4:56 am PST
From: larsky
EMS: INTERNET / MCI ID: 376-5414
MBX: larsky@jamvax.sunet.se

TO: * Dag Forssell / MCI ID: 474-2580
TO: 0004742580

EMS: INTERNET / MCI ID: 376-5414
MBX: "0004742580@mcimail.com"@kth.sunet.se
Subject: fattiglivet i sverige

Hej Dag:

Vi skulle kanske kunna tävla i vem som är fattigast och vem som har de sämsta framtidsutsikterna, fast jag känner mig helt övertygad att det är du som är kapitalisten och jag proletären. Nåväl, det ger nog ännu mindre än vad man hittils fått ut av sin akademiska karriär. Hur som helst har jag inget annat val än att bli docent och därför skriver jag en lärobok, det skall docenter göra. I boken kommer jag att presentera 20 st uppmärksammade systemteorier och jag befinner mig just nu i en urvalprocess vilka jag skall ta med. Jag fick Powers rekommenderad och eftersom jag inte visste nåt om honom så måste jag skaffa mig kunskaper om hans teori.

Systemvetenskap är applied science till systemteorien och dess huvud-sakliga vetenskapliga och tekniska verktyg finns inom Informatics, dvs datorer, comm utrustnigar olika nätverk, litteratursökning m.m.

Dess metoder är Systems Approach, Systems analysis, Systems engineering etc. De kan användas inom alla storskaliga mänskliga system till fel-sökning o design av bättre system.

Alla svenska universitet och vissa högskolor har en systemvetenskaplig linje fast i praktiken är det mest en datologilinje. (Man skall inte studera systemvetenskap för det kan man inte leva på och ingen begriper vad man håller på med och varför)

Geografiskt sitter jag på högskolan i östersund, bara sitter för jag har inget jobb där. men jag har mitt e-mail konto där. Jag bor ute på landet i en bondgård där jag fn har köttdjurspoduktion. Vi slutade med mjölk-korna innan jag for till libanon som fn-officer.

Boken riktar sig till studenterna och doktoranderna som jag ibland sporadiskt undervisar.

Mina e-mail förmedlas av datornoden i KTH, sthlm därför ingår det i min adress. Mina mystiska tecken och paranteser genereras av datorn i comm nätverket.

Det som styr ett styrsystem är naturligvis ett Metasystem av olika typ i olika fall.

Hälsningar från en fattigakademiker.

Date: Fri May 29, 1992 12:44 pm PST
Subject: in memoriam

[From: Bruce Nevin (Fri 920429 15:07:41)]

I posted the following to the linguist digest today.

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Last Thursday night, May 21, Zellig Harris died in his sleep after a pleasant working day. He was 88 years old. He was born in 1904 in Byelorussia. I am told that he chose the name Zellig Sabbettai when his family immigrated to the United states when he was four. I like to think that the semantics of happiness and steadfastness were on his mind. Certainly they were keynotes of his life. I would guess that his parents chose the name Harris.

When he died, he was just finishing a book on politics that he had been planning for most of his life. With the 1992 publication of his book _A Theory of Language and Information_ (Oxford), he had wrapped up his life's work on language, at least for the time being. He seems to have felt at liberty to take up this other unfinished business. I understand from Paul Mattick, Jr., who was Harris's friend and neighbor for many years in New York, that this last book describes how to get from capitalism to socialism. This is surely not a conventional take on either capitalism or socialism, Harris was an anarchist. Oxford was interested in publishing it, and he had also talked with Cambridge.

There is no memorial planned, beyond something very private for his family. However, there is some discussion beginning of a public meeting with scientific content. I would hope that the festschrift that Haj Ross called for in the LSA meetings some years ago might at last come into being.

Harris described himself as a methodologist rather than a linguist. This could be misleading. He always said that his work was not part of linguistics as it is institutionally defined, and that linguists would not be interested in his work, though people interested in language would be.

Nonetheless, he was surely a linguist by most of the operational definitions one might come up with. He had done extensive fieldwork on a variety of languages. When he was doing the final revision of the 1992 Oxford book, he undertook to test the theory of language against every language of which he had some control, 44 languages. He spent months reading grammars from morning to night, and evaluating whether his theory had a reasonable account for what he found there. He was clear that no scientific conclusions were warranted, and so no particular notice of this check is given in the book, but he wanted to feel reasonably secure that his conclusions were not idiosyncratic to English, French, German, Korean, and the few other languages that had been the primary bases for their development. He was pleased with the results.

His contributions to the field were numerous and weighty. He founded the first linguistics department in the U.S. He introduced the algebraic representations and abstract mathematical treatment which have become so much norms of the field that it is difficult now to appreciate how much he did so over the kicking and screaming protests of his peers. He invented X-bar notation, though of course not by that name, to compensate for the well known weakness of Immediate Constituent analysis (aka Phrase Structure Grammar) with the head-of relation. He developed ways to accomodate discontinuous morphemes in grammatical analysis. He charted a way out of difficulties experienced by Bloch and others in phonology, by saying that contrast rather than phonetic identity is the

basis for setting up phonemes, a ghost that has risen to haunt generative phonology more than once. He invented string analysis as a complement (not rival) to immediate constituent analysis. Their complementarity with respect to the head-of problem is the basis of Joshi's Tree-Adjoining Grammars (TAGs). He invented transformational analysis in context of developing discourse analysis to get at the information content of texts. Other contributions await recognition and exploitation in the field of linguistics as institutionalized today, and in other fields. Obvious examples include sublanguage analysis and sublanguage grammar, operator grammar based on word dependency, discourse analysis for information content, and his theory of information as an account of a central aspect of semantics. For example, string grammar and its natural extension into transformational grammar is the basis of the very successful work of Naomi Sager and others at NYU in information formatting of sublanguage texts, applied there mainly to medical informatics. Stephen Johnson has implemented a system for representing the information content of texts, based on operator grammar. Successes of this sort are little noticed within linguistics.

It is characteristic of Harris that there was no vanity or self importance in him. He knew that his work was of lasting importance, and treated it as such, but he was no guru or empire builder seeking followers, and would not accept any such role being projected onto him. Those students who sought entree to linguistics as a social institution in academia were bound to be disappointed. However, he could scarcely be blamed for their disappointment. He did not provide such entree, nor did he pretend to, and in my hearing actively discouraged students who imagined work with him would further their ambitions in the field. Once, in my role as TA for John Fought, I prepared a lecture on Harris's approach to syntax and semantics. As we were setting out for the lecture hall, we encountered Harris, and I blurted out "I'm about to give a lecture on your theory to John's class." (John, with characteristic wry humor, asked if he wanted to take anything back.) Harris bemusedly questioned whether anyone would be interested in what he was doing. Nonetheless, when he gave a public lecture on "The two structures of language: report and paraphrase" in 1969 or 1970, the large auditorium (I think it was in the Furness building) was filled to capacity, and the critique by John Corcoran, published later in the volume _Transformationelle Analyse_ edited by Senta Ploetz, was also well attended. Broad attendance on and acclaim for his work could easily have been his, had he chosen it. That is simply not where his ambitions lay.

A clue as to the basis of this choice against fame and influence may perhaps be found in his advice to a student starting out in his first teaching position, many years ago. Don't invite anybody over for dinner, he said, and don't accept any invitations. If you get involved in the social life of an academic, you won't be able to get any work done. The work came first.

Harris was always an intensely loyal man to his friends and family. The consequences, when combined with his laissez-faire anarchism, were not always happy. His friend and close colleague of many years, Henry Hiz, was much more concerned with building a Formal Linguistics Program as an institution. The disparity of character could be devastating to students.

I studied with Harris from 1966 through 1970. I was an undergraduate much of that time, but that did not matter to him. He had a sink-or-swim approach like that attributed to Sapir (Darnell 1990), except that his seminars were of course focussed on theory rather than the data of, say, Athabaskan. He would come in to his seminar and just start talking about what he was working on. When I started with him, this was the work that resulted in his 1968 book, _Mathematical Structures of Language_ (Wiley). The process was not a lecture or monologue, but a continuing conversation with his students, trying out alternatives, posing and working out problems for a mathematical characterization of language. After a while, with intensive reading outside, one began to catch on and to participate.

I recall telling him at the end of one seminar meeting in my first year that I would try to disprove his theory. This troubled him not a bit. I worked up a problem in Modern Greek that I thought might be troublesome for his approach. (I had lived in Greece for a couple of years, and spoke the language, but I worked with an informant for this project.) When my results turned out actually to corroborate the point I had intended to challenge, he merely thanked me for the data on Greek. A year or two later, I had come up with a proposal to analyze definitions in a dictionary to extract semantic primitives by a form of componential analysis, much as Martha Evens and now others have done. Although the notion of semantic features seems inimical in concept and method to his work, he said (and this is an exact quote) "Others have tried this and have failed, but you are welcome to try." I offer this in refutation of the sometimes heard view that Harris was dictatorial. I ran into conflicts in such matters with Hiz, never with Harris.

I have also heard it asked why he never retorted to attacks on his work. I think it did not matter to him. He did not expect his methods and results to be understood and taken up by everyone in the field of linguistics. Maybe his attitude differed in the 1940s, when he wrote the structural restatements and the manuscript eventually published as _Methods in Structural Linguistics_. (BTW, the title was to have said "Descriptive" but the publisher substituted the buzzword "Structural." I recall him saying, amusedly, "I don't remember whether they asked me or not.") Maybe his expectations of the field changed after some of Chomsky's followers began making him out to be the bad guy. I don't think so, based on his writings and on the testimony of some who were his students then. I never heard him comment on the commonplace attribution to Chomsky of the discovery of transformational grammar and the "transformational revolution." There is a passage in _The State of the Art_ (1968) in which Hockett attributes to Harris "nothing, or a long silence, after 1957," showing ignorance not only of things like string analysis, for which he might be excused, but even ignoring the 1965 paper Transformational Theory prominently published in _Language_. I showed this passage to Harris, and he shrugged. It did not matter.

In particular, I never saw any evidence that Harris opposed or blocked Chomsky's ambitions. In my experience it would have been entirely out of character for him. For example, it was Harris who proposed Chomsky to speak in his stead to the 1962 International Congress. A similar canard regarding Bernard Bloch has recently been laid to rest in an editorial in Language. One must I think be alert to the social psychology that leads some people to rewrite history so that their

avatar is depicted as an embattled hero. Now, an old Indian friend once told me that one cannot point a finger without having three other fingers of the same hand pointing back, so I hasten to add that this is not the picture I intend to paint here of Harris. He accomplished what he intended to quite well, thank you very much, and seems to have been quite happy in the process. The point is precisely that he seemed in no way embattled by attacks and uncomprehending misconstruals of his work.

And uncomprehending misconstruals abound. Frawley's review of _A Grammar of English on Mathematical Principles_ (GEMP) is a good example. He identifies Harris's operator grammar with predicate calculus, though Harris is at pains to delineate critical differences between language (a fortiori operator grammar) and language-like mathematical systems, including predicate calculus. Frawley can see in this comprehensive grammar only an attempt to do 1960s generative grammar in 1980, because he is unable to step out of the Generativist paradigm so as to understand Harris's work on its own terms. Another review (Eric Wheeler, 1984 in _Computers in the Humanities_) asserted that Harris's grammar was unable to account for certain familiar semantic problems--middle voice, the semantics of find vs. seek, and quantifier scope in examples like "someone was opposed by everyone." In my review (_Computational Linguistics_ in 1984) I showed how Harris did in fact account of each of these problems in the book. Michael Kac, in his review of Harris's selected writings, asked "why bother?" And indeed, from within the Generativist paradigm that must be the only plausible question. It is only in setting aside paradigmatic blinkers that one can see, having these writings in one place, how consistent and self coherent Harris's program has been over the years. Transformational grammar was not a revolutionary break but part of a continuous evolution.

I will mention only one other misconception about Harris's work, not because it is in any way fundamental but because it is so commonplace. I probably will be greeted with disbelief when I say that discovery procedures were not his aim. (Jim McCawley's witticism about Harris and discovery procedures in the collection traditionally circulated in May really reverses the roles of the teller and the butt of the joke.) It is not hard to see how linguists have come to this mistaken belief. Discovery procedures are an abiding fixture for linguistics as institutionally defined. When _Methods_ was published, linguists sought an aid to fieldwork and writing of linguistic descriptions. Now, discovery procedures are institutionalized as a whipping boy. This has colored perceptions of Harris's intentions and results.

For Harris, it was certainly of interest and value when redundancy on one level of linguistic representation could be used in a practical way to determine boundaries of objects on the next, but this was a corroborative byproduct, not an aim. The "constructional procedure" described in the 1955 paper From Phoneme to Morpheme was implemented in FORTRAN in the early 1960s and proven to work, and Ralph Grishman has had some preliminary success in implementing programs to discover word classes and rules of sublanguage grammar from sublanguage texts. But in general Harris did not think that discovery procedures were feasible. In particular, he told me he thought that grammatical analysis could not be done solely with a corpus or by asking informants, one had to control the language oneself. And then one had to work over the data to tease out pattern and wrestle it into coherent form, a lengthy and demanding process, as probably most of us know from experience. So much for the

popularized image of feeding in a corpus, turning a crank, and having a grammar reel out the other end.

In the introduction to _Methods in Structural Linguistics_, Harris states clearly that these methods are not discovery procedures. He accepts that one uses many means to come up with proposals for describing what is going on in a language--hunches, guesses, heuristic rules of thumb, typological generalizations, proposed universals, comparison with related languages or earlier stages of the language, and so on, more art than science (or rather, more art than engineering). Harris was acutely aware of the danger of swamping one's control of the language by growing familiarity with marginal examples. Language is after all a social institution, continuously in change as it is constantly recreated in the crucible of use. The aim of the methods was not to substitute for these informal ways of coming up with possible analyses, but to verify, for any given result, whether the result had a valid relation to the data of the language. Of those who have actually read the book, how many have said (and some have in fact said to me) "he didn't really mean that." But if nothing else, Harris was always careful to say exactly what he meant.

This concern for verification arises out of a deeper concern which becomes more explicit in Harris's later work. This is a critical point for linguistics. For any other science, there is a standpoint external to the science domain for its metascience. In particular, practitioners in physics, chemistry, even in mathematics, rely on the "background vernacular" of language to ensure communication about shared meanings and ultimately to validate the relation of conclusions, however reached, to the observations on which they are based. Not so for a science of language. Harris recognized and accepted that there is no vantage point outside of language from which to describe language. And, observably, each language contains its own metalanguage. I'll repeat that, because it is I think a key to understanding what Harris was about, and because it is easy to overlook its importance. There is no vantage point outside of language from which to describe language. By contrast, Generativist theory postulates a universal metalanguage, external to language, that is part of one's biological endowment. (I personally do not find this biologicist, neophrenologist doctrine of mental organs credible, but the issue rests not on opinion but on facts yet to be determined.) Harris's stance seems to me perfectly consonant with the argument made by Stephen Anderson in "Why phonology isn't natural." One cannot derive linguistic structure from the findings of some study bearing a metascience relation to linguistics.

Harris was interested in how language can carry or transmit information, and this is the thread that underlies the really remarkable consistency in his work over more than 50 years. Intuitively, we know that differences in form correlate with differences in meaning, but the correlation is messy and inconsistent in the observed data of language (say, in a body of writings or of phonemic transcriptions, including whatever utterances the investigator may come up with in the ad hoc search for examples). What Harris found was how this messy, inconsistent stream of words can be the product of two concurrent systems: a system of word dependencies that correlates with perceptions in a subject-matter domain such as a science subfield, and a system of reductions that changes word shapes (often to zero), motivated in part by issues of redundancy and efficiency and in part by historically

contingent social convention. The reductions introduce degeneracies such as ambiguity and paraphrase, and otherwise obscure the correlation of form with meaning, but without destroying that correlation.

Given that structure (differences of form) correlates with meaning, it is of critical importance that the machinery of description not import any structure extraneous to that found in language. Harris's endeavor was always, then, to determine a "least grammar," a description that required an absolute minimum of primitive objects and relations. Any additional objects or relations in the description introduce extrinsic structure that obscures the informational structure in language. This could be the basis for a telling critique of various other theories of language. Harris chose not to make such a critique. When I asked him once about certain aspects of Generativist theory, he would only comment, with evidence of mild amusement, that it did seem to be over-structured.

Like his teacher, Sapir, Harris had an interest in problems of international communication and an international auxiliary language. (A paper on this appeared in a 1962 volume on avoiding World War III. Remember WWIII, everyone?) And like Sapir and Bloomfield he had in particular a long standing interest in international cooperation and communication in science. This culminated in _The Form of Information in Science: analysis of an immunology sublanguage_ (with Michael Gottfried, Tom Ryckman, and others, 1989, John Benjamins). This book describes the grammar of the sublanguage of immunology during a specific period in the development of that field, based on discourse analysis of sublanguage texts from that period and adequate for making explicit the information structures in arbitrary other texts in that sublanguage. The analysis shows how the structure of the sublanguage changed concurrently with a change in immunologists' perceptions in the domain of their science. A difference in informational structure correlates with a difference in meaning. The informational structures that are clearly represented in the binary array resulting from discourse analysis are still present in the actual form of the source texts as written albeit obscured under reductions in word shape, some motivated by considerations of informational efficiency and avoidance of redundancy, some dictated by conventions of language use as a human social institution.

Harris arranged his life so as to enhance the autonomy of his work. I understand that his kibbutz in Israel is a wealthy one, to which members give their assets and income, and which in turn supports them in their needs. I believe that the kibbutz purchased his apartment building on Charles Street. Until his retirement, he held an endowed chair at Penn, the Benjamin Franklin Professorship in linguistics. He was Principal Investigator for a long series of grants from the NSF, NIMH, and other agencies whose committees and referees found his work of continuing value. Throughout his life he was involved with scientists and science. His wife was a physicist at the University of Jerusalem, and had been Albert Einstein's assistant at Princeton. A brother was an immunologist (he is an author of some of the work analyzed in the 1989 book). He felt that the rough and tumble of polemic attack and retort was inappropriate for science, and would not participate in it. That too would be a distraction from the work.

After one of the Bampton Lectures at Columbia in 1986, a young member of

the audience approached him and asked what he would take up if he had another lifetime before him. He mentioned poetry, especially the longer works of 19th century poets like Browning. He mentioned music. And he mentioned sign language.

He had a long and very productive life. He had brought his life's work to a successful culmination. With the completion of his book on politics, I imagine Death coming to him, as to the chess playing knight in The Seventh Seal, and him saying "OK, I'm ready now."

It was a privilege to know him and to learn from him. He is an abiding inspiration.

Bruce Nevin bn@bbn.com

Date: Fri May 29, 1992 3:35 pm PST
Subject: Misc replies

[From Rick Marken (920529 14:00)]

Tom Bourbon [052792 -- 0:46] says

> I need copies of books and computer demonstrations on PCT.

Well, how about my "Mind readings" book. Since you're the one who gets to go to France, though, you'll have to buy a copy yourself.

By the way, thanks for the copies of the reviews of the "Models and their worlds" paper. Well, maybe "thanks" is the wrong word. But isn't it fun to see the strange things that come from the "minds" of the establishment? I'm glad to see that you are well on your way to a nice thick file of bad reviews. The two I saw are really suitable for framing.

Avery Andrews says:

>When I get back home, I'm contemplating programing the following little
>Reorganization project, whose purpose is to illustrate Reorganization in
>the simplest possible setting (to see if I really understand how
>it's supposed to work). Comments welcome.

I think the simplest demo of reorganization was already alluded to by Bill Powers in a comment to Chris Love. This is the "random walk chemotaxis" demo which is described in two papers in chapter 4 of my "Mind readings" book (available for ONLY $18.00 from CSG Publishing, 460 Black Lick Rd., Gravel Switch, KY 40328 USA -- hint, hint).

>Astro is supposed to learn to go to a point in free space and stay there.

Sounds like what the e. coli model does. It really might be worth it to think about writing the e.coli simulation first. You "astro" project may end up being more complex than you think.

>Distance from the goal-point will constitute intrinsic error,

You mean distance is the intrinsic variable that is controlled relative to a fixed intrinsic reference? Intrinsic error must be a difference between

variables in the system, not in the environment.

>What astro has to figure out is how to vary the velocity reference
>level w.r.t. the distance from the goal point so as not to overshoot,
>which it will do by altering the parameters of a distance -> velocity
>ref. level function.

This should be an interesting and instructive exercise, Avery. I am impressed that you want to try it. Modeling is the best way to learn control theory. I just hope that, when you are successful, you don't expect me to go out and learn linguistics. That stuff is too COMPLICATED.

Ed Ford (920525:14:20) says:

>I think that directing a person's awareness to levels (or areas) which
>aren't broken can be very productive. Obviously, if people are doing
>well in one or more areas, but their belief-in-self system isn't, then
>having them reflect on what they're doing well can be most helpful in
>rebuilding confidence. It is best to build from strength, not
>weakness.

I guess I didn't make myself clear. Based on subjective experience (not the PCT model) it seems to me that skill breaks down somewhat when you direct your attention (consciousness) to the means being used to produce a particular result WHILE YOU ARE DOING IT. This is easy to demonstrate; while you are typing, think about HOW you are doing it; how you are moving your fingers, how you are adjusting and coordinating movements of the fingers, etc. You start making mistakes (more of them, anyway) when your awareness moves to these levels of control; control seems to work better when it occurs unconsciously -- zen control. But there is no problem when you IMAGINE typing and become conscious, in IMAGINATION, of how you do things. In fact, certain kinds of conscious imagining is reputed to improve control when you get down to actually controlling. I remember Dwight Stones imagining, over and over again, the details of a high jump event just before executing it. I guess his hope was that once he'd imagined it enough he could just go and DO it (control it) unconsciously. The trick is to be able to change easily from conscious imagining to unconscious doing. Sometimes it works; sometimes it doesn't. Whether imagining itself can actually make thinks better -- I don't know. Maybe that's why we dream -- but that is usually unconscious (I think?).

I have a feeling that consciously focusing on what one does right (in imagination mode, of course) may make one feel better but does not necessarily help a person in others areas. For example, I can't see how focusing consciousness on, say, one's ability to throw a football, can help with ones ability to sell cars. I agree that it might help a person control self confidence a bit ("yeah, you can't sell a car to your mother -- but you can throw the football pretty well"). But that's just making things better in imaginaiton mode anyway. When the fella gets back to the car lot his confidence goes right back to hell. I think its better to just get down to the business of helping a person "move up a level" so that he can see that he is creating his own problems. Of course, it might help him spend the 50 minutes in a session if he feels good about you and himself. So, what they hey.

Best regards Rick

Date: Fri May 29, 1992 2:45 pm PST
Subject: Marken book

[Gary Cziko 920520.1655]

Rick Marken (920529 14:00) in his usual self-serving manner says to Tom Bourbon:

>Well, how about my "Mind readings" book. Since you're the one who
>gets to go to France, though, you'll have to buy a copy yourself.

And later to Avery Andrews:

>This is the "random walk chemotaxis" demo
>which is described in two papers in chapter 4 of my "Mind readings" book
>(available for ONLY $18.00 from CSG Publishing, 460 Black Lick Rd., Gravel
>Switch, KY 40328 USA -- hint, hint).

I have purchased TWO copies of Rick's book and I recommend it highly. I had seen most of the papers before, but it is very handy to have them all collected in one volume.

I particularly like the preface to the book. Here Rick writes a very nice essay on PCT which introduces the themes of his papers. I suggest that Rick post this preface to CSGnet along with ordering information for the book. If Rick is too shy(?) to do this, perhaps Greg could.

--Gary

Date: Fri May 29, 1992 6:20 pm PST
Subject: PCT & Corrections

[From Rick Marken (920529 20:00)]

Ed Ford (920517:12:35) writes:

>The other night I had my meeting of local control theorists who are
>trying to implement these ideas in their jobs.

>Alan and I have been working on the practical applications of PCT to this
>kind of setting [deliqnents] for several years.

[description of PCT inspired treatment program]

>The juvenile is given total control over when he gets out of
>the facility. He has to accomplish certain goals but he alone has
>control over how quickly he can get released.

Well, it's not TOTAL control; a fellow (I presume they are all guys) can't get out 1 second after he gets in -- or two, etc. They can control when they get out -- but there is a lower (and, I bet, upper) bound to how long they can stay in, no matter how they act.

> Although to
>you freedom loving control systems on the net, this might not sound

>like PCT,

I presume that's me. Actually, I only like freedom for people who do not plan to hurt me, my family or anyone else I like (ie. everybody). The program sounds just like PCT to me -- the juveniles are controlling for getting out; you are controlling for the behavior of the kids; trying to make it look "under control". A program cannot be PCT or not; PCT is just a model of behavior. The program you describe is neither good nor bad -- but given that you like it, it is apparently working for you. The kids seem to function in it just fine too. I imagine that the deliquents in this facility have set references for and achieved some results that have hurt other people. I am against people hurting other people and when people do hurt others intentionally (and PCT tells us how to find out if they are doing this intentionally) then I am for preventing that result by any means possible. I am particularly in favor of this kind of intervention if people are doing it because they have become organized in such a way that this kind of hurting is part of the way they are controlling other variables. Thus, I am in favor of doing whatever can be done to stop the activities of grand inquisitors, nazis and KKKers. These people are often perfectly functional and happy control systems that have developed a hierarchical control structure that includes hurting others; they not only think it's not wrong but that it is a positive good. I think some juventile deliquents are this way. There is not much you can do about a happy, functioning control system that wants to hurt others. There is nothing in PCT that says how to "deal" with this situation. My own preference (which is to stop the asshole if possible and have a good defense available) has nothing to do with PCT.

So, if these juveniles are organized so that violoence to others is just part of their organization, then I don't care what you do to them; just keep them out of my society.

If, however, the behavior that got these kids into the facility is the result of reorganizations (because the kid has not been able to get in control of his intrinsic variables) or just irrelevent (unintended) side effects of control efforts that could be eliminated by education or counseling, then I think there might be other ways of dealing with the situation.

One point that might be worth noting -- it seems to me that PCT should at least make one sensitive to the possibility that one's efforts to HELP another control system are really an attempt to perceive that system's behavior to be "as we like it". After all, WE are control system's too, no?

Best regards, Rick

Date: Sat May 30, 1992 5:17 am PST
From: Hortideas Publishing / MCI ID: 497-2767

TO: * Dag Forssell / MCI ID: 474-2580
Subject: Thanks and...

From Greg Williams (920530 - direct)

Thanks for sending "The Deming Management Philosophy" and the Ackoff seminar with your updates (which look good to me). Your area of the CSG archive is prominent!

In answer to your query about a list of Bill Powers' posts -- I'm sorry to say that I haven't compiled such a list. If and when you do, I (and some other netters, I am sure) would greatly appreciate a copy!

Regarding your comments on my post about the PL materials I went through -- I like your "technical masterpieces," and what I was trying to get at about the behavior/action distinction is that behavior is the key for prediction, rather than action. It takes a "leap" to abstraction (as even Skinner recognized with his "operant" abstraction), from specific actions to a set of actions which result in the same behavior, for good predictions to be possible SOMETIMES (i.e., if reference signals of the subject are changing!). That leap isn't always very easy!

I think you are doing an excellent job.

Greg

Date: Sat May 30, 1992 7:35 am PST
From: William T. Powers
EMS: INTERNET / MCI ID: 376-5414
MBX: POWERS_W%FLC@vaxf.colorado.edu

TO: * Dag Forssell / MCI ID: 474-2580
Subject: Your mailing

May 30, 1992

Dear Dag,

Got your latest introductory-letter package. I realize that I'm not a CEO and have my own peculiar slant on life, but I still think it sounds like promising me whatever you think I might go for, complete with obligatory buzzwords like "paradigm." You could fill in the blanks with the name of any panacea (operant conditioning, crystal gazing, reading the Bible, assertiveness training, control theory, a cavity-fighting toothpaste) and most of it would sound the same (just change a few words to fit the context). Somehow you have to break out of the "salesman" mode and get into some other mode -- a salesman will say whatever is required to sell the product, and everyone knows what the salesman wants: the most profit for the least product.

One of the great myths is that all people can be manipulated by the clever use of words. Some people can, of course, particularly uneducated people, people desperate to get out of miserable, boring, or unhappy ways of life, or people whose own illusions make them prey to any slick stranger who can figure out how to take advantage of them. Somehow, however, I suspect that you will not find many competent CEOs in any of these states of mind -- if anything, their experience is on the selling side. They've heard it all; they'd like to BE the slick stranger, not listen to one. They've heard all the salesman they care to hear saying how wonderful, effective, and magical their products are.

The Deming Management Philosophy and Total Quality Management are today's whiter-and-brighter buzzwords. I have no doubt that behind these words there are many sensible ideas, but most of the package is packaging. If that's the game you want to play, along with every other hotshot charismatic business consultant, then I suppose you'll play it, but I think there's a better way. Well, if you've got that charisma and that smooth way of making common sense

seem like a relevation, then I suppose there isn't a better way of making lots of money. But not many people have it. And it's not a better way to self-respect.

A charismatic lecturer-teacher has a way of sounding as if everything is being said off the cuff in a friendly informal bull-session. Of course that is all very carefully worked out and is the result of years of practice at being sincere, but it doesn't sound carefully worked out and it sure sounds sincere, even passionate. The success of this approach depends on projecting absolute confidence and simplicity, so the listener begins to think "Gee, this is really pretty simple, I could be that way, too." It's an illusion, of course. The simple solutions turn out to be not so simple when you're on your own faced with a situation the lecturer didn't mention.

I don't think that charisma is your style. When you try using a sincere and informal approach, it sounds carefully worked out. You're only sincere and informal when your mind isn't on that, but on what you're teaching. You can't successfully imitate a bullshitter. What you are is a clear-headed engineer who likes to get his ideas into order and lay a firm foundation for understanding and applying them. You have a sure understanding of control theory and you can teach others the same understanding. Your teaching materials, and the classroom methods I can see underlying them, look highly effective, free of nonsense, and oriented toward the goal of having the student master them. Once you can get people into a seminar with the correct understanding of what they are there for, you should experience great success -- your students will go away with real understanding that they can apply even when you're not there. Tney'll be able to teach others.

You problem is that you're trying to get people interested in doing this as if something entirely different were going to happen -- as if the seminars were going to be Barker stuff, revival meetings full of clever anecdotes, inspirational stories about successful people, insider stories about industrial shenanigans and disasters, enthusiasm, clever one-size-fits-all formulas, and shallow insights. Your introductory material tells CEOs that you're going to make their companies more competive, their employees more productive, their managers more able to manage, their customers happy. They're not going to believe you. You might as well tell them their teeth will get whiter, too. They know that you know that you don't know if any such things will result. They know that you hope they will believe that such things will happen, and thus hire you to make them happen by giving three days of seminars. Of course you will not make them happen. They may or may not happen. That's not up to you. And a CEO with any brains knows all this. Are you looking for an audience of CEOs with no brains?

Try this, Dag. Try writing a letter as if you were writing it to me. Try writing it without any attempt to persuade me to believe you or want what you have to offer -- I mean without even WANTING to persuade me. If you like, think of this as a Zen exercise. If it will help, do it in an exaggerated way first, downplaying everything to the point of self-hatred. But then really do it.

Tell me who you are. Tell me about the seminar series you're trying to organize. Tell me why you want to do this for a living. Tell me how you teach, and what you hope the individuals in the classes will come away with (what they will understand when they are through). Tell me why these ideas are understandable and useful TO YOU, not to me. Tell me what makes you think that other people can learn them. And do all this just to inform me, not to

persuade me that you have any answers to my problems. Avoid manipulation altogether: do not ask "what is his error signal?" You don't know what anyone else's error signals are. Forget all about tricks. Just be Dag, your own self, without doubting that just plain Dag is quite good enough. More than good enough. Just tell me. You may be surprised at what you find yourself saying. That would be best of all.

Best Bill P.

Date: Sat May 30, 1992 9:51 am PST
Subject: Mind Readings

[From Rick Marken (920530 11:00)]

Thanks for the comments about the book. I would be happy to post the introduction but I don't have a copy here at home (I think I have it on a disk at work). Maybe Greg could post a copy this weekend if he has it?

I am shamelessly pushing the book because I want the Williams's (Greg and Pat) to get paid back for their great work it -- it's extremely well produced; the profit on each book sold goes directly to them. I don't make anything until it starts to outsell the current Stephen King novel. So those who buy the book can rest assured that they are helping two deserving, hard working people -- not me.

With that disclaimer, here again is the ordering info:

MIND READINGS
EXPERIMENTAL STUDIES OF PURPOSE
Richard S. Marken
x + 212 pp.
ISBN 0-9624154-3-X $18.00

Order from:
CSG Book Publishing, 460 Black Lick Rd., Gravel Switch, KY 40328 U.S.A.

Kentucky residents should add state sales tax. Quantity discounts are available. All prices postpaid, U.S. funds only. ---

I'll be back. Rick

Date: Sat May 30, 1992 12:33 pm PST
Subject: Rick's intro

Introduction to MIND READINGS by Richard S. Marken

The papers collected in this book are the result of a decade of research on the control-theory model of purposeful behavior developed by William T. Powers (1973). I decided to gather them together in a single volume for several reasons. First, I felt that a collection of papers describing experimental tests and demonstrations of control theory would be a useful supplement to existing theoretical (Powers, 1989) and textbook (Robertson and Powers, 1990) treatments of the subject. I also felt that my published research covered a

broad enough range of topics to make a book like this feasible. Finally, and on a personal note, the publication of this collection marks the end of an era in which my research focused largely on what is wrong with current theories of behavior and the beginning of an era in which my research will focus almost exclusively on what is right with control theory.

Purposeful Behavior

I used to think that it is a scientist's job to show what is wrong with one theory before proposing a new one to replace it. But I have learned that things are not so simple with control theory. The problem is that control theory is not really a replacement for any existing theory of behavior. Rather, it is an explanation of a phenomenon that is not even recognized by current theories of behavior -- the phenomenon of control. Control is the process of producing consistent results in the face of unpredictable disturbances. Control can be as simple as keeping your car in its lane on a windy day or as complex as keeping your business profitable in a shifting economy. The phenomenon of control is more commonly known as purpose. The driver has the purpose of keeping the car in its lane; the businessman has the purpose of keeping the business profitable. Control theory is an attempt to explain this kind of pur poseful behavior -- that is, to explain control.

Control theory is a comprehensive, scientifically rigorous, and humanistically satisfying approach to understanding the behavior of organisms. Nevertheless, it has been almost completely ignored in the field that takes the study of behavior as its purview: psychology. I believe this is because many psychologists (and even some control theorists) don't understand the phenomenon that control theory is trying to explain. Thus, it is appropriate to begin a book about control theory with a description of the phenomenon of control. The first chapter of this book provides a formal description of behavior as a process of control. It is argued that a great deal of what psychologists have been calling ``behavior'' is actually purposeful behavior -- in other words, control. The chapter describes objective methods for testing whether or not any behavior involves control. The goal of these tests is to discover the controlled (purposeful) results of an organism's actions. Controlled results are also called controlled variables. A controlled variable should vary, but doesn't; it is kept under control. As a car moves down the road, its position in its lane should vary considerably, but it doesn't. The position of the car is a variable that the driver keeps from varying; it is a controlled variable. Once a controlled variable has been identified, the theory of control can be used to explain how it is controlled, and why.

Mind Reading

Systems that control the results of their actions are called control systems. Thus, all living organisms (and some non-living artifacts, such as thermostats) are control systems. An important (and, perhaps, surprising) characteristic of control systems (living and non-living) is that they control what they perceive, not what they do. This means that it can be difficult for an observer to tell what an organism is ``doing'' just by looking at its behavior (that is, by looking at the results of its actions). The observer must do tests to determine which results are perceived and controlled by the organism. These tests are done by applying disturbances to variables that might be under control and watching for lack of effect. If a variable is under control, then the effect of a disturbance will be cancelled by the organism's actions. This approach to finding out what an organism is doing (that is, what variables it is controlling) is called the ``test for the controlled variable.''

The papers in Chapter 2 describe the test for the controlled variable in detail and show how it can be used to do some thing very much like mind reading. The test is a means for detecting the intended results of an organism's actions. Intentions are in the organism's mind (or brain, if you prefer), so they cannot be directly observed; but they can be inferred by testing for the variables that the organism is controlling. The test for the controlled variable can be viewed as a way to distinguish intended from unintended (accidental) results of an organism's actions. The demonstrations described in Chapter 2 show that behavior, from a control-theory point of view, is a subjective rather than an objective phenomenon. Control theory shows that it is impossible to know what an organism is doing without knowing what it intends to do; that is, without knowing what is going on in its mind. The test for the controlled variable is a reliable, model-based alternative to the ``operational definition'' approach to defining behavior. The operational definition says that behavior is whatever the psychologist says it is; the test for the controlled variable says that behavior is what the organism intends to do.

The Causal Circle

Control theory explains how control systems control perceptual variables. The basic functional organization of a control system is a causal loop; what the system does affects what it senses, and what it senses affects what it does. The behavior of this loop has no beginning and no end; there is a continuous wheel of causality. This creates a problem for psychologists who have been trained to look at behavior as part of a causal sequence (the last part). When confronted with a clear-cut case of control (such as the tracking tasks described in Chapter 3), the psychologist is likely to see the organism's behavior as the end result of a chain of cause and effect. This chain begins with stimulus inputs and ends with behavioral outputs. In a tracking task, for example, where the subject uses a handle to control the distance between a target and a cursor, the target-cursor distance is seen as a stimulus that causes the subject's be havior (handle movements). It looks like control (keeping the cursor on the target) can be produced by this type of cause-effect process. In fact, it cannot, but the cause-effect view is deeply ingrained in the way we think about behavior, making it difficult to imagine that behavior could work any other way.

The papers in Chapter 3 show that control cannot be produced by a cause-effect sequence. The stimulus in a control task is just not smart enough to know how to cause the subject to make exactly those responses that keep a variable under control. The variable that looks like the stimulus in a control task is usually the controlled variable itself. This is true in a tracking task where the distance between target and cursor is under control. In fact, target-cursor distance is both a stimulus (because it influences what the subject does) and a response (because it is influenced by what the subject does) at the same time. When there is a circle of cause and effect, the old straight-line causal view of behavior just doesn't work. Straight-line causality cannot produce control. Only a causal-loop control system can keep a variable in a constant or varying reference state. The reference state is the intended state of the controlled variable. In a tracking task, the reference state is usually defined as ``cursor on target,'' but it could be ``cursor 1 cm to the left of the target,'' or ``cursor 10 cm to the right of the target,'' or ``cursor moving back and forth between each of these two points.'' Whatever the reference state, it is the control system itself, not the environment, that determines its value. It is in this sense that a closed-loop control system is an autonomous agent. A control system controls its own inputs; it is not controlled by those inputs.

Control of Consequences

By ignoring the existence of controlled variables, it is possible to see the behavior of a control system as though it were controlled ``from outside,'' by environmental variables. One way the environment appears to control behavior is through ``selection by consequences.'' This view of control is associated with reinforcement theories of behavior. These theories are based on the observation that certain consequences of behavior (reinforcements) influence the occurrence of the responses that produce them. But responses also influence the occurrence of the consequences themselves. Thus, when a rat presses a lever for food, the food influences the occurrence of the lever press, but the lever press also influences the occurrence of the food. Reinforcement theorists have focused on the influence of consequences on responses and have concluded that reinforcements control behavior. Control theorists, on the other hand, take into account the influence of responses on consequences and show that it is more appropriate to view behavior as the control of reinforcement.

The papers in Chapter 4 show that the effects of consequences on behavior depend on what the subject is trying to do -- that is, what variables the subject is trying to control. The first paper shows that the consequences of responding, even if they are considered reinforcing, do not control behavior. When the consequences of behavior are random, subjects are still able to produce consistent results -- that is, they can control the consequences of their actions. The second paper presents a model of how subjects control consequences, even when these consequences are random. An important feature of the model is its ability to specify a reference state for the conse quences of its actions. The model shows that what constitutes a reinforcement is determined (and can be changed) by the intentions of the organism; reinforcements themselves have no intention to control behavior.

Hierarchical Control

Organisms control variables in order to control other variables. They control muscle tensions in order to control joint angles, they control joint angles in order to control body movements, and so on. There seems to be a hierarchy of control sys tems involved in the production of behavior. This hierarchy is evident when we look at the behavior of organisms. A bird retrieves twigs in order to build a nest; a composer draws little black dots on paper in order to create a symphony. The idea that behavior is organized as a hierarchy is not new, but control theory gives it a new slant by suggesting that behavior involves the control of a hierarchy of inputs, rather than outputs. In the control-theory hierarchy, higher-level systems tell lower-level systems what to perceive, not what to do. These higher-level systems specify the reference states for perceptions that the lower-level systems are trying to control. In other words, the higher-level systems determine the purposes of the lower-level systems. Similarly, the lower-level systems are used to achieve the purposes of the higher-level systems.

The papers in Chapter 5 give examples of hierarchical control and show how a hierarchical control system works. Hierarchical control can be seen in the relative timing of control actions. In a control hierarchy, lower-level systems must operate faster than higher-level systems. Higher-level systems cannot produce an intended result before the lower-level systems have produced the results on which it depends. This nesting of control actions can be seen in the differential speed of operation of control systems at different levels of the control hierarchy. Lower-level systems not only correct for disturbances faster than higher-level ones; they carry out this correction

process during the higher-level correction process. The lower-level control process is temporally nested within the higher-level control process. This nesting is evident in the experi ments described in Chapter 5, where a faster lower-level system controls the distance between a cursor and a target. This system keeps operating as usual even when, due to a change in the relationship between handle and cursor movement, there is an increase in perceptual error. Normal operation is restored only after a slower higher-level system has time to con trol the relationship between handle and cursor movement.

The second paper in Chapter 5 shows how a hierarchy of control systems actually works. A three-level hierarchy with four control systems at each level is simulated in an electronic spreadsheet. The spreadsheet model makes it possible to observe the dynamic behavior of the control systems as they correct for the effects of environmental disturbances and changes in higher-level reference signals. The systems at each level of the hierarchy control a different perceptual aspect of the environment. The spreadsheet model shows that each system in a control hierarchy is able to achieve its own purpose without preventing other systems from achieving theirs at the same time. Nevertheless, conflict between control systems can occur. The model shows that these conflicts result when two or more systems try to control the same (or a similar) perceptual variable. The model also shows that conflict is the most debilitating thing that can happen to a control hierarchy, short of physical destruction of its component parts. Conflict makes it impossible for higher-level systems to achieve their purposes. The model provides an understanding of the nature of conflict that should be of interest to theorists and clinicians.

Coordination

The hierarchical control model provides an elegant solution to one of the most difficult problems for theories of behavior: the problem of how an organism coordinates all the actions required to produce an intended result. For example, how does an organism coordinate the temporal pattern of movements of its arms and legs in order to produce the result called ``walking''? Some theories assume that coordination is the result of complex computations that command just the right actions at just the right time. Others assume that coordination is the result of complex physical constraints on how the organism can move. But none of these theories explain how coordination is possible in a constantly changing, disturbance-prone environment. The hierarchical control model solves this problem by controlling the perceived consequences of its actions, rather than the actions themselves.

The papers in Chapter 6 show how control systems can generate precisely coordinated actions so as to produce consistent behavioral results. This coordination is achieved in the face of unpredictable and undetectable disturbances, just as it is by real organisms in the real world. The coordination problem is solved automatically by the disturbance resistance that is characteristic of the individual control systems. As control systems act to control their own inputs, they often disturb inputs controlled by other control systems. The actions of these latter systems appear to be coordinated with those of the former simply because the actions of any control system compensate for the net effect of disturbances to the variable that it is controlling. It is not necessary to make detailed computations of outputs in order to produce coordinated behavior. In a control hierarchy, it is only necessary to compute the discrepancy between the in tended and actual consequences of those outputs. Coordination flows naturally from the process of controlling perception.

Applications

Finally, the proponent of any scientific theory likes to think that the theory might be of some value to humanity. A good theory helps us understand some aspect of reality (control theory helps us understand the reality of purposeful behavior), but it also helps us solve some of the problems of dealing with that reality. In the final chapter, I make some suggestions about how control theory might help us solve some of the problems of dealing with the highly technological environment that has grown up around us in the last few decades. These problems are the concern of people in my own profession -- human factors engineering. The human factors engineer tries to design technologies that can be used easily, safely, and productively. In order to do this, the engineer must know something about how people use these technologies to achieve their own purposes. In other words, the engineer must know something about how people control. The final paper in this book shows how control theory can help engineers design a more ``user friendly'' environment, one in which human control systems are best able to do what they must do -- control.

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Powers, W. T. (1973) Behavior: The control of perception. Chicago: Aldine.

Powers, W. T. (1989) Living control systems. Gravel Switch, KY: The Control Systems Group.

Robertson, R. J., and W. T. Powers, eds. (1990) Introduction to modern psychology: The control-theory view. Gravel Switch, KY: The Control Systems Group.

Date: Sat May 30, 1992 3:56 pm PST
Subject: changing the name;therapy;Mary's request re travel

[From Dick Robertson]
(To gsgnet in general) I've been amused and bemused at the way the term for referring to our mutual interest keeps growing over time. First it was just CT, then it got to be PCT, and lately I notice, it seems to have become HPCT. Now suppose we moved on again, to Hierarchically Emerging Perception-Controlling Action Theory? Dig that acronym, as we old timers used to say.

(David Goldstein on group therapy post)

>I would appreciate some comments on how to apply HPCT to a group therapy
>situation with adolescents. I am taking a look at how the groups are run...
>The residents are sometimes stirred up by the group discussion and act out
>afterwords. The residential living staff complain of having to deal with this.
>The residential living staff participate in the group therapy and the clinical
>staff feel powerless to influence how they participate. The worthwhileness of
>the group therapy is being called into question.

Then you give nice analysis of the goals that you believe can be achieved by group therapy. However, you kind of mix together the colloquial statements and the CT (or PCT, OR HPCT OR HEPCAT) expressions. I wonder if it would add any- thing to the analysis if you followed the scheme that Bill used in his

book & the textbook for translating commonsense experiences into analyses of controlled variables, remember: (let me have some liberties with the order)

Behavior (informal word or phrase)	Action	Variable	Reference
Learn about self from the group	Receive (solicit?) comments	Descriptions made of S by others	Aha, I can use that
Learn to relate w/o control'g	Observing attempts at control	"should statements"	Other -> S = 0 S -> O = 0
Improve Communi-cation skills	State intentions Hear intentions Infer intentions	"I want..."s- "You want..."s- "I'm guessing you want-statements	OK, you got it Hey, you hear me That's right
Learn emotional supportiveness	Make "you're OK" statements & Hear the same w/o ease or pleasure	Complimenting & encouraging statements	Feelings of pleasure

Well, this is pretty crude, just what I batted out in a few minutes, but I think you can see that by separating your everyday-language formulations from PCT formulations it can help to zero-in a little faster on the controlled variables in the situation. Then you ask

>how should the meeting be organized to reach these goals?

and you follow up with some mighty sensible suggestions. Again I would suggest that you might find it useful to try and organize them in a hierarchical fashion AND I'd suggest separting what you perceive (infer) as the resident's* goals, the treatment staff's* goals, the residential living staff's* goals and your own known goals. I put an (*) behind each of those group names because, as I'm sure you're aware, those groups consist of individuals whose personal goals may be so much at odds that no coherent action from that group is possible.

HOWEVER, there is one group that I think should be treated as a quasi-control system. That is the group you started with: the group comprising each group therapy meeting. Over the years I have found it very useful to stop watching individuals for a while in group meetings and try to observe the group as a unit, find out how its sense of "being a group" is defined by the members (that is, what common denominator do the majority of them, at least, seem to agree on), what common denominators do they seem to share as to what values or attitudes should be implemented to maintain their sense of who they are and what they stand for, and what do they have to do implement those values {you get the idea, I'm sure}.

When you start looking at things that way you (I, at least) begin noticing where a group goal if implemented could help individuals realize their individual goals. Here's where some of Ed's descriptions of his work with groups comes in so beautifully, if the residents want to become ex-residents, it should be useful to suggest they share their lore as to how that's accomplished. And then where does the work of the residential living staff come in? What variables are they trying to control? We presume they are controlling things that they perceive as having consequences like, praise from

the boss, thanks from graduates, raises in pay, etc. What can the residents, and the treatment staff, offer to help, what can they offer in turn? Well, you know how to do all of that, in principle, anyway. So I'm wondering about your statement that

>the groups are often chaotic<

I wonder if that means that reorganization gets triggered for some people in the meetings and they don't understand about how anxiety often goes with reorg. so they panic, and you're seeing that distress. Or, might it be that members of the different subgroups perceive their personal self-interests as inimical to those of the others, in which case maybe the staffs need to have their therapy meetings before being turned loose on the residents??

Anyway, David, are you interested in giving one of your excellent case descriptions of a segment of group interactions for the rest of us netters - who like to keep trying to understand higher order system problems better - to have a look at? I'd be interested for one.

(Mary Powers) You asked about people who would be leaving the conf. on Sunday afternoon. I guess that includes me. I've got my travel agency working on tickets for me, and I told them to bring me back Mon am if Sunday is out of the question.

Date: Sat May 30, 1992 9:41 pm PST
Subject: France; Address change

From Tom Bourbon [920531 -- 0:15]
Thanks, to those of you who offered material that I can take to CACS92. Special thanks to Gary Cziko, who offered himself! (Gary, my presentation will be the morning of 7 July. For complete details, you can contact Jean-Arcady Meyer at meyer@frulm63.bitnet. All sessions are plenary, at the Ecole d'Art, in Aix en Provence. Beyond that, I do not know the details.)
****
My participation on CSG-L has been limited in recent months, while I tried to conclude the academic year, present at several meetings, and prepare for an impending move. The move is at hand. This is my last post from Nacogdoches, Texas. I will log on from Galveston, Texas, as soon as I learn how to use their system. My addresses and phone numbers will be:

MEG Laboratory
Transitional Learning Community
Galveston, Texas 77553
(409) 763-6325
I will most often be at that address and phone number. Other times, I will be at:
Division of Neurosurgery, E-17
University of Texas Medical Branch
Galveston, Texas 77550
Phone: (409) 772-1227
FAX: (409) 772-6352
That is the place to send a FAX.

My temporary address for e-mail (after I activate the dormant account) will be: PAPANICOLAOU@UTMBEACH.BITNET
Soon, I will establish an account in my name.

Tom Bourbon

Date: Sun May 31, 1992 6:42 am PST
Subject: Re: abstract,concrete,HPCT

I see I forgot to put the salutation on the letter I copied to CSGnet. It was to David Chapman (and Philip Agre) at the Artificial Intelligence Laboratory at MIT. Avery Andrews sent me their paper, "Abstract reasoning as emergent from concrete activity," Proc. 1986 Workshpop Timberline, OR, "Reasoning about Actions & Plans." Penni Sibun's name was on the copy -- maybe she can say how to get copies or where to find the publication.

Bill Powers

Date: Sun May 31, 1992 6:54 am PST
Subject: Abstract, concrete, HPCT

[From Bill Powers (920531.0700)]
Copy to CSGnet.

Greetings from CSGnet. My name is Bill Powers. I have just received a copy of your delightful paper (with Agre), "Abstract reasoning as emergent from concrete activity," from my nephew Avery Andrews, who is a linguist residing in Australia. How is "Agre" pronounced? Is it "ah-gruh" or "aeger?" Or something I haven't guessed? I think I can handle "Chapman."

There are some points of contact between your ideas and the basic model that's behind CSGnet (a Bitnet-internet list). The CSG stands for "control systems group", which is a collection of people (including many off the net) from many disciplines who have taken up some ideas I developed (with Clark and McFarland) in the 1950s, and have been working on since then.

There are three aspects of this theoretical framework.

One, called CT, or control theory, is just the basic body of theory developed by control-system engineers in the 1930s and 40s to describe and predict the behavior of closed-loop negative feedback systems -- servomechanisms, regulators, and such.

The second is PCT, or perceptual control theory, which is the adaptation of CT to the universe of organismic behavior (starting with Wiener, Rosenbleuth, and Bigelow but branching off quite early from cybernetics). The basic idea behind PCT is that living control systems act to bring perceptual representations of external variables into a match with internally-specified reference signals, maintaining them in a near match despite changes in the reference signals and occurrance of external disturbances tending to alter the perceptions. "Perception" is used in a generic sense to mean all experiences from raw sensory input to abstract representations. We talk about PCT when we mean to indicate only that some perception is under control by behavior, the kind of perception being secondary.

The third aspect is HPCT, meaning hierarchical perceptual control theory. This is not really control theory per se, but an attempt to introduce facts of experience and some neurological facts into the general model, to make it specific to human experience and human architecture. I'm going to bore you with a rather detailed description of this hierarchy, because unless you understand it you won't see how it relates to your work.

The concept behind HPCT is a hierarchy that runs in two directions: a perceptual hierarchy building upward, and a control hierarchy building downward. A given level (containing many control systems) receives inputs that are copies of perceptual signals of lower order, some under direct control and some uncontrolled. A perceptual function in a specific control system generates a new signal that represents a variable of a new type, derived from lower-level perceptions (or sensors, of course, at the lowest level). A comparator compares the state of this signal with a reference signal received from systems of a higher level. The error signal resulting from the comparison goes to an output function that ends up distributing reference signals to control systems of the next lower level -- the same level where the perceptual signals originated. Only the lowest level of outputs generates muscle action.

So each level of system acts to match its own perceptual signals to reference signals received from higher levels, and acts by means of varying reference signals for systems at the next lower level. The result is a hierarchy of goal-seeking and goal-maintaining control systems with many systems at each level and many levels. The highest level of reference signals has to be handled in a special way, of course, which I won't get into here.

The first level of perception is called the "intensity" level. The perceptual signals at this level are generated by sensory nerve endings (a perceptual signal is measured in impulses per second -- individual spikes have no significance in this theory). Each first-order perceptual signal therefore represents the intensity of stimulation in one sensory ending. As neural signals vary only in magnitude (carried as a frequency), they are one-dimensional: they represent how much stimulation is present, but not what kind. So the first level of perception is a collection of millions of signals representing pure magnitudes: essentially, positive numbers. This first level of perception contains all possible information about an external world, as far as the brain is concerned (meaning, of course, as far as we are concerned). Some first-level perceptions are under direct control: primarily, those representing muscle stretch and tension. We experience these as "efforts."

Second-level perceptions are functions of sets of first-level perceptions. The functions are probably weighted sums. The signals that result are called "sensations," which are vectors in little subspaces made of a few independently variable intensity signals. Taste, for example, seems to be a function of only four kinds of intensity signals. Color seems to be a function of three kinds. Second-level sensations are controllable by varying reference signals for those first-level perceptions that are under control: muscle tensions. Most second-level sensations are not under control. There are probably uncontrolled perceptions at every level, although fewer at the higher levels.

Sensation-signals, just like intensity signals, can vary only in magnitude: one signal can represent only how much of the particular sensation is present, not what kind it is. The "kind" is determined by the weightings applied to the intensity inputs in the perceptual function. So this is a pandaemonium model:

one control system controls only one kind of perception, and controls it strictly with respect to its magnitude. This holds true at all levels.

Third-level perceptions, functions of sets of sensation-signals, are called "configuration" signals. I don't know the nature of these functions, or of any perceptual functions from here on up. At this level, the world of objects and static patterns comes into being. But there are also sound configurations (phomemes, chords), tactile configurations (a squeeze), and somatic configurations (internal feelings like nausea) -- all sensory modalities are involved. A given configuration signal has a magnitude that indicates the degree to which a given kind of configuration is present. One signal can represent only one kind of configuration.

This is the perceptual world that we think of as consisting of "concrete objects." You see where I'm going -- this is one of the lowest levels of the same world you refer to as "concrete."

The next level is concerned with something like "transitions," which could mean rates of change (like rate of spin) or partial derivatives and integrals -- paths from one configuration to another. The shapes of paths can be altered smoothly, as can the speed and direction with which paths are traversed. You can traverse a path partway, stop, and reverse to the starting configuration. So the control of transition-perceptions involves at least the dimensions of shape, direction, and speed. The "shape" dimension may simply be an underlying configuration perception.

Next comes "events." An event is a unitary set of transitions, configurations, sensations, and intensities perceived as a space-time package. An example is "jumping." Below the level of events, the underlying perceptions flow smoothly from one state to the next. At the event level we make arbitary divisions of this flow into sections that we perceive and control as a single thing happening.

Above events are "relationships," which are derived from perceptions at the event level on downward. Relationships are things like on, in, beside, before, after, inside, outside, between, and so forth -- not as named, but as perceived. Control of relationships is involved in most behaviors. The means of control is to vary reference signals for events, transitions, configurations, etc.

Above relationships are "categories." This is the first "digital" level: all the levels below are basically analog. At the category level we perceive different things as examples of the same thing: we perceive dogs instead of individual instances of dogs. And at this level, I believe, we begin to symbolize: substitute one representative perception for a class of perceptions. The "representative" perception can be arbitrarily chosen: a representative perception standing for many different configuration signals that look different but are classified as the same might be the configuration of marks that looks like this: "dog." A word is simply a perception used as a symbol for -- used to indicate a category of -- other perceptions, the symbol in this case being a visual configuration perception. Any perception can be used as a symbol for any other perception or class of perceptions. I am not, by the way, very satisfied with the definition of this level, particular the process of naming. There could be a missing level.

The category level, once defined, leads to a re-evaluation of the lower levels: we realize that lower level perceptions, in themselves, are neither

names nor categories. One of the difficulties in parsing experience into levels of organization is that we often apply an inappropriately high level of perception in trying to grasp the nature of a lower level. A configuration perception, for example would not be "a dog." It would be that configuration, directly experienced, that we put into a category with other configurations and refer to with a name, "dog." I think you allude to this problem in your paper.

Above categories we have "sequence," or "ordering." I think this is also what Common Lisp users mean by a "list." It is not the elements of the list; it is the sense of "listness" or ordering itself. It is a perception standing for a set of lower-order perceptions with regard to their sequence of occurrance. It is NOT a "program," because it contains no choice-points. A sequence is like a recipe: break two eggs into a bowl, stir well, add milk, pour in frying pan, add bread, etc., with the final element being called French toast. The elements of this sequence are categories of relationships among events consisting of transitions from one configuration to another, all built out of sensations having variable -- and controlled -- intensities. There is control at each level, but the highest level of control involves assuring that the perceived sequence is of a particular recognizeable kind.

Category-names in sequences become the elements of "programs." A program is a network of choice-points. To perceive a program is to perceive a particular recognizeable network: not any one path through it, but the entire module with all its branches at once. Each element in the network can be anything from a sequence, a list, on down. This is the main level, I think, where "abstract reasoning" takes place (although of course the elements with which reasoning deals are sequences of symbols for categories of ...).

Above this level (!), I believe, is a level at which we perceive "principles." Other words might be "generalizations" or "heuristics." These are things that human beings have no trouble recognizing and controlling for, but which we have as yet not succeeded in getting hardware to do. We can generate programs that are EXAMPLES of principles (successive approximation, for example, which you mention), but those programs are not the principles. Similarly, our names for principles are really names for lower-level situations that constitute instances of principles, as a particular set of sensations is an instance of a configuration, with other sets of sensations being instances of the SAME configuration.

And finally, at the top (as far as I know now) we find "system concepts." These are things like "physics" and "government" and "AI" and "self." They are entities perceived as functions of sets of principles etc. The system concepts for which we control determine what happens at all lower levels -- in general, although not, of course, in detail.

These levels were defined on the basis of subjective experience, but also meet some communicable criteria for a hierarchical control relationship. A perception at any level, if analyzed into elements other than smaller perceptions of the same type, proves to consist of sets of perceptions of the next lower level and of a different type. This is a subjective call, of course, and my analysis might not exactly match someone else's. But so far there seems to be pretty good agreement with others who have looked critically at the same aspects of experience. I expect all the definitions to change, eventually, as we explore them experimentally.

The other criterion is that in order to control a perception of any given level (act to bring it to a specific state), it is necessary to VARY the target-states of lower-level perceptions. To alter the visual configuration we call (at the category level) "squareness" to make it a little less square, we must alter the sensations that constitute its sides and corners. CONTROLLING any given level of perception requires VARYING lower level perceptions.

I think that my definitions of levels meet these criteria. The only way to check this out, of course, is to look for yourself.

You have probably noticed that in this hierarchy of perceptions, the entire world of experience, everything from the most concrete stimulus intensity to the most abstract system concept, appears as a perception in the brain. The "outside world" doesn't come into it at all. When you lean your bicycle against the wall, you're controlling one configuration perception to bring it into a specific perceived -- but not necessarily named -- relationship with another configuration perception. When you worry about how to lock the door without letting the bike fall and spill groceries everywhere, you're sorting through sequence perceptions, trying to find one that will work (in imagination, a subject we'll skip but that is in the model). And the sorting is done in terms of the NAMES of CATEGORIES of lower-level perceptions, these names becoming symbols that are handled by some sort of logic, under control of principles such as "don't blow it."

What's going on in the outside world while you're controlling all these levels of perceptions is a good question. I think it can be answered only in terms of models of possible realities. What we experience consists of neural signals.

Well, in a very small nutshell, that's HPCT. I haven't talked about the logic of control, or the kinds of experiments one does to establish what in fact is being controlled with respect to what reference state, but perhaps this is enough to tell you that we may have some common interests. I've probably given the impression that the theory is much better developed than it really is, particularly at the higher levels. But in Big Picture terms, perhaps you get the point. In a phrase that I'm trying to discourage the use of, because it's turning into a slogan, it's all perception (and control of perception).

Control theory says that control systems VARY their actions in order to CONTROL their inputs. Not their outputs. What others see as controlled output -- as behavior -- is really just an indirect effect of controlling perceptions. Another way to say this is that control systems control OUTCOMES rather than MEANS. This is why some of your buddies at MIT are on the wrong track: they're trying to build models of motor behavior that specify outputs, where the real system works by specifying inputs. They're forgetting that between muscle tensions and their final effects are many other unpredictable influences that also contribute to the outcome. Regular outcomes can be produced only if they are sensed, and if control is centered on matching what is sensed to some reference state. To produce the same outcome twice, in the real environment, you must NOT produce the same outPUT twice.

As you can guess, HPCT has a lot to say about AI. And a lot to learn from it.

If you want to look in on our list, the listserver is at listserv@vmd.cso.uiuc.edu (U. of Illinois) Send the message (to the above address, not to me, as you no doubt know) SUBSCRIBE CSG-L lastname, firstname, location It's an open forum, and pretty active (a megabyte per month, sometimes). You might find any subject at all being discussed, but all

in terms of control theory. Don't hesitate to start a new thread -- or to just listen if that's your preference.

I think you may find HPCT a great tool for saying all those good things you have to say.

Best, Bill Powers

Date: Sun May 31, 1992 6:14 pm PST
Subject: logical gain

[From Rick Marken (920531)]

Here are some thoughts motivated by an article on the front page of the Sunday times announcing the development of a new lightbulb that will last MUCH longer and consume less energy while putting out the same amount of light. The improvement is based on logic -- the light is produced by "signal processing" rather than heating.

This led me to think about loop gain in control systems. I am thinking of loop gain as all the multiplications that occur as effects propagate around the control loop. There are really only two multiplications -- k.o, the organism function which amplifies input (and is in units of input/output) and k.f, the feedback function, which amplifies (or reduces) output (and is in units of output/input). Loop gain, K, is the product k.o*k.f. K is thus a dimensionless quantity. The bigger K is, the better control is.

It appears to me that the same level of loop gain (K) can be achieved in two ways -- by increasing k.o or k.f. Increasing k.o requires putting energy into the c
ontrol system (by eating, e tc) and this can be done in only one way -- because the system can only get "strong" in one way. Increasing k.f can be done in many ways -- that's where the new light bulb comes in. It seems to me that one of the advantages of having higher level control systems is that they can figure out clever ways to make k.f more efficient (and have fewer undesireable side effects). The new light bulb let's me control the illumination level in the room with far less energy input than was required for the old light bulb. k.o is still the same (I still have to exert the same amount of energy to turn the rheostat in order to make the lights brighter or dimmer). The loop gain for controlling illumination is still the same -- but the amount of energy required to transform my output (turning the knob) into my input (perceived brightness) is much less. This is not really an increase in k.f to offset an decrease in k.o; it's a decrease in the energy required to produce k.f. There are, however, many examples of higher level control systems (at the principle level, at least) contributing to an increase in k.f that increases loop gain dramatically with no change in k.o; levers and pulleys, for example.

So it seems that higher level systems can serve to increase the loop gain of lower level systems without necessarily changing the physical construction of the control system itself. This might be a way of looking at the evolutionary advantage of having higher level control systems; its a way to increase loop gain without too much structural change in the organism. That is, higher level systems let you increase K by increasing k.f rather than k.o.

Or maybe not. Just some Sunday musings; now back to the hammock.

Best regards Rick

Date: Sun May 31, 1992 9:12 pm PST
Subject: Re: abstract,concrete,HPCT

>I see I forgot to put the salutation on the letter I copied to CSGnet. >It was to David Chapman (and Philip Agre) at the Artificial Intelligence >Laboratory at MIT. Avery Andrews sent me their paper, "Abstract >reasoning as emergent from concrete activity," Proc. 1986 Workshpop >Timberline, OR, "Reasoning about Actions & Plans." Penni Sibun's name >was on the copy -- maybe she can say how to get copies or where to find >the publication.

Bill Powers

I got the paper from one of the authors. The proceedings are published, but i imagine they're hard to find. There is an easier-to-find paper of the period, viz.,

Agre, P. and D. Chapman (1987), ``Pengi: An Implementation of a Theory of Activity.'' {\em Proceedings of the Sixth National Conference on Artificial Intelligence}, Seattle, pp~268-272.

The two papers should really be read together; i don't think the Pengi paper makes any sense at all w/o some sort of background (it's extremely short and dense). Since the abstract/emergent paper is hard to find, I'd be happy to send the pair to anyone that's interested.

FYI, Chapman is at teleos research in palo alto (zvona@sail.stanford.edu) and agre is at ucsd (pagre@weber.ucsd.edu). they did this stuff as gradstuds at mit; they've moved on somewhat to other things, but pengi remains their most important (imho) and certainly most famous work. W/ pengi, they managed to change the course of a subfield of artificial intelligence (planning). Very briefly, AI planning is the part of AI concerned w/ the issue of how a system decides what to do and does it. The paradigm used to be that an agent selects a goal, builds a plan out of actions to achieve the goal, and then executes the actions as specified by the plan. It was generally assumed both that all action happened this way and that the possibility that the world might change in important ways before the plan was done was irrelevant. A&C managed to question these notions forcefully enough that AI planning weenies have to at least pay lip service to concepts of a fast-changing world and actions taken w/o deliberation. The deeper points of the work--such that an agent and its world are mutually constructed and that it follows from this mutual construction that the world constrains the choices an agent has at any point, so that most of the time deciding what to do just isn't a big deal (agre calls this ``leaning on the world'')--have been
largely missed.

--penni