CSG_9110

Date: Tue Oct 01, 1991 10:14 am PST
From: POWERS DENISON C
Subject: comments on Judd's paper

[From bill powers (911001.0745)]

Joel Judd (910930) --

Your foa paugh is phorgiven. I like the beginning too. Some notes.
If the goal is "keep the knot over the mark", there are several ways to achieve it:

1. Drive a nail through the knot into the mark (no control needed).
2. Shoot a curare arrow into the other person so he/she will stop disturbing the knot.
3. Employ a visual-motor control system and give it the reference-image or goal that we describe as "knot over mark."

Just quibbling.

>This signal is fed into a comparator where it is compared with ...

You have scratched a small itch for me. Yes, electronic types have long spoken about "feeding" a signal into a circuit. Describing a closed-loop system, it would be natural, in those terms, to say that some of the output is fed back into the input part of the circuit. By a well-known mechanism with which linguists are familiar, the verb "to feed back" is then collapsed into a noun: feedback. Thank you.

Your explanation of the "uselessness quotient" could include Gary Cziko's way of putting it: if you just said that everybody behaved according to the mean of the group, you would come 82% as close to predicting what an individual did.

Bob Yates (910929) --

In your challenge to Joel Judd concerning regularities in SLA, what was the nature of the data you cited showing that there are universal stages in this process? Does EVERY SINGLE SL learner go through the hypothesized stages, or it is simply that a statistically "significant" (if unimportant) number of them do?

Rick Marken (910930) --

Glad you admit to having a curious mind.

Best to all

Bill P.
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Date: Tue Oct 01, 1991 10:58 am PST
From: jbjg7967
Subject: language "universals"

[from Joel Judd]

Now that there's a short break from the paper, I can comment on some of the recent language discussion. The thoughts expressed have been priceless, and it's a good thing too, cause at the moment I couldn't afford to pay for any of them.

Bob Yates sent the following directly to me. I think that a few others might have something to say on the matter, so without his permission I'll post it to everyone:

>If I understand your conclusion correctly, then CT predicts that L2 learning should be dramatically
>different from individual to individual. Each has different goals and perceptions.

>Are you familiar with Long (1990) TESOL Quarterly, 24, 649-699. Long reviews an immense
>amount of research in SLA...the first implication is of interest:

> Common patterns of development in different kinds of learners under diverse conditions of
>exposure means that a theory that says nothing about universals in language and cognition
>is incomplete or, if considered complete, inadequate.

>Can the application of CT to SLA predict common patterns of development across different
>types of learners under different types of exposure?

First, I would say that everyone has different PERCEPTIONS of goals--it is necessary to understand what an individual means by 'learn English.' Teachers and administrators tend to hear something like that, or assume that's what a learner wants because he has paid to attend classes, and go about the teaching business with THEIR perceptions of the goal 'learn English.' Regarding learners themselves, I think that yes, language learning IS dramatically different (depending on what you mean by dramatically) from individual to individual, especially in what each does to develop and maintain reference signals. I also think that the younger one is, less variation is found in part because there is less complexity in the hierarchy (less degrees of freedom?), and the goals of children are more "pure" and shared--they are mainly interested in socio-cultural participation for all aspects of their lives, and language is one of (if not THE) way to participate. Adults, on the other hand, generally do not have such overriding concerns, and so their goals vary in complexity and intensity.

Longs quote prompts several questions. I don't remember now all the studies he references, but I think many if not most of them are the kind I mention in my paper. I don't know of any small-subject SLA study carried out with the type of perspective CT provides. I think the quotes from Taylor (1950) apply to SLAs search for "universals" and "patterns."

"Common patterns of development in different kinds of learners under diverse conditions of exposure..." begs a number of questions. Is he talking about patterns ACROSS or WITHIN learners? If the former, then what do we learn about a single individual? If the latter, Long's wording here leads me to think he's assuming interchangeability of learners; that is, but for "diverse conditions of exposure" and "kind of learner" (variables which presumably can be experimentally controlled and examined) all learners are the same. Runkel (1990) explains how this follows from reliance on methods of relative frequency to understand individuals.
What does he mean by "universals in language and cognition"? Besides Taylor, a well-known language teacher from the middle part of this century wrote the following:

"Sometimes the assumption has been made that certain principles of language learning are so basic that they can be applied universally. If this assumption is sound, then language learning differs markedly from all other sorts of learning. Recent studies of learning have been largely devoted to the isolation and measurement of specific factors which modify and even controvert what were once thought to be general laws. General principles do appear but they are functions of certain conditions. In language learning, then, we would de well, not to proceed deductively from general principles we have assumed, but to induce principles from a detailed study of specific phenomena" (Dunkel 1948: 64).

I have more and more of a problem with viewing "language" as the thing to be acquired, and with corresponding claims that there are "universal ways of doing so. What is "universal" is the development of [hierarchical] perceptual systems, however they turn out to be described. Language just happens to be a crucial kind of perception. Bruce Nevin said something relevant to this last week:

"In learning to use a language a child learns the normal categories of the culture, learns to care about the same kinds of differences as others in the culture do, for the sake of SIMILAR GOALS which the child has learned in the process to maintain internally. The categories have no physical reality other then this internalized economy. This learning of shared categories and GOALS, however, is what constitutes social reality...they [categories] exist "out there" as social realities because to some rubbery, tolerable limit of detail they must be shared by the members of a culture and the speakers of its language else there is no language and there is no culture and human intercourse as we know it comes to a halting crawl"(CSGnet 910923).

"Can the application of CT... predict common patterns...?
Sure, but perhaps not in a way that satisfies one expecting to demonstrate linear relationships among variables of experimental importance. To share socio-cultural goals, one comes to accept them, to learn them as children do. Doing this requires, INCIDENTALLY, learning language. Inasmuch as a group uses the "same" language in the "same" way (as prescribed by linguists), the people within the group will show patterns of development. Describing such patterns, either statistically or by simple observation, NEVER DOES tell us WHY or HOW any one INDIVIDUAL learns the language. This is what I WANT TO KNOW.

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Date: Tue Oct 01, 1991 1:58 pm PST
From: RYATES
Subject: Re: comments on Judd's paper

Bill,

I appreciate your long reply to me earlier in the week. I have to get a handout ready for the same conference where Joel is presenting his paper.

Your questions about developmental sequences is a good one. There is in L2 acquisition research a collection of studies on the acquisition of morphemes. They are seriously flawed for a variety reasons and one of them is directly related to your point about drawing conclusions about individual learners from a group average.

I am more impressed with longitudinal studies on the acquisition of the negation in English which shows that all learners go through similar stages. This work is summarized by Schumann. From longitudinal studies, the acquisition of verb second in German follows a regular pattern for all learners. See Pienemann (1988) in Rutherford and Sharwood Smith _Grammar and Second Language Teaching_. In my own dissertation research I found almost all learners followed a particular pattern of acquisition for the structures I tested. In other words, judgements about structure a was always better than judgement about structure b, which were alwasy better than judgements about structure c, etc. Almost all means 3-4 out of 100+.

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Date: Tue Oct 01, 1991 2:26 pm PST
From: jbjg7967

Subject: same question

[from Joel Judd]

Bill,

Thanks for the help. Never thought about bringing weapons to the show. Probably would correlate negatively with volunteer participation. I thought that the simplicity of the paper (due to time) wouldn't create much disturbance in anyone. At least I can feel I've got the basics down. I think this paper would work up into something publishable, and then I'll probably say something more disturbing.

Bob,

Even finding that every single English speaker acquires negation in the same manner, I still have yet to read a paper that suggests HOW and WHY this might be the case. Certainly noone is saying that it happens that way because it's the LINGUISTIC thing to do? This is where I think SLA might learn a thing or two about acquisition studies from people like Bruner (Child's Talk). There we're getting closer to perceptual development, in which language plays an incidental role. There must some way of describing PLA or SLA in a way that parallels, say, learning to ride a bike. First, someone accepts a goal of 'ride the bike.' Then there comes a number of processes which need to be learned to accomplish the goal. The fact is, everyone learns to grasp the handlebars, use the feet to pedal in a clockwise motion, stay upright, etc. So finally one has learned to ride. Does one then say "Great! I've learned to ride" and leave it there? I don't think so. One goes places on the bike--to school, store, circus. It's not an end in itself. Yet I can watch a thousand people (randomly sampled, of course) learn to ride and document the steps they go through and still know nothing about their purposes. Does this make sense?

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Date: Tue Oct 01, 1991 6:30 pm PST
From: RYATES
Subject: Re: same question

Why people learn to ride a bike is a very interesting question, especially if you have happen to sell bikes.

But the analogy to primary language learning fails in one crucial way: no child decides "I will not learn a language." In fact, you have to literally lock the kid in a closet for 13 years for her not to learn a language. And even if the kid can't hear, he may even invent his own language with agreement morphology. (See Goldin-Meadow and Mylander (1990) _Language_ 323-356. Then, the kid may get input which is very disorganized with frozen forms every-where and regularize it. (See Jenny Singleton's dissertation (1989) done at the U of I.)

People have choice to learn to ride a bike or not; people don't have choice about learning a first language. That is my why question.

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Date: Tue Oct 01, 1991 7:20 pm PST
From: psy delprato
Subject: Carver & Scheier/Relatives?

[FROM: Dennis Delprato]

RE: Rick Marken (910930)

We have two different (and I suggest equally legitimate) perspectives of control theory, as well as of, in the present case, the work of Carver and Scheier. Before going on, should anyone be interested, Carver and Scheier have published in the area of personality and social psycholgy. They have published numerous research papers as well as review and theoretical statements. In doing so, they have covered a wide variety of content/problem areas such as self-regulation, clinical psychology, health psychology, anxiety, depression, intention, and optimism. One of the most recent papers by this prolific pair is "Origins and functions of positive and negative affect: A control process view" (Psychol. Review, 1990, 97, 19-35). In 1981, they turned out a book, "Attention and Self-Regulation: A Control-Theory Approach to Human Behavior" (Springer-Verlag). As Rick Marken previously mentioned, according to their citations, Carver and Scheier take control theory as presented by Bill Powers to be fundamental in their work. At least they did up to the 1990 paper cited above: "We have adopted Power's position as a conceptual heuristic" (p. 20). (They go on to cite Marken here, but I won't mention this.)

So why do I look favorably upon what Carver and Scheier have accomplished? First, let me state what to me is your perspective, Rick, and contrast it with mine. You work from the inside of control theory a la Powers. You know it inside and out (but are humble so as to realize you don't know everything--sure). You are, in fact, a major contributor to PCT and have advanced the field directly in line with its original formulations. And if you are not distracted by extraneous interests, I presume you will continue to participate as an insider in moving PCT forward. Thus, even Carver and Scheier's work, which one can view as itself extending PCT, is not necessarily of relevance to you. Their research problems are different from yours. Their entire professional network has very little overlap with yours.

I will never contribute to PCT in the way you have and will, for my interests and perspective lie outside PCT. The closest I will come to an inside contribution is by stimulating a student to operate the way you do, i.e., take on control systems as a research specialty. I've come to realize that my area, to the extent that I have an identifiable area nowadays, might be described as behavioral metascience with a specialization in the behavior of psychologists. I would neither dignify nor insult my interests by the appelation, philosophy of science, but I suppose there is some overlap between what I do and certain practices in philosophy of science. In the area of psychology, I detect that "philosophical psychology" and "theoretical psychology" are becoming more respectable, and what I do might fall into these areas as well. I try to avoid "schoolish" affiliations. There are some, e.g., who label me an "interbehavioral psychologist." My reply is that I am not a member of this school or cult *if* the label refers to such. I admit to being somewhat knowledgeable about this literature and to finding it has some strong points. But that's it. I find much of value in literatures that the vast majority of "interbehaviorists" wouldn't be caught dead reading and promoting. I sometimes refer to myself as an intellectual voyeur *with scruples,* I have a good idea of what to look for and what to avoid.

What I look for is based on Kantor's historico-critical analysis of psychology (a macro-history no less). This analysis hardly stands alone. It is compatible with other examinations of cultural history and the history of science, including the well-accepted view in cybernetics that something like circular causality represents a fundamental departure in world view from that of unidirectional mechanistic causality. I sketched some of this in the first part of my chapter in W. Hershberger's Volitional Action (1989, North-Holland) as well as elsewhere (e.g., Ray & Delprato, Behavioral Science, 1989, 34, 81-127). I presented two papers on this at the first CSG meeting I attended in 1987 ("Control System Thinking and the Evolution of Scientific Thinking" and "A Sketch of Two Versions of Control"). You may still have copies of these gems somewhere deep in your files.

So, you are coming at all this from the inside as an active first-line researcher and theorist. I am an onlooker who is always thinking in historical and metascientific terms--to the best of my ability. To put it simply, I consider the work of Carver and Scheier progressive and ahead of the vast majority of their peers because they *do* behave in accord with general PCT thinking that, in turn, is compatible with the most-recently evolved approach to describing the world, i.e., in terms of integrated fields or systems. From my perspective of the big picture, I see so many behind Carver and Scheier that I cannot help but note their progressiveness. If one were to lay out the fundamental postulates of PCT and examine those of Carver and Scheier against these, I believe there would be sufficient overlap to conclude that we were dealing with close relatives here. By the way, this would make an interesting paper--any takers? I am not convinced that Carver and Scheier's work, from the standpoint of control theory, "are based on the wrong [fundamentally different] assumptions."

Rick, your perspective leads you to ask if Carver and Scheier explicitly test the control model of behavior. You would like to see more work of this specific sort. I think it is safe to say that they have not come to generative modeling. But they are helping to bridge an enormous gap between mainstream behavioral science based on a nineteenth-century framework of science and a behavioral science of the future based on integrated-fields, systems, cybernetic control, et al. That is my perspective. What do I recommend regarding your relationship to Carver and Scheier's work? (1) Only concern yourself with it if you decide to address research problems that they have worked with or it you aspire to write a general history of control theory as applied to behavioral systems. (2) Do not place their work in a negative light by dwelling on its shortcomings from your perspective; one consequence of this is that it could contribute to someone avoiding control theory who might otherwise have been first attracted in its direction by Carver and Scheier publications. (3) If you are so inclined, make constructive recommendations on how they could expand into generative modeling and testing of control theory.

I suppose the main thrust of my previous post was that you seemed to be writing off Carver and Scheier's work because of its moldy and hapless character. To me, it is unfortunate when we reject ideas because they are not *perfectly* compatible with ours. I prefer integrationism but with a definite bias as noted above (i.e., not eclecticism). There is a great deal of junk out there. I find it inspiring and potentially fruitful to detect commonalities.

Along these lines, in my opinion, Wayne Hershberger's handling of Volitional Action was exemplary. What a tribute to control theory! The breadth of that volume speaks highly of the potential of control theory. Speaking of Wayne leads me to think that it seems we could have the same sort of discussion as above with a focus on Gibson's ecological psychology. In fact, I recall you, Rick, not long ago stirring up something about Gibson. You wanted his work booted out too. As you might surmise, I agree with Wayne's position re. Gibson's work. (One of these days you will learn to express your opinion.)

I cannot answer whether or not Carver and Scheier will help you "to understand human nature in the context of a control system model of behavioral organization." This all does remind me that you mentioned something about a symposium at APS. Why don't you see about doing something with them?

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Date: Wed Oct 02, 1991 6:19 am PST
From: Bruce E. Nevin
Subject: in the vat

[From: Bruce Nevin (911002 0734)]

Suddenly swamped. A few quick comments, but mostly I'll be in a yin phase for a while.

Ed Ford (910927.1140) asks:

>I recently posted a letter (910913) that I sent to Dr. Gerald Corey who
>is the author of Theory and Practice of Counseling and Psychotherapy.
>In the book, I quoted him as saying "Although the ideas of control
>theory are not original with Glasser, most of the recent work on this
>new theory and how it can be applied to systems is based on his
>observations, which are summarized in his 1985, Control Theory."

Glasser's ideas about control theory are based loosely on the seminal 1973 book BCP by WP.

RYATES (Tue, 1 Oct 1991 18:30:37 CST)

>But the analogy to primary language learning fails in one crucial way: no child
>decides "I will not learn a language." In fact, you have to literally lock
>the kid in a closet for 13 years for her not to learn a language.
>People have choice to learn to ride a bike or not; people don't have choice
>about learning a first language.

I thought that was called autism.

Victor Raskin <raskin@j.cc.purdue.edu> responded to me in another venue to a friend's request for information about rearing children bilingual. He and other respondants said that a key factor is the social context of using language to do things. An excerpt:

>'Naturally' is the key word here. The baby cuts through any crap, such
>as one-parent games (I'll talk French to her and you English).
>Language is a survival/acculturation thing, and the child acquires
>only what is necessary for him/her. If a game is persistently
>enforced, this may result in alienation from the enforcing parent or
>from the one linguistically excluded. When I see a Chinese woman with
>a monolingual American husband babytalking Chinese to her baby, I know
>that the father is really fond of his golf-playing and/or
>beer-drinking buddies.

>The languages are typically beautifully balanced at the preschool age.
>The child never mixes them and never makes a mistake about which to
>use when. Well-balanced, the languages do not interfere with each
>other at all. The chill will vehemently object to the parents' mixing
>the languages, especially when pronouncing things like 'income tax' in
>Russian phonemes. Later, she will acquire the ability to mix language
>like wordplay, but the phonology remains unmixed.

>Unsupported by cultural and social institutions and practices, like
>Sarah's Russian (thank G-d for that!), the "other" language will start
>being interfered with severely and may become totally dormant.
>However, a few days' even hours' exposure to a group of MONOLINGUA
>speakers resurrect the language almost 100%. Remember, she won't play
>any games--she will speak Russian to you only if she must. when
>fascinated adults asked her to interpret a word or a sentence for
>them, she coolly refused, saying that they did not need it. At the

>same time, she provided voluntary and competent interpreting from her
>parents' Russian for her beloved American nanny at 21 mos.!

Locking a child in a closet would not just remove her from exposure to language, but from engagement in all social interactions, including especially but not uniquely the way people "do things with words."

As I understand it, in at least some forms of autism (no physiological damage) the child chooses not to interact. I read a few case studies ten or mor years ago, but don't have references now.

Different languages entail different ways of being human. We have read on this net how one person refused to learn proper French pronunciation because he refused to hold his face in that "prissy" way. Another spoke flawlessly only when he indulged in what was to him laughable and embarrassing comportment (which looked perfectly normal to a native speaker). It is this ability to assume a different way of being human I believe that gets locked up at puberty, and not some mystical language acquisition device. No teenager is willing to be a baby!
I am coming to speculate that our much-vaunted langue vs parole distinction may be illusory--that "language" is really only the systematic, scientific arrangement of descriptors of speech. If it be a fundamental error to hypostatize this as the Real Reality underlying the diverse ways of speech of different members of a speech community, then problems about teaching all students in a program "the language" take on a different complexion. I don't know quite what to make of this yet, but am mulling it over. It seems to me to be a straightforward corollary of a CT view of speech.

Bill (many posts)

I too believe that we are in fundamental agreement, tugging on different parts of the same bundle with the same sorts of goals. I am not in a position to do any sophisticated modelling, and language performance is notoriously difficult to disturb. I will be mulling over our conversation, digesting, integrating, and when I get a bit of time free again will perhaps be able to pose some more productive questions and suggestions.

I hope you have been able to get a copy of Lieberman and Blumstein. Many important clues and insights there. For instance: articulatory positions for stops used by most languages are regions where relative imprecision in location makes little difference to formant positions, for aerodynamic and acoustic reasons (labial, alveolar, velar, pharyngeal). There are two pharyngeal values, and these are less used cross-linguistically (in different languages), I would guess because of the more extreme effect on adjacent vowel formants. Many languages distinguish dental/alveolar/retroflex for the tongue tip; palatal is almost always affricate c (ch or ts stop plus spirant) and/or palatalization (y-sound) of adjacent vowels. All in all these are less "universal" than the labial/dental-alveolar/velar p-t-k series just noted. L&B also suggest that vowels are more dependent on acoustic perceptual cues and consonants (other than this p-t-k) more on articulatory cues (what would I have to do to produce that sound?). A very rich trove here! I hope you are getting a chance to delve into it.

I'm also waiting with bated breath for more news of tracking formants. L&B will help you here a lot too, I believe. I passed your code and comments on to my dad, but we haven't had any time to talk about it, too many hurricane-felled trees to cut up and stack.

I have to focus on my work here at BBN and on preparations for that exam re-take in a couple of months, not to mention dissertation research.

Be well,

Bruce

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Date: Wed Oct 02, 1991 6:20 am PST
From: POWERS DENISON C
Subject: language universals

[From Bill Powers (911002.0750)]

Joel Judd, Bob Yates (911001) --

Bob, you say

>In other words, judgements about structure a was always better than
>judgement about structure b, which were alwasy better than judgements
>about structure c, etc. Almost all means 3-4 out of 100+.

I presume that you meant 3/4 of 100+, not "3 to 4.".

If 3/4 exhibit the phenomenon, then 1/4 don't. This means it's not a universal phenomenon, a property of human beings. When there are that many exceptions to the rule, one has to suspect that we're looking at a social/cultural phenomenon. Practically everyone in the country stands when the flag is shown and the national anthem is played. That's because we're taught to do it that way, not because it's a universal behavior. To see this as a universal behavior, "practically everyone" would have to mean 99.99%, with serious looks at the remaining 0.01% to see where we measured wrong, or what's happened to these individuals (Oh -- a paraplegic or a visitor from another country).

Of course when you get 3/4 of the people showing some behavior pattern, you could be on to something. It might be that you've found a measure that is related to the true phenomenon, but includes too many or too few dimensions of variation. It could be that you're looking at something that depends indirectly on some truly universal property of human beings. To get closer, you have to study the people who didn't fit the majority pattern and see what's different about them.

Patterns of progress in language learning might be cultural, in that most people have seen how others raise children and teach them things, and try to do it the same way (all parent being amateurs). Most of us have heard baby-talk (and perhaps swore we wouldn't indulge in such foolishness until we found ourselves doing it with our own babies, for lack of a better idea). School systems share textbooks and methods, and teach children about language in much the same way -- this surely isn't a basic property of human beings! If there is any basic property involved here, it's the ability of children to pick up any damned fool approach to learning that adults currently think is effective.

On the other hand, it could also be that there are universal stages through which human beings MUST go in order to learn language. But as Joel Judd is saying with growing conviction, along with Bruce Nevin, these universal properties are more likely to *include* language learning rather than being confined to it. Clearly, before you know the meanings of any words, you can't acquire grammatic rules. Before you can grasp things, you can't learn how to use a fork. So that's a crude but probably inescapable kind of universal progression. There may be many others of this kind that are not so crude and are just as absolutely true. The levels of perception might give us a lead: you can't perceive/control relationships before you can distinguish the things that are to be related, and so on. This is the sort of fact about development is that really likely to be true of every human being in good working order, with only a minuscule number of people deviating from the pattern. That's really what we're after in any scientific approach to human nature, isn't it? We want to build up a picture of human nature that is built on very high-quality facts, facts that you can count on being true of everyone you meet. Like the fact that essentially everyone will behave in the same way in the rubber-band experiment -- once you can define "the same way" in terms of basic control relationships instead of the particular movements the person makes.

Lots of good thinking going on among you guys. I'm enjoying it.
Best

Bill P.

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Date: Wed Oct 02, 1991 8:19 am PST
From: RYATES
Subject: Re: language universals

Bill,

I tested several structures and almost everybody had the same order in what structure they had most native-like judgements about, then the next structure. This was way over 75%. Lesss than 5% of my informants deviated from this order.

Joel and I are reading very different types of research in second language acquisition. Some of the things he says researchers aren't doing I think they are.

Bob Yates

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Date: Wed Oct 02, 1991 10:33 am PST
From: jbjg7967
Subject: missing out

[from Joel Judd]

Bob (911002),

There surely isn't a definitive analogy to language, but the point you make is precisely why I wanted to find some way to think about it. Nobody makes a choice not to learn (except pathologically, cf. Bruce's comments) because it would be DEADLY to do so. The organism would not survive. The WHY question in PLA seems to be more and more obvious. The question now is whether we accept it, and what we do with it. There are probably few SLA situations calling for such extreme reorganization on the part of adults nowadays. Even living in a foreign country many can make the choice not to "learn" the L2. Why does a kid learn to ride? Because "everyone else has a bike," "all the cool kids are riding to school," and other reasons perhaps specific to different cultures. Learning the motor programs necessary to ride are secondary to the purpose.

I am very interested in the "very different types of research" you are referring to. I don't expect others to do my library work for me, but a crucial chapter of my dissertation argues that IN GENERAL, SLA theory (such as it is) is influenced by and based on the type of research Runkel calls methods of relative frequency. The well-known studies that aren't, such as Ervin-Tripp (1974) where she follows four Mexican children their first six months in a southern California school, seem to suffer from attempts to interpret them AS IF THEY WERE "quantitative" studies, and so the few insights they provide are missed because of the overall research bias. If I am missing important work, I'd like to know about it--apparently I am. If there is published work which approaches language acquisition as it is reflected in the organization or reorganization of levels of perception in a system, please send me the references.

re: Bruce's friend's comments (911002),

I can add my testimony to the bilingual child comments. I have my mother-in-law from Peru at home, with my wife and three children. The m-i-l speaks almost no English, my wife is fluent in both Spanish and English, and I claim to be also. My seven-year old is hopeless. She was growing up when my wife was in early stages of English acquisition. She is a native English speaker, tried and true. She prides herself on "proper" pronunciation, spelling, and whatever else she's learned in school lately. She enjoys correcting anyone who doesn't do it right. She can say some formulaic things to her grandma, but generally requires/demands translation from my wife or me. She knows some words for things and action, but doesn't conjugate, ar agree, or get gender right or anything else.

The five-year old likes her grandma very much. She too was born while her mother was solidifying her English ability, but her m-i-l arrived a year and a half ago when she was still 3 1/2. She is currently attending a bilingual prep program for pre-schoolers. Her mother takes more time to speak to her in Spanish now, and she communicates with her Grandma almost entirely in Spanish. Yesterday she corrected ME on a Spanish word I had said. While the Spanish input she gets here in the middle of cornfields is limited, it is much improved over that of her sister, ostensibly because a) she very much enjoys "going to school" and being more grown up; and b) because she is more attached to her Grandma.

The littlest (2 1/2) has had a more consistent exposure to Spanish. He gets it all the time from Grandma and virtually all the time from Mom. He went to Peru with Mom last January and was the complete opposite of his oldest sister when she went at the same age. She cried four of the five weeks we were there, refusing to talk to others or interact with them. He appeared at ease from the start, immediately going to others and talking with them. He speaks Spanish mostly to Grandma and Mom, but English to me, except in specific cases where he only knows one word (like the color 'blue'). Much of the language used to interact with us he knows how to perform in English or Spanish. The second daughter speaks to him in Spanish, even though her Spanish is highly constrained--I haven't thought of a way to find out why (other than just asking her).

Didn't mean to go on--just wanted to say "Amen" to the other comments.

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Date: Wed Oct 02, 1991 12:20 pm PST
From: Bill CUNNINGHAM - ATCD-GI
Subject: missing out

:JOEL JUDD:
From the age of your kids with respect to wife's English acquisition, we conclude that Spanish is a lovin' tongue.

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Date: Wed Oct 02, 1991 12:47 pm PST
From: marken
Subject: Carver & Scheier/ We are FAMILY

[From Rick Marken (911002)]

Dennis Delprato -- Thanks for the reply to my post about Carver and Scheier. If you are familiar with their work I would still appreciate getting a description at least ONE of their studies. It would be easier for me to see how their ideas are related to mine if I can see a concrete example of their research.

This discussion of Carver & Scheier (and some of your other comments) suggests a topic that might be nice to thrash around on the net a bit -- namely, what is the relationship of PCT to the results of research done within the framework of conventional social science (by this I mean research based on the independent variable (IV)-dependent variable(DV) approach to experimental research described in virtually all methods texts) ? This topic seems particu-larly germane right now since there have been references(for example, by R. Yates in the language discussion) to some of the published results of conventional language acquisition studies as evidence of a particular (alleged) behavioral phenomenon -- the acquisition of language rules in a regular order. There have been many other references to conventional research, of course, but I can't remember all of them. To a very large extent, control theorists tend to ignore conventional research, although there are notable exceptions (for example, we have done some modelling of results of operant conditioning experiments, challenged the results of work on motor control and coordination -- like the Powers-Williams reply to the Bizzi, et al Science article). But, by and large, control theorists seem to ignore a great deal of the "wealth" of social science research. Why?

The fact that control theorists often ignore conventional research is, I think, one reason that Dennis Delpratto perceives me as "insular". I don't think this is true. I am familair with lots of conventional research -- and I try to scan the journals relatively often in an effort to keep current. In my most recent paper I do try to use results from conventional research as part of my argument for hierarchical control processes. But the fact of the matter is that most conventional research means very little to a control theorist. There are several reasons: 1) it almost always reports data averaged over subjects (this is enough to make it useless right off the bat, regardless of any other merits) 2) the behavioral measurements are too variable (its mostly noise) 3) it is collected in an IV-DV framework so it is often difficult to determine what is controlled (if there is no control then, of course, control theory is irrelevant).

The results of conventional research can be suggestive -- but once the suggestions are taken they must be turned into studies that produce quality data. It is difficult to devise experiments that produce quality data (correlations between variables on the order of .999), especially when dealing with the control of higher order variables (categories, programs, principles). And much of the research that seems to interest conventional psychologists seems to deal with these more complex variables. Language, for example, is an area where control theorists have not done much work. The reason , I think, is because much of the foundation for this kind of research (from a control theory perspective) does not exist. Conventional psychologists apparently assume that they do have a good foundation for the study of language, so they forge ahead. That's fine. I guess control theorists (like me, anyway) suspect that this is a risky business. If you assume, for example, that language involves the generation of outputs rather than the control of inputs, and if the latter is correct, then a great deal of what you discovered in the context of the former assumption is likely to be worthless or misleading. But it also may be very exciting to your peers so you'll die famous and happy, while the sour puss control theorist dies ignored and happy.

I would like to develop this discussion further but first let me respond to a couple of irksome points in Dennis' post. Dennis says:

> From my perspective of the big
> picture, I see so many behind Carver and Scheier that I cannot
> help but note their progressiveness. If one were to lay out the
> fundamental postulates of PCT and examine those of Carver and
> Scheier against these, I believe there would be sufficient
> overlap to conclude that we were dealing with close relatives
> here.

This sounds more like religion than science. Don't we invent these myths (ah, theories) in order to explain a PHENOMENON? I don't care if someone's theory sounds like mine. I want to know what they are doing with it. That's why I want to know what their research is about. People have used the control theory myth to explain presumed cause-effect phenomena for years. That's what the whole discussion about the "old" manual control theorists that we had several month ago was about. These "old" control theorists MISSED the main phenomenon that control theory (PCT) explains -- PURPOSIVE BEHAVIOR. So I am not that impressed when people pledge allegiance to the same or a similar theoretial system as mine -- I'm more interested in what thay are doing with it. What phenomenon are they trying to explain. Many psychologists have enjoyed the control theory myths -- the legends of negative and positive feedback, loop gain, etc.-- but few have understood what these myths were invented to explain -- the PHENOMENON OF CONTROL.

> I prefer integrationism but with a definite bias as noted above
> (i.e., not eclecticism). There is a great deal of junk out
> there. I find it inspiring and potentially fruitful to detect
> commonalities.

Me too. But

> I recall you, Rick, not long ago stirring up something about
> Gibson. You wanted his work booted out too. As you might
> surmise, I agree with Wayne's position re. Gibson's work. (One
> of these days you will learn to express your opinion.)

I don't want anyone "booted out" of anything. I just want to know how their ideas can help me understand the phenomena I want to understand. If their ideas or theories or observations seem to be incorrect then I think I get to say so. But I certainly have to make a convincing case (verbally or, better, through modeling and observing). This ain't a religion. We don't prove we're right by excommunicating or otherwise eliminating opposing points of view. My comments about Gibson had only to do with the fact that he claims to explain a phenomenon (perception) when he doen't really have a model(IMHO). His verbal statements suggest that he beleves in environmental entities (affordances, invariances) that have properties that I don't imagine the environment has. I think Gibson has come up with some very interesting experiments/demonstrations, at least one of which I mentioned as having interesting implications for the relative ordering of the transition and configuration levels in the control model (a set of lines becomes a three dimensional cube when they move -- transition preceeds configuration perception).

It is true that I don't know how to express my opinions without them becoming disturbances to those who disagree. But I hope that you can assume that my opinions are NEVER intended to be a personal insult to those who disagree with me. If they were perceived as such I humbly apologize.

Now, let's talk some more about the relationship between PCT and conventional social science research.

Regards

Rick

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Date: Thu Oct 03, 1991 6:08 am PST
From: POWERS DENISON C
Subject: Language; Carver & Scheier

[From Bill Powers (911003.0700)]

Bruce Nevin (911002) --

Good to hear

>I too believe that we are in fundamental agreement, tugging on different
>parts of the same bundle with the same sorts of goals. I am not in a
>position to do any sophisticated modelling, and language performance is
>notoriously difficult to disturb.

What we need is the linguistic equivalent of the rubber-band experiment. That's not very sophisticated, but it makes the point. I can think of lots of modeling work that would be lovely to do but is beyond me. At this point in the growth of control theory I think it's less important to do advanced experimentation than it is to accumulate proof-of-principle demonstrations. Despite my bursts of enthusiasm for learning about sound spectrographs, formants (yes, I've now seen Lieberman et. al.) and the like, I'm not going to mount any major experimental effort in linguistics. That would be silly, with real linguists around. If I can come up with a few new slants on the subject, great; I bequeathe them to anyone who can make use of them.

Remember, I'm 65 years old and not in pursuit of career goals or a spot on a talk show (unintended pun). From my point of view, our approach to a reasonable level of agreement is a comfort because it tells me I don't have to go much further in getting linguists to understand and apply control theory, which is the extent of my desires in this field. You, Joel, Bob, and others with professional knowedge in linguistics will be the ones to transform this discipline, just as Chuck Tucker, Clark McPhail and Kent McClelland will transform sociology and Rick Marken, Tom Bourbon --- well, you get the idea. My ambition is to start something like this wrt all the sciences of life or as many as deign to succumb before I start forgetting what it was I wanted to do. This is the level at which I feel capable of doing something useful. When all the sciences of life grasp the principles of control and begin using them to make sense of their data, there will be only one science of life with areas of specialization, and I will be even more unemployed (but happy).

The tracking-formats stuff is on temporary hold while I catch up on converting the Little Man to C. Also, my Dad's coming to visit (driving up from Tucson at age 91-1/2) next week and so is my daughter Allie, so I have to put a cat door in the downstairs bathroom window (I'm sure you follow that) because of my daughter's dog. But I will finish it to the point of seeing how it works.

Bob Yates (911002) --

Your data sound good. As I DO have to rely on others doing the library work for me, how about a precis of this research?

If you have truly found some believable regularities in order of learning, this could help us understand the levels in the hierarchy better, and how they grow. Control theory can't predict things like that; all it can do is put them in a framework of understanding and suggest implications to be checked out by more experiments. The structures you are talking about might turn out to be related to levels of perception in general. Control theorists may come down hard on other theories that aren't really theories, but we don't throw out good data or try to pretend it doesn't exist. Tell us more.

Re: ADAPTIVE BEHAVIOR: maybe this would be a good place to publish some models.

Rick Marken, Dennis Delprato (911002 &&)

Carver and Scheier: The winter before the Indiana University of PA CSG meeting of 1990, I called Carver and asked if he might come to our meeting. He was too busy; I'll try again. I mentioned the "self-awareness" research and he said that they had given up on that years ago because the results weren't clear.

Mary got hold of "Origins and functions of positive and negative affect: a control-process view" (Carver, C.S. & Scheier, M.F (1990), Psych Rev *97* No. 1, 19-35). I had seen an early version of it. My problem with C and S is that they do quite well at presenting the basic model (their examples are good and their explanations are clear) but they insist, for some reason, on saying that it's *behavior* that is controlled. I suppose you could define behavior so it means outcomes or consequences of action, but I just get the feeling that an insight is missing here. I really think they have the basic ideas. It could just be that they're compromising in order to be understood, but I don't like that -- it just solidifies wrong concepts of control (they seem to reify goals, for example, making them external or objective). I'll try to get them to come to our next meeting. The best way to understand what they're trying to do is to ask, and it would probably do them good to mingle with CT people from other fields. Dennis, how would you like to take on the job of persuading them to come to the next meeting?

I hope a lot of people on this net are considering coming to the next meeting.

Best to all

Bill P.

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Date: Thu Oct 03, 1991 7:10 am PST
From: jbjg7967
Subject: quickie

[from Joel Judd]

Just wanted to see if the following summary makes sense before strolling down the path too far (Rick's comment [901002] caused me to write this).

I took a look at Pienemann(1988), the article Bob Yates mentioned yesterday. In this the author reports on ten children (7-9) learning German as a L2. He uses the order of acquisition of one aspect of inversion in German in order to study the "teachability" of this rule. He shows his hand at the outset--he wants to know "what is the optimal way of presenting any set of structures which has been selected as a learning objective" (p.88). He tries to affect the natural acquisition process by introducing a grammar rule instructionally BEFORE subjects show evidence of using it. Details of two subjects were given, one of which repsonded to instruction and one of which didn't. Why? "Since all other so-called learner-external factors had been controlled for, the differential had to be attributable to a learner-internal factor--namely, the successful informant's readiness to learn INVERSION. Thus the same input produced different results because only in one case had the learner acquired the prerequisites for the correpsonding learning process" (p.89). His conclusion after looking at all the results? "In the mixed setting of natural and formal L2 acquisition a certain linguistic structure (INVERSION) could only be added to the interlanguage by formal instruction, if the learner was close to the point where this structure was acquired in a natural setting" (p.98). In other words, the search for "optimal" input configuration is going to be constrained by the learner's own [perceptual control] development. Bad news for traditional educational practices.

So Rick's request for comparison (between PCT and other theories) interests me now, for the reasons he suggests. I'm going to be asked why I "ignore" much linguistic research. A study like the one above is a case in point. OK, so every one of the ten kids acquired the INVERSION in the same way; every kid produced similar, observable behavior. I don't know

WHY they're doing it. The author's own perspective is one of education as the-search-for-most-effective-input. There is something inherent in German such that all who learn German are caused to learn it in such and such a way. It seems to me the kids are learning to control a linguistic perception of INVERSION, but there is nothing in the study to suggest HOW they are doing so. Pienemann suggests a "learner-internal" factor is at work, something called "readiness to learn INVERSION." I'm getting the feeling that Bob and I are not so much reading different research as reading the same resarch differently. Am I missing something important here?

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Date: Thu Oct 03, 1991 8:30 am PST
From: jbjg7967
Subject: real quickie

[from Joel Judd]

I meant to leave a summary statement (earlier) to check my understanding, and then lost track of it in the article summary. How does this strike anyone:

It's not the fact that a particular linguistic structure is acquired [in a particular order], but the fact [PCT claims] that all people in a given linguistic community learn to control the perceptual aspects of the language of that community, that should be of primary importance. If it demonstrated that all learners of language X learn feature X1 the same way, then that should lead to investigation of what it is about X1 that reflects common perceptual control. There is nothing inherent in "the language" that will lead us to understand how and why language is learned.

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Date: Thu Oct 03, 1991 8:31 am PST
From: Bruce E. Nevin
Subject: readiness to learn INVERSION

Learner A learned it, learner B didn't. He says the difference was that learner A was close to getting it sans formal instruction. How does he know this?

"Readiness to learn INVERSION" or an "INVERSION-acquisitive property" sounds oddly close to the "dormitive property" Voltaire lampooned in Candide. Is this offered as an explanatory principle, like so much of the alphabet soup of linguistic acronyms, or is there something there that might be restated in terms of testing for control?

Ignoring "the literature" is a guaranteed way to marginalize yourself. The only approach I can see is to show what is missing and articulate why it is crucially needed to get results that mean anything. A few works can be picked on, then others subsumed by reference as being members of the same category (subsumption with neglect of differences that don't make any difference, thank you Mr. Peckham). This fattens your bibliography in a legitimate way, and if you have spelled out the conditions for "significant contribution" clearly enough (sketch of CT and test for control) any objector must show how some particular work does meet those conditions after all. A much more productive stance to take, I think, than ignoring them as useless.

In haste,

Bruce Nevin
bn@bbn.com

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Date: Thu Oct 03, 1991 9:48 am PST
From: marken
Subject: Ignoring the literature

[From Rick Marken (911003)]

Bruce Nevin (911003) writes:

>Ignoring "the literature" is a guaranteed way to marginalize yourself.
Trust me. Your marginalized even if you DON'T ignore it -- guaranteed.
>The only approach I can see is to show what is missing and articulate
>why it is crucially needed to get results that mean anything.
>A few works can be picked on, then others subsumed by reference
>as being members of the same category

I've tried EXACTLY this approach in my studies of coordination. I've referred IN DETAIL to well known and RECENT studies that are considered to be important. I explained what was missing from the studies in the literature (observing hypothetical controlled variables under CONTINUOUS disturbance, for example). I have managed to publish a couple of these studies. What I have found is 1) when you do refer directly to the literature (as you suggest) you make it harder for yourself to get published (because all the reviewers KNOW that you are wrong) and 2) if you manage to twist your way through the review maze then your publication is ignored (the "other side" guarantees the insularity you tried so hard to avoid).

>This fattens your bibliography in a legitimate way, and if you have
>spelled out the conditions for "significant contribution" clearly
>enough (sketch of CT and test for control) any objector must show
>how some particular work does meet those conditions after all.
>A much more productive stance to take, I think, than ignoring them
>as useless.

The objector "must" do nothing of the kind. The objector (based on my reading of reviewers comments) has no idea what you are talking about. The concerns of a control theorist are nearly orthogonal to those of most other psychologists. I had this problem in another area -- operant conditioning -- where I published research to test "reinforcement theory" explanations of goal directed behavior. After reading my description of the control model of a particular behavior, one reviewer said something about how the "controlling variable" was obviously such and such. Reviewers are not above hallucination -- this reviewer was commenting on my description of a controlled variable. He or she could not even perceive the fact that I was discussing variables that are controlled by the organism; all the reviewer knew was that variables control the behaior of organisms. It was impossible (even with math and computer programs) to get the concept of a controlled variable across to this (and MANY other) reviewers.

I admire anyone who tries to act the way you suggest -- going to the literature, finding a juicy study and working on a control theory approach to dealing with it. Sounds right. I hope it works for you (or anyone else willing to try it). But I think the only way to make it work (and I'm really trying not to be cynical here) is to recast control theory so that you can do the kind of research with which the "opposition" will be familiar -- that is what, I think, Carver and Scheier do. I just don't want to do that; it takes away alot of the beauty of control theory for me (besides the fact that it's wrong).

For the last ten years I've tried to connect my reserach to "the literature". I think I did a pretty good job -- but the only people who read my stuff are people who already accept PCT. So I'm done with actively trying to connect to the existing literature -- the people out there just aren't that interested. My future research will be dedicated to testing various aspects of the control model, whether that matches up with stuff in the

literature or not. I won't ignore "the literature" but I won't go out of my way to link up with it either. I figure it like this -- conventional psychology is to PCT as alchemy was to chemistry in the 1600s. You can either spend your time showing what's wrong with alchemy or you can spend it showing what's right with chemistry. I'm now more interested in the latter.
Best Regards

Rick

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Date: Thu Oct 03, 1991 10:51 am PST
From: jbjg7967
Subject: see what I mean?

Re: Rick's and Bruce's go-around on literature reviews--that's why I put "ignore" in quotation marks--I'm damned if I do and don't graduate if I don't.

Date: Thu Oct 03, 1991 11:06 am PST
From: Bruce E. Nevin
Subject: color me naive
But maybe there's an outside chance the situation is not so entrenched in linguistics and applied linguistics such as SLA. To the extent that SLA folks want to show up "respectable"
psychology it may be equivalent.

Here is a problem that bears exactly on the problematic social aspects of CT. What perceptions are they controlling, that they ignore and distort real-time perceptions (e.g. your words) and resist them as disturbances? What practical techniques for communication, teaching, and persuasion have been or can be developed from CT and applied reflexively to the promulgation of CT itself?

Bruce

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Date: Fri Oct 04, 1991 5:04 am PST
From: Bruce E. Nevin
Subject: limitations of response

[From: Bruce Nevin (911004 0723)]

Joel Judd (Thu, 3 Oct 1991 13:38:57 -0500)
> I'm damned if I do and don't graduate if I don't.

Here I find us agreeing with Peckham when he inveighs against

>those guardians of remote meta-directions - such as philosophers - [who]
>are busy about . . . the limitation of response to remote explanatory
>terms. . . .

One cannot respond except that the response (a) be in "normal" terms, (b) be distorted so as so to construe it, or (c) be ignored. This is limitation indeed, but it is limitation on what your audience makes of your immediate response.

Emendation 1 to Peckham, then, it to observe that B's response is no response at all until it is perceived by A, and that it is what A makes of those perceptions (at higher levels of control) that constitutes the "response" and not B's behavioral outputs themselves. It is in the "normalization" of B's behavioral outputs in A's perceptual hierarchy that the limitation is imposed. It is in the possible mismatch between (1) B's perceptions of A's consequent behavioral outputs and (2) B's expectations and goals, that that limitation affects A. It is A (and not B) who may impose consequent limitations on herself in the interest of coordinated action with B, or may abandon that goal of coordinated action for the sake of other goals. The range of limitations that A imposes on herself includes adaptation to B's construal at one extreme, and a renewed effort to communicate the intention behind her (A's) "response" at another. The latter is a limitation insofar as A would prefer to get on with the goals that called for coordinated action with B.

It is so much easier to hypostatize the "response" as an "objective event" out there in the shared environment of A and B, and so much more awkward to bring it back to A's and B's respective perceptions, who would be willing impose such limitations on discourse and thought without first having some goals that merited the trouble?

This is an attempt to begin applying CT to our dilemma. Can we say more about this, and find our way out of the paper bag? The offered alternative (truth will prevail eventually, just persevere as maverick scientists) is not inviting. No wonder people prefer "normal science"!
Bruce Nevin

Date: Fri Oct 04, 1991 6:16 am PST
From: POWERS DENISON C
Subject: Inversion; marginalizing [From Bill Powers (911004.0600)]

Joel Judd (9110.03) --

INVERSION: What is inversion? Isn't it the deliberate alteration of sequence (i.e., order of utterance of words) ? "Readiness to learn inversion" may mean "readiness to vary reference signals for sequence" in general. In order to manipulate sequence (i.e., to begin choosing different reference-sequences instead of just repeating the same sequence again and again by rote), the child must have started learning control at the program level (or whatever level is next that would use *variations* in sequence as its means of control). Control of sequence is evident if the child is capable of reproducing the same sequence over and over and correcting errors in sequence. To VARY sequence, however, requires a higher level of control.

If this is true, and if learning at the program level is a real stage of development that cuts across all modalities, then there should be evidence that the child is also learning to manipulate sequences of many other kinds. Is this the age where it is hilarious instead of frightening when someone walks upstairs backward, runs a movie backward, or reverses some other familiar sequence of familiar (categorized) events? There should be evidence of a general change in behavioral organization if readiness to learn (verbal) inversions signals an important shift to a higher level of learning.

You have said or at least hinted something similar in saying

>If it demonstrated that all learners of language X learn feature X1 the
>same way, then that should lead to investigation of what it is about X1
>that reflects common perceptual control. There is nothing inherent in
>"the language" that will lead us to understand how and why language is
>learned.

The problem with specialization is that it disguises general capabilities as specific ones. If you were studying arithmetic skills, you would probably identify this same stage as "readiness to learn logic" or something like that. The linguist, focusing only on language, sees the term "inversion" as meaning "word inversion" instead of "variation in sequential order" in controlling ANYTHING.

This makes something a little clearer for me. At the highest level that is usefully organized, reference signals are taken from memory. We repeat what we have experienced, over and over and over. This builds up one kind of experience as the only reference signal we normally use. As mastery grows, we repeat different experiences of the same kind in the same stereotyped way. When a new level begins to appear, however, instead of repeating the same old experience we begin selecting different states of the same perceptions and controlling relative to them: this is the sign that the new level is connecting to the lower-level reference inputs, either by selecting different memories or (?) bypassing memory as the only source of reference signals. Good rhetoric, anyway.

Bruce Nevin (911003) --

>"Readiness to learn INVERSION" or an "INVERSION-acquisitive property"
>sounds oddly close to the "dormitive property" Voltaire lampooned
>in Candide.

In fact, doesn't "readiness to learn INVERSION" mean precisely that the child has NOT learned inversion? The data show that A has learned it and B has not. Therefore B must be "ready" to learn inversion ?????

Rick Marken and Bruce Nevin --

Marginalizing yourself may be necessary if you're to do anything new. I speak from experience. I tried to centerize myself by going into graduate school in psychology (1960) and had to leave after a year. When (generic) you study a field as intensively as you're asked to do in graduate school, your mind becomes challenged by the problems and you start thinking up answers and questions of your own. I was different from most graduate students, having spent a lot of time developing an independent approach to behavior from far out in left field before becoming immersed in learning what goes on in psychology (other than what is learnable in undergraduate laugh courses). I could see that I wasn't just being asked to solve the problems presented to me; I was being asked to accept the underlying and mostly unspoken premises that defined the problems -- and to ignore the same problems that my teachers ignored. It was like going for a stroll in the park and suddenly finding you were in quicksand up to your knees. I think that most graduate students don't notice until it's over their heads, if they notice at all. Graduate school is an intense learning process that can be of great value, and I often regret not having gone all the way through it. But it's also a powerful and effective form of brainwashing to which I found myself alarmingly susceptible. The "mainstream" consists of people who have gone through the brainwashing without becoming aware of it.

Of course it's only "brainwashing" if you have a better idea and lose it in the process. In less alarming terms, it's acquiring principles and system concepts. If you don't acquire them in college or graduate school you acquire them somewhere else. But I think that graduate school is TOO good at filling in the blanks: it leaves no room for explorations and variations outside the framework being taught. The teachers have resolved all the sticky questions to their own satisfaction, which is far from saying that they've resolved them. The graduate students, when they come up against the same sticky questions and see how easily their mentors glide past them, think there's something wrong with themselves -- gee, that doesn't seem logical to me, but he must know what he's talking about so I'll just have to accept that and hurry to catch up with him.

We're talking, of course, about the people who review your papers, Rick. You and I look at the referees' comments, roll our eyes and gnash our teeth, and laugh incredulously at the arguments they present. "You've forgotten about discriminative stimuli!" Ho ho ho. But to those reviewers, those arguments are the fruits of thousands of hours of labor and confusion. They have overcome vast difficulties in reaching the understanding they have. Those difficulties didn't warn them that there was something wrong; they simply increased the conviction that what they were learning was important, just as THEIR professors insisted.

Then along comes Marken with a neat simple little experiment and analysis in which nothing is hidden and everything makes sense, all without using a single concept that the reviewer considers essential to understanding. If Marken is right, that reviewer has been had. Leaving out the concept of discriminative stimuli isn't just an oversight on your part; it's a direct threat. You're saying that you don't NEED that concept to explain the behavior. But if the reviewer were to understand what you say without recourse to that concept, where would that leave everything else that the reviewer considers important? The reviewer, faced with papers like yours, isn't controlling for scientific correctness. He's controlling for personal scientific and intellectual survival. You may not like it, but you can't blame the poor sucker.

Lest we forget, faith in anything leads to this same problem -- even faith in control theory. Faith means that you've accepted something without taking all the steps that lead from there to here. I think it generally means that you've bypassed some problems that you couldn't solve, and have just sort of waved your hands and abracadabra the problem is behind you. We can't sit around stewing over every little discrepancy forever; eventually cirumstances require that we move on. All our ideas are riddled with patched-over holes. that's just life. But awareness of the holes, while uncomfortable, at least gives you the chance to reflect now and then, to lift the patch and see if you really should have concealed what was under it. Every patch is a choice-point, and sometimes it's necessary to make a different choice, even if that invalidates a lot of work. Who is brave enough to do that?

Here you are in a big ocean liner with swimming pools and parties, on your way to Copenhagen, and you spot this little rowboat full of earnestly rowing people heading off at right angles to your course. You yell to them "where are you going?" and they yell back "Copenhagen!" Are you going to leave the bright lights and the comforts of the cabins and the professional helmsmen and the fatherly Captain for a place in the rowboat? Even if there's a chance that the ocean liner is off course? Isn't it more likely that those fools down below will end up in Baffinland or dead? Consider all the ocean liners and all the rowboats; calculate the average destination; compute the confidence level; go back to the party. Even if you don't end up in Copenhagen, ocean liners are more comfortable than rowboats, and less work.

I don't think that control theory can be adapted to other approaches. I think that trying to do so will just prolong the agony. A reorganization is needed. But there's a lifetime ahead and we have to be practical. Bruce Nevin said it:

>I'm damned if I do and don't graduate if I don't.

I didn't graduate. But here I am.

Best to all,

Bill P.

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Date: Fri Oct 04, 1991 6:34 am PST
From: TJ0WAH1
Subject: Robinson's oculomotor model

[From Wayne Hershberger]

(Gary Cziko 910924 re: servos used in model aircraft)

>Now, are these servos strong enough to interact with a human?
>That is, could I grab hold of the arm (if only delicately with
>two fingers) and disturb it and feel it fighting back? For a
>good demo, it should have enough loop gain and "muscle" so that
>I can feel it resisting, but not so much so that I can't even
>budge the arm.

Yes to both questions, and one needn't be particularly delicate. The smaller servos generate less torque (about 20 ounce-inches) than the larger ones (about 60 ounce-inches) but any size will do. I frequently use one in class to demonstrate exactly what you suggest: disturb it and feel it fighting back. It vibrates a bit when it is fighting back and one feels this vibration as much as anything. The arm simply feels very stiff or rigid when the power is on. In contrast, it moves relatively freely when the power is off.

(Gary Cziko 910924)

>I just got home from a talk given by Ted Weyand, a post-doc here
>in Joe Malpeli's lab on "Cortical Circuits in Eye Movements."

>Using cats doing saccades in an "operant conditioning" paradigm,
>Weyand has done lots of single-neuron recording of corticotectal
>cells which connect the frontal cortex, "association" cortex,
>and visual cortex with the superior colliculus. From what I
>understood of his talk, it seems that his findings are quite
>consistent with PCT, with the superior colliculus acting as a
>comparator.
>
>When I asked him about this, he didn't seem to know anything
>about control systems. But Joe Malpeni talked to me later and
>said that there is a large literature on understanding eye
>movements using control systems. He mentioned in particular the
>work of David Robinson at Johns Hopkins.
>
>Wayne, if you don't already know of Robinson's work, perhaps you
>should check it out. If you are familiar with, perhaps you
>could let us know how close he actually comes to using a
>control-theory model.

Thanks for the information Gary. I will get in touch with Joe Malpeli. As for David Robinson's model of the oculomotor system, it is definitely closed loop. Robinson is a first rate control theorist. One of my preprints (with Scott Jordan) I distributed in Durango in August described an improvisation on Robinson's model. It is titled "Visual direction constancy: Perceiving the visual direction of perisaccadic flashes," and deals at length with the superior colliculus. Did you pick up a copy? If you did, please take a look at it and let me know how closely it fits Ted Weyand's presentation.   If not, I'll send you a copy.
Warm regards, Wayne

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===

Date: Fri Oct 04, 1991 7:32 am PST
From: Gary A. Cziko
Subject: Re: Robinson's oculomotor model

Here is a copy of a message I sent to Wayne Hershberger of NIU who was responding to some queries of mine after meeting you and Ted Weyand last week.

I have set up an electronic e-mail network for discussions of control theory which includes about 130 people in 19 or so countries. Some of what goes on there may be of interest to you. I could put you on the system, or you might want to take a look at it via a local usenet newsgroup you can access on campus. It is info.csg-l.

[from Gary Cziko 911004.0945]

Wayne Hershberger (911004)

>Thanks for the information Gary. I will get in touch with Joe Malpeli.

His e-mail address is j-malpeli@uiuc.edu. I may send him a copy of this message to you to break the ice.

>As for David Robinson's model of the oculomotor system,
>it is definitely closed loop. Robinson is a first rate control
>theorist. One of my preprints (with Scott Jordan) I distributed
>in Durango in August described an improvisation on Robinson's
>model. It is titled "Visual direction constancy: Perceiving the
>visual direction of perisaccadic flashes," and deals at length
>with the superior colliculus. Did you pick up a copy? If you
>did, please take a look at it and let me know how closely it fits
>Ted Weyand's presentation. If not, I'll send you a copy.
>Warm regards, Wayne

I think I have a copy, but it never made it out of my "to read, sometime" stack. I'll have to retrieve it and give a look.

But your response is relevant to the "marginalization" discussion that has been going on. If Robinson work is so highly regarded and if he is a "first-rate control theorist" then he must be publishing in places that are receptive to this model of behavior. Of is this type of research itself "marginalized" within psychology? I suspect that perhaps your idea of a "first-rate control theorist" is different from Rick Marken's. Does Robinson look at controlled variables? Does he construct models of working systems? Or is it an adaptation a la Carver and Scheier of control theory to fit the traditional mold of psychological thinking?--Gary
================== Gary A. Cziko Telephone: (217) 333-4382
University of Illinois FAX: (217) 244-0538

Date: Fri Oct 04, 1991 10:37 am PST
From: Bruce E. Nevin
Subject: SNAYSFA

[From: Bruce Nevin (911004 0159)]

So Near And Yet So Far Award:

> Premack discovered the reversible relationship between rewards and responses. What served as a reward in one setting would serve as the thing to do to get a reward under other conditions. The key concept in Premack's theory of motivation was the notion of a

probability of response. In order to increase response A, a more probable response had to be made contingent [on] it. Thus, let's say an animal had equal access to water and running. Let's also say that prior to being exposed to these two, a rat had had ample opportunity to run but not to drink. It would come as no surprise to observe that such an animal would spend more time drinking than running during our equal access test. When the opportunities were reversed, so were the results. Premack then went on to show that the thirsty animal would run in order to have the opportunity to drink, and that the confined animal would drink in order to have the chance to run! In short, what served as a reward was not abslute. It depended on the context, the psychological state of the animal. Such a powerful concept as this leaves simplistic pharmacological experiments [e.g. administering opiates to brains of clinically depressed persons sans motivating context--bn] doomed to failure.

Gazzaniga, _Mind Matters_ p. 116

Bruce Nevin

Date: Sat Oct 05, 1991 7:40 am PST
From: eprince
Subject: autism

Does anyone have any ideas about new ways of "treating" autism. I joined this conference because of applications to ESL (and I've been too swqamped to really get involved). Hopefully, I'll be able to become a more active partgicipant. However, in the mean time, I have an almost 13-year-old now basically non-verbal (limited ASL) autistic daughter. Without spedning a long time on history, let me just say that although I do in fact appreciate the contributions the B-mod approach used at the May Institute has made to her ability to learn, I am quite unhappy with their whole approach to communication (purely B-mod, not with a goal of learning any kind of real langauge). Any ideas or comments?

They can be put on the conferece or sent to me at enam@lynx.northeastern.edu.
Tna

Thank you

Eileen Prince
Associate Director, English Language Center
Northeastern University
Chair, Program Administrators' IS, TESOL

Date: Sun Oct 06, 1991 10:57 am PST
From: POWERS DENISON C
Subject: CT and autism

[From Bill Powers (911006.1000)]

Eileen Prince (911005) --

Welcome to CSGnet, Eileen. There will be many listening who wish they could dip into the resources of control theory (or any theory) and come up with a solution that would help your daughter. I wish I could. At least you can be sure that many people will be racking their brains trying to think of something useful to say.

The basic problem is that no one who understands control theory has studied austistic people to see how the theory can be applied -- no one I know about. Control theory isn't like most other theories: it doesn't say that if X happens to people, Y will be the resulting effect on their behavior. It's about the way behavior works; it describes relationships of a very general nature between perception and action. At the same time, it is a theory of individual behavior: in order to apply it to an individual, one must determine what variables that individual is controlling and with respect to what internally specified states, and the quality of that control. The hierarchical model suggests a nested stack of types of controlled variables that people seem to be able to control when all is well -- but the particular examples of these types that an individual controls can be discovered only by studying that individual.
Control theory doesn't use categories such as "autism" to explain behavior. To say that a person is autistic is only to say that certain externally-visible patterns of action have struck people as similar enough (and unusual enough) to be lumped into a "disease entity." This does not mean that the same defect exists in all autistic people, or that the symptoms arose from some common history, or that the same treatment will succeed with (and not harm) everyone included in this category. The conventional empirical approach to treating problems as "diseases" is simply to try something on people in a given category and see if it helps a statistically-significant number of them. There is no attempt to analyze what has actually gone wrong -- what the person can still do normally, and what the person can't do. There is no attempt to relate deficits to a model of internal

functioning. I suppose the idea is that if you accumulate enough experience with treating people in arbitrary categories, you will eventually be able to look up the symptoms in a big book and read off the treatment that has been effective most often in the past. In my view, this approach is an ill-advised attempt to bypass understanding of the human system and find solutions by relying on guesswork and luck. Before the advent of science it was all we had. Sometimes it works. But there has to be a better way.

If behavior modification helped your daughter, this may give some hints about what is wrong and what is right with her. From the standpoint of control theory, behavior modification techniques work by giving a person control over some variable for the state of which that person has a preference. In behavior-mod, the environment (including the modifiers) is arranged so that this variable can be affected by the person only through performance (or termination) of some particular act. If the person is capable of reorganizing at the level required to do or refrain from that sort of action, eventually doing or not doing the act will become the person's means of controlling the variable. Of course behavior modifiers think of the variable as a "reinforcer" and believe that it is causing the changes in performance, whereas the control theorist sees the performance as the means by which the person controls the "reinforcer" or controlled variable.

If your daughter, during these treatments, modified her own actions in order to gain control over some reinforcer, this tells us several things of importance. First, it tells us that she can perceive the reinforcer (although what she perceives about it may not have been what the behavior modifiers had in mind). Second, it tells us that she had a preference, a reference level, for the state of the reinforcing thing, situation, or whatever that she perceived. Third, it tells us that she is capable of reorganizing at least to the extent of changing her means of controlling that reinforcer, that perception. And fourth, it tells us that the change in action entailed in achieving control did not result in any serious conflicts with other inner preferences (if it had, she would have continued reorganizing and would not have ended up with the same actions).

Of course in behavior modification, the reinforcer is not thought of as a controlled variable. As a result, the kind of control learned must be pretty sketchy. Generally, people whose behavior is being modified are given the reinforcer only when they do something closer to what the modifier wants; their own (other) goals are not considered. The person does not get as much reinforcer as desired, and is certainly not taught how to *decrease* the amount of reinforcer. So the control that is learned is pretty much one-way: one learns how to increase the amount of reinforcer when there is too little of it, relative to the internal reference level for it. Almost nothing is learned about how to control the "reinforcer" both upward and downward as a means of controlling other things.

Behavior modifiers will usually focus on some action that a person is doing that is annoying or dangerous to others, or harmful to the person herself. The *output* is to be changed. But under control theory, the output is far less important than the consequence it is being used to create and maintain. If we see an autistic child (I have seen some but know little other than that) sits silently rocking back and forth for hours, there are two ways we could interpret what we see. As behaviorists, we could say that this rocking-behavior is what we want to change; we would then find something -- candy, praise, pretty lights --that the child is known to like and withhold it until the rocking lessens, then give it as a reinforcer for lessening the rocking. This would "shape" the rocking out of existence. However, if the rocking were an essential means of controlling something else, it would eventually reappear, or some other behavior aimed at the same result would appear --not necessarily one that would be approved.

As control theorists, we would try to guess what perception is being maintained by the rocking actions. We would try supplying that perception ourselves, as a disturbance, until we found something that would cause the rocking to stop. At that point we would be providing what the rocking was providing, so the child's rocking efforts are no longer necessary to maintain the perceptions. The most simple-minded possibility is that the actions are providing the physical sensations of rhythmic rocking. So we would hold the child and do the rocking ourselves. If the child's muscles relax, we would know that this is what she was controlling for. The technical definition of a reference level can be stated as "that level of input at which output becomes zero." When you have what you want you stop trying to get it.

The question then becomes "how much rocking is enough?" For starters, if I were serious about finding out, I would be willing to guess anything up to 24 hours or until I dropped. Then I would start again, and go on until the child decided that doing something else was more important.
And what if the behavior is head-banging? Do we get a hammer and do it for the child? Obviously not -- but we might try some bumping with the rubbery side of a hand. We mustn't think that we're so smart that we can guess what another person is controlling for without some kind of test of the guess.

I think that the aim of any therapy based on control theory would be to give a person more and more control of things that matter to him or her. With an autistic child, it may be hard to figure out what the child wants and is trying to get. But when you start thinking as a control theorist, it may not prove as hard as it seems: you just have to notice the obvious. I've heard (with some astonishment) a parent say of an autistic child doing something bizarre, "Oh, he's just trying to get attention." What is wrong with giving the child attention? If doing so causes the bizarre behavior to cease, you know that this is precisely what the child wanted. The next thing is to see what the child does with the attention once he has it. I think you would find out something about the child's goals, and about what is preventing their attainment. When you understand what a person wants, you understand what that person is doing. And the more you understand of that other person, the more like you that person seems and the better you will be able to help.

I'm not trying to suggest how your daughter should be helped -- only to show how a person acquainted with control theory would make a start. Control theory can only help you understand what is going on; it can't predict what you will find. There may be problems with which you can only cope, not cure. But I should think that if you were to learn the principles of control theory and use them in interacting with your daughter, you would learn more than you know now and might well think of things to do that would not have occurred to you otherwise. At the least you would begin to think of your daughter (I'm sure you do already) as a person trying with great difficulty to accomplish things for herself instead of as an organism shaped by what is done to it -- the behavior-mod view. Despite its name and the awful engineering jargon that goes with it, control theory leads us to respect and nurture the individual will, to see others as sharing our own human condition, and to see the attempt to control others as the ultimate insult to the integrity of a living system.

I hope that control theory has something in it that will ease your plight.

Best regards,

Bill Powers

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Date: Sun Oct 06, 1991 5:29 pm PST
From: cam
Subject: A bike control anecdote

[From Chris Malcolm]

This is an anecdote which seems interesting from a CSG viewpoint. I'm a motorcyclist, and I recently fixed some problems with the bike which made it much easier to control the engine power. The throttle control had become rough and stiff, due to rust, and the twin cables to twin carbs had kept changing slightly and individually the amount of slack in each cable, so that the carbs had never been properly synchronised, and had kept varying slightly with respect to one another. I was unaware of any problems, just supposing it to be a rough old bike, until some other problem caused me to strip and rebuild the throttle. Finding these other problems, I fixed them.

I now find to my surprise that I seem to have much more attention to devote to the road and the traffic. This is a very marked effect. It is as though my consciousness has been enlarged. I am aware of more of what is around me, with less effort.

My hypothesis is that I used to have to pay some extra attention to controlling the engine. Because of variable stiffness of the system, odd bits of backlash, etc., it was not simple to control the engine. There was no simple relationship between muscular effort, throttle opening, and engine power. So the lower level control loops (working on such a simple relationship) would always be breaking down and needing repair (oops it seems to be stuck a bit, ah that's it, etc.). Now that I have fixed it, there *is* now a simple reliable relationship between muscular effort, throttle opening, and engine power. So the engine can now be controlled with a simple low level loop which doesn't break. Consequently I now have more attention to pay to other things. What surprised me was how noticeable the extra was.

Underlying this hypothesis is the assumption that the span of my conscious attention is a relatively fixed quantity which, in the performance of complex tasks, is consumed piecemeal by the number of awkward (breaking down) control loops I have to run. Yet I had never been aware that I was devoting any attention to this difficult throttle. Handling it was quite unconscious. Yet (if my hypothesis is correct) this unconscious activity was consuming some of what could otherwise have been devoted to enlarging the span of my conscious attention.
Comments?

Chris Malcolm, Dept of Artificial Intelligence, Edinburgh University.

Date: Sun Oct 06, 1991 6:16 pm PST
From: eprince
Subject: CT and autism

Dear Bill:

Your message is probably the most refreshing comment on autism (in quotations) and many other aspects of learning, freedom, ... that I have read in a long time. I plan to download it and print it out when I get to my office (and printer) tomorrow.

I cannot respond fully until I have a chance to really look at it, (I've only read it twice, quickly), but I need to (1) thank you for your time and concern: (2) let you know that I in now way subscribe to Bmod theory, but that I have found that to a limited extent it works in getting Katy to start to focus on learning and then, as I think you were saying, the task itself draws her in and becomes the goal. Also, the reinforcer they use is in fact far less important that setting up the expectation that she can succeed because the task is being given to her.

There is much more that I want to say, but I'm going to control myself now and sign off until I've had a chance to digest and think about your message.
Best,

Eileen

Date: Mon Oct 07, 1991 5:29 am PST
From: POWERS DENISON C
Subject: autism   [From Bill Powers (911006.2130)]

Chris Malcomb (911006) --

>Underlying this hypothesis is the assumption that the span of my
>conscious attention is a relatively fixed quantity which, in the
>performance of complex tasks, is consumed piecemeal by the number of
>awkward (breaking down) control loops I have to run.

A beautiful tale. This is clearly a phenomenon crying out for experimentation. What does consciousness/attention/awareness have to do with control? Why does consciousness seem to be dragged in whenever something complex is going on? Does complex mean "hard to control?" We ought to be able to measure the parameters of control for a given well-learned task, and see what happens to them when a second familiar task has to be carried out concurrently. I can see several things that might happen.

First, demanding attention for a second task might cause a deterioration of control in the first task. Second, it might not. Either way we learn something.

The unjustified hypothesis I've been working under is that consciousness becomes involved with control systems in which error is abnormally large. This brings reorganization to bear on the system having a problem. I want this to happen so that reorganization will be applied where it's needed instead of disrupting perfectly good control systems. There seems to be some subjective corroboration of this connection among error, attention, and reorganization. Your tale of the motorcycle gives strong anecdotal support to the idea.

This semi-hypothesis is meant to answer a long-outstanding puzzle: what is consciousness FOR? A model of a hierarchical control system doesn't need it: you can have everything from system concepts to intensities under control using nothing more than neural computing elements. All the perceptual signals will be there, in the brain or in a computer model of a brain, signifying what they signify; the only thing missing would be anyone at home to know that the perceptions exist. What is this wierd Observer than I am, that shares none of my other characteristics and adds the missing one? Is this what people have been trying to explain with words like "soul" or "atman?" Whatever the answer, the question still remains: what function does this thing serve in the otherwise mechanical system?

If the hypothesis is true, that awareness and reorganization and error are all connected somehow, a lot would become understandable -- for example, the kinds of things that go on on this net. What do people like us spend all our conscious time doing? Seeking out uncontrolled variables and trying to get them under control. Our consciousnesses are busy eighteen hours a day reorganizing our mental equipment, cleaning up the rough spots, trying to find answers to unsatisfied questions, trying to resolve conflicts and inconsistencies, trying to make beauty and harmony out of ugliness and discord. People who aren't lucky enough to have a safe comfortable place to do such things, or who haven't had enough education or experience to equip them to succeed at problem solving, do this vicariously. They read novels full of conflict and resolution, error and correction, disharmony followed by happy endings. They watch soap operas and game shows and sports. They go out on the streets looking for trouble, because trouble is the most interesting thing there is. If we can't find some to iron out with this powerful conscious error-correcting reorganizating system, we make some.

I'm reminded of Phaedrus' search for "quality" in Robert Pirsig's *Zen and the Art of Motorcycle Maintenance* (an image no doubt suggested by your tale of tuning your cycle). At one point Phaedrus likened the search for quality to the leading edge of the locomotive that drags along our whole train of consciousness, creating and shaping reality as it goes. The leading edge of human growth is reorganization, which carries our consciousness with it (or vice versa), leaving a trail of order through the chaos surrounding us, unceasingly creating and building realities, and going back to trim them up and try new limits.

If any of this poetry is true, we ought to find that when error appears in a control system, the parameters of control will pretty soon start changing at random, or rather in a biased random walk toward better values, e. coli-wise. As you suggest, there is probably a limited capacity for reorganizing many systems at once, or many dimensions of a single complex task at once. Objective evidence of reorganization should be accompanied by subjective awareness of error: attention should go immediately to the part of the task in which control has run into a problem. It's easy to change the external part of a control loop so as to require reorganization if successful control is to continue.

David McCord (Western Carolina U) was going to do something like this last year, with my help, but I backed out because of an overload. David, are you still there and are you still interested? If not, who else has facilities and available subjects? This is an easy experiment that should provide lovely data and clearly publishable results. It wouldn't hurt to have ten people doing this in parallel; this is such an important phenomenon that we can't have too much independent replication.

Thanks, Chris.

Best to all

Bill P.

Date: Mon Oct 07, 1991 6:30 am PST
From: Bruce E. Nevin
Subject: complexity, attention, autism [From: Bruce Nevin (911007 1000)]

Chris Malcolm (Mon, 7 Oct 1991 02:23:00 BST)

>[Hypothesis] . . . that the span of my
>conscious attention is a relatively fixed quantity which, in the
>performance of complex tasks, is consumed piecemeal by the number of
>awkward (breaking down) control loops I have to run. . . .
>this unconscious activity was consuming some of what could otherwise
>have been devoted to enlarging the span of my conscious attention.

Bill Powers (911006.2130)

>What does consciousness/attention/awareness have to do
>with control? Why does consciousness seem to be dragged in whenever
>something complex is going on? Does complex mean "hard to control?"

I am taking it as significant, Bill (maybe perversely), that you made "autism" the title of this response to Chris's very thought-provoking post, though you said nothing more about autism. Maybe it was just the juxtaposition of Ellen's query and your reply with Chris's account in the same morning batch of mail, but I came up with a hunch that the difficulties of some children called autistic have to do with this complexity/control nexus, and inability to bring attention to focus in such a way as to trigger (direct?) reorganization.

Someone a while back was talking about complexity of tasks in a cockpit. Bill Cunningham? Martin Taylor?

Bruce Nevin

Date: Mon Oct 07, 1991 10:37 am PST

From: jbjg7967
Subject: MacArthur [from Joel Judd]

My [PCT] belief systems survived a weekend among applied linguists. As soon as I can synthesize some of the key points raised at the conference, I'll be posting questions and comments about where the field seems to be heading. For the size of the conference they had some really big guns there and all spoke on the nature and role of theory. It was fun to see them coming around to real philosophy of science issues. I've never heard Kuhn and Popper mentioned (with such abandon) at a Second Language conference before. More importantly, I got the feeling that by naming the right names and using the right words, there's a real possibility of getting the PCT viewpoint published in a forum where it will be read by large numbers of people; the time seems to be right.

I'm anxiously awaiting a report from one of the presenters, Russ Tomlin (U. of Oregon--anyone know him?), who's been combining research on perception and attention with language learning for several years now, and who described to me work which appears to be an unequivocal demonstration of linguistic perceptual control. Virtually 100% of the subjects responded to the task as predicted, and cross-lin guistic subjects also responded with whatever the equivalent form was in their L1. He said he knows nothing about Phillip Runkel, nor has he read _Casting Nets..._, but he is convinced that studying the individual language learner is the way to go.
And not to be selfish, but because I like to take credit for my own cynicism, the "damned if I do" statement was mine. So back to work.

Date: Mon Oct 07, 1991 12:45 pm PST
From: marken
Subject: color me naive [From Rick Marken (911007)]

Bruce Nevin asks, with respect to my argument that the "marginalization" of control theorists comes as much from being ignored by conventional researchers as it does from from us ignoring them:

>Here is a problem that bears exactly on the problematic social aspects
>of CT. What perceptions are they controlling, that they ignore and
>distort real-time perceptions (e.g. your words) and resist them as
>disturbances? What practical techniques for communication, teaching,
>and persuasion have been or can be developed from CT and applied
>reflexively to the promulgation of CT itself?

Great question. And I have a hypotesis about what variable conventional psychologists (of virtually all stripes) are trying to control; the perception that they are able to have relatively (statistically) predictable effects on what other organsisms do. Not surprisingly, the behavior of other organisms is, from the point of view of a psychologist, a controlled (or potentially controllable) variable. Even in cognitive psychology this holds, I think. I used to do some research on visual search. Nearly all of this work is aimed at trying to find factors that affect the rate of search -- such as similarity of target to background, statistical properties of the back-ground, and so on. You can find things that have pretty strong STATISTICAL effects on search rate. So you can control search rate (or, at least the average rate) by messing around with the background. To the extent that you get the effects you want in your study (effects that match your reference) then you are happy. The experimenter is typically more concerned with his own ability to control what happens, then in the organism's ability to do so.

Some time ago (like when CSGNet began) someone suggested a "naturalistic" experiment to test what conventional psychologists are controlling for which makes them avoid or distort control theory. The experiment simply involved hypothesizing a controlled variable and then testing this hypothesis by looking over the reviews of various papers (I have many reviews in my file) and seeing whether negative comments about the paper pertain to ideas in the reviewed paper that would be a disturbance to the controlled variable.

Re: Bruce Nevin's "So Near And Yet So Far Award".

It so happens that I knew both Premack AND Gazzaniga (both taught at UC Santa Barbara when I was getting my PhD there). So, although Premack (and Gazzaniga) missed the boat, one of their students didn't (me, of course).

Gary Cziko replies to Wayne Hershberger:

>But your response is relevant to the "marginalization" discussion that has
>been going on. If Robinson work is so highly regarded and if he is a
>"first-rate control theorist" then he must be publishing in places that are
>receptive to this model of behavior. Or is this type of research itself
>"marginalized" within psychology? I suspect that perhaps your idea of a
>"first-rate control theorist" is different from Rick Marken's. Does
>Robinson look at controlled variables? Does he construct models of working
>systems? Or is it an adaptation a la Carver and Scheier of control theory
>to fit the traditional mold of psychological thinking?--Gary

I suspect that Robinson is a "first rate control theorist" in the same sense that my old "manual control theorist" friend is a first rate control theorist. I looked at some stuff on oculomotor models over the weekend and they discussed Robinson's work. My impression is that Robinson's model of oculomotor control is very much like the "manual control theory" models of manual control. They get the closed loop control equations right; they know how to work with the differential equations that define the dynamics of control. They just ignore a few issues that PCT type control theorists think of as exceptionally important:

1) there is no attempt to test the definition of the controlled variable. It is just assumed that THIS (position of a line, angular displace-ment of the eyeball, whatever) is what needs to be controlled, so we will make up a control system that can do it and

2) there is no variable reference for this variable "inside the model". The refernce state of the controlled variable is placed outside the system by incorporating it into the definition of the stimulus (ie -defining the stimulus as distance from an external target).

I have just scanned Wayne's description of the Robison model in his paper on visual direction constancy. It looks like Robinson's model does use stimulus-target discrepencies as inputs. So I stick by point 2. I have a suspicion that I'm right about point 1 also.

There is nothing wrong with Robinson's approach to applying control theory, except that it does tends to narrow one's view of the applicability of the theory to small behavioral subsystems. The big point of PCT is that ALL purposeful behavior, from moving your eyes to moving to a new church, involves the control of perceptual input. So it's true that there have been some "main stream" studies based on control theory. But they have been main stream because 1) they treated small subsystems of behavior in input-output terms and 2) they ignored the big picture, that all behavior (well, purposeful behavior) is the control of perceptual variables.

Wayne: To the extent that I am wrong about Robinson and he understands oculomotor control as control of perceptual variables, then I would really like to know about it. Just tell me where is the reference signal in his model and what is the source of that signal.

Hasta Luego

Rick

Date: Mon Oct 07, 1991 1:22 pm PST
From: Gary A. Cziko
Subject: Indirect Causation   [from Gary Cziko 911007.1530]

Rick Marken (911007) says about non-PCT control theorists:

>The refernce state of the
>controlled variable is placed outside the system by incorporating it into
>the definition of the stimulus (ie -defining the stimulus as distance from
>an external target).
>
>I have just scanned Wayne's description of the Robison model in his paper
>on visual direction constancy. It looks like Robinson's model does use
>stimulus-target discrepencies as inputs. So I stick by point 2. I have a
>suspicion that I'm right about point 1 also.
>

>Wayne: To the extent that I am wrong about Robinson and he understands
>oculomotor control as control of perceptual variables, then I would really
>like to know about it. Just tell me where is the reference signal in his
>model and what is the source of that signal.

Rick, I think you've put your finger on a crucially important point. Control theory can be made quite compatible with traditional approaches to psychology if the reference levels are seen as being determined by the environment. But as I understand PCT they are not determined by the environment, although they may certainly be influenced by the environment during reorganization.

I wonder if we need another type of causation in PCT to help get this idea across. We all know what CIRCULAR causation is, but what about INDIRECT causation. Don Campbell talks about this. Let me give an example.

In biological evolution, the environment has a very important influence on the course of evolution of a species. But this influence is indirect in the sense that the environment only SELECTS those variations which have a better fit to the environment. The variation in phenotype (caused by variations in genotype) are caused by the organism. While changes in the environment might well change the rate of variation (e.g., radiation resulting in an increased mutation rate), the environment does not directly cause adaptive variations to appear. So while the toad may carry a "picture" of the tree bark on its back (for camouflage), this picture is not a photo of the tree bark. The organism is not a Lamarckian camera. It must generate the patterns similar to the tree bark (along with many which are not) and the environment (composed of both tree bark and tree toad predators) does the selection. This is INDIRECT causation. The environment influences, but does not determine. So even in biological evolution we see the organism as an autonomous, creator of the variations which serve as the fuel of evolutionary change and adaptation.

Isn't this what reorganization is all about? The environment certainly does play a role, but it is not a simple direct one. The environment's role is limited to
(a) causing chronic error signals which starts reorganization and
(b) providing the context within which new, reorganized control systems are selected.

Now, what is deceiving is that certain environments do tend to lead to the same type of reorganization. For the most part, kids do learn to read in school (at least middle-class, suburban ones do) and so it looks like the environment of the school (teacher, books, etc.) determines this reorganization. And if kids in inner-city schools don't learn to read, the traditional direct causation view makes us look for those environmental factors which are responsible for learning to read so that they can be "applied" to the problem kids and, voila, they are reading whether they like it or not!--Gary

Gary A. Cziko

Date: Tue Oct 08, 1991 5:18 am PST
From: goldstein
Subject: PCT and autism

To: Eileen Prince
From: David Goldstein
Subject: PCT and autism
Date: 10/07/91

As a clinical psychologist, I have worked with some autistic people in special education settings, developmental centers and one child in my private practice. As a parent, I have the experience of a child with special needs.

However, I do not present myself as an expert on autism. Based on my experience with autistic people and interest in PCT, I wanted to write an answer to your post.

In my opinion, Behavior Modification is the most useful psychological approach that I know about at the present time for helping autistic people to learn. At some point in the future, PCT may make a contribution to this area. At the present, I would not look to PCT as a source of new assessment or treatment. It may, however, provide a new set of ideas which you could apply when observing your daughter. A new assessment or treatment method could follow from understanding your daughter better.
I would like to refer you to the following book for helping autistic people develop language:

The Autistic Child: Language development through behavior modification by O.Ivar Lovass. New York: Halsted Press, 1977.
There may be an updated version of this book by now. This book contains specific instructions for specific language objectives.
I am sure that at your daughter's school they must employ the services of a speech and language pathologist. These are the professionals who have the most practical knowledge about teaching language to people who have not acquired it
spontaneously or who have lost it through stroke, accident, etc.. As in other professions, not all speech and language pathologists are equal. You have to compare what you want from them with what kinds of experience they have actually had.

Also, the use of American Sign Language has been found to be helpful in some cases. I think that you mentioned you are aware of this. One reason is that it is possible for another person to help produce the sign while this cannot be done with the spoken word. This does require you to learn the signs as your daughter learns them. Some parents have a negative attitude towards it. I think that ASL can help the autistic person learn some general things about communication. However, your daughter may be well beyond this point.

I second what Bill Powers said about applying Perceptual Control Theory to autism. If you start with some easily identifiable actions and ask the quesiton of what experience this action is controlling, you may learn something new about your daughter. Don't forget that some of the experiences may refer to body states, such as fear, as well as states of the environment. Make a guess about what experience is being controlled. Do or say something which should alter the experience which you guess is being controlled. Observe whether your daughter's action restores the experience which you disturbed to its original state. In PCT, this is procedure is called the Test for the controlled variable.

Here is a slightly different approach. If you find out what kinds of experience your daughter can and cannot control you will be in a better position to understand and possibly help her. The levels of perception are a good starting point. Using the levels of perception, ask yourself to think of examples where she demonstrates the ability to control at each level. If when thinking about a level, you cannot come up with any examples, then perhaps she cannot control at that level at this time. This would be a starting point for focusing reorganization efforts. It takes a while to grasp the different perceptual levels. The people on CSGnet would, I am sure, be glad to help you learn the levels.

Date: Tue Oct 08, 1991 5:31 am PST
From: Bruce E. Nevin
Subject: control and influence

[From: Bruce Nevin (911008 0742)]

Rick Marken (911007)

>1) there is no attempt to test the definition of the controlled
>variable. It is just assumed that THIS . . . is what needs to be
>controlled, so we will make up a control system that can do it and
>2) there is no variable reference for this variable "inside the model".
>The refernce state of the controlled variable is placed outside the
>system by incorporating it into the definition of the stimulus. . . .
>There is nothing wrong with [this] approach to applying control theory,
>except that it does tends to narrow one's view of the applicability of
>the theory to small behavioral subsystems. . . .

>Just tell me where is the reference signal . . . and what is the source
>of that signal.

Peter Cariani, what is your treatment of the reference signal?
Gary Cziko 911007.1530

>Rick, I think you've put your finger on a crucially important point. . . .
>reference levels are seen as being determined by the environment. But
>. . . they are not determined by the environment, although they may
>certainly be influenced by the environment during reorganization.

This distinction between causation and influence corresponds to the distinction between hierarchical control within an organism and social influence between organisms. Control/causation is deterministic in individual cases, influence is only statistically predictive. It is so because influence depends crucially on what the responding organism "makes of it" for its effect, and the details of the higher levels of control vary from one control hierarchy to another. For this reason, calling it "causation" (even "indirect" causation) is a misleading metaphor.

>Isn't this what reorganization is all about? The environment certainly
>does play a role, but it is not a simple direct one. The environment's
>role is limited to (a) causing chronic error signals which starts
>reorganization and (b) providing the context within which new, reorganized
>control systems are selected.

Does the environment cause the error signals? No. A perceptual input path, a comparator, and a reference signal play equally important roles, and since all of these are within the organism I think we must say that it is the organism that causes the error signals by preferring that a perception be one way rather than another. Does the environment cause the selection of a more "fit" alternative control system? No, the organism does, by stopping the internal reorganization process.

I think "indirect causation" is a misleading metaphor. I think the point to be made is that there is *no* causation between environmental stimulus and an organism's response, and in particular *no* causation between the behavior of one organism and that of another, only influence. There is no causative determination of outcome, only statistical "prediction" of likelihood.

I think that to make this point we might say that causation is defined such that "A causes B" means "If A, then B, 100% of the time, no exceptions."

The task in focus then is to demonstrate that the problem does not lie with uncontrolled variables, and that the results would rise to 100% if only we could have more rigorously controlled experimental conditions. Because even with the abysmal record of psychological experimentation I think this is what psychologists believe. We all grew up believing that our circumstances and particularly other people made us feel feelings and do things, and trying very hard to make other people do things as we wanted. "You made me mad!"

Bill Powers (911006.2130)

>What do people like
>us spend all our conscious time doing? Seeking out uncontrolled variables
>and trying to get them under control. Our consciousnesses are busy
I know you had something else in mind here, and I am ripping this comment out of context, but I think the generalization is valid to our commonplace conviction that we control one another.

I want to say something here about how this desire for control is what underwrites research funding, since people with a strong need for control frequently attain positions of economic power and influence and want researchers to tell them how better to maintain control. How people who tell them that their control is illusory and their efforts at it counterproductive are not welcome. How children in families of such people are taught by example and some (but not all) determine fiercely that they will not be subject to another's control, and set out to be "winners" rather than "losers," and other poppsych spinoffs. And to connect this to the importance for these people of controlling the social promulgation of "accepted" reference values for higher levels of control, as Peckham rightly points out. But I haven't time.

Bruce Nevin
bn@bbn.com

Date: Tue Oct 08, 1991 9:45 am PST
From: POWERS DENISON C
Subject: Real control theorists; evolution

[From Bill Powers (911008.0700)]

Loose ends:

Bruce, why did you make me give you credit for Joel Judd's "damned if I do" statement?

I can tell you what made me title the reply to Chris Malcomb's post "autism:" a lapse of 12 hours, during which I forgot which of my transmissions didn't get sent because of a chronically busy line. See the Control Theorist falling apart before your very Eyes.

Rick Marken, Gary Cziko (911007) --

Excellent thoughts about why "first-rate control theorists" have missed at least our boat, if not THE boat. The problem with putting the goal into the environment is that IF you have correctly identified the controlled variable and IF you have correctly evaluated its reference state, *the model will work properly.* All the equations will work, and you will get correct predictions of behavior just as good as any we have ever come up with. It isn't hard to identify obvious controlled variables and their reference states, if the reference states remain reasonably constant. The real question is how you explain their existence, and how you explain changes in the reference state.

With the reference condition defined in the environment, you are stuck with an unexplainable puzzle: why was exactly that quantitative target state the one that was effective? In visual tracking in particular, you have to explain not just why the subject looked at the target, but why the reference position for looking was a particular spot on the target defined with a precision of a couple of minutes of arc. Why THAT spot and not the one next to it in any direction? Why not a spot off the target? You also have to explain why this spot keeps jumping around.
Projecting the reference condition into the environment leads to asking why that particular state is "salient." What gives it the "stimulus value" or "goal value" it seems to have? What is "rewarding" or "reinforcing" about it? What makes it into an "attractor?" The physical world starts acquiring some very strange properties when you do this. It starts to get metaphorical. You inevitably start down all those false trails we see in the conventional approaches.

The effect of saying that the system perceives error is to put the comparator into the sensor. Because the input signal is now the error signal, your model doesn't need an internal comparator, so of course it doesn't need an internal reference signal. This is why this view "tends to narrow one's view of the applicability of the theory to small behavioral subsystems." There is no way for higher systems to set the goal for the control system. The control system becomes a self-contained entity unconnected to anything else in the organism.

The worst effect of hypostatizing reference signals, therefore, is that you are prevented from discovering the hierarchical control model. It's been almost 40 years since I started on this path, but I seem to have a memory of making this very discovery -- I can catch an echo of the big AHA when I realized, thanks to a remark by Kirk Sattley, that a reference signal could be the output of a higher-level system. This led immediately to seeing that the perceptual inputs only report the actual state of the external world, not the reference state as well (in those days, "actual" meant something different to me; I wasn't thinking about epistemology). Shivers up the spine! Goals are inside us, not outside us! It wasn't long after that that Bob Clark and I led each other to realize that we control perceptions, not the names of perceptions or the reified correlates of perceptions. That put the controlled variables inside, too, and we were on the way.

There's a tremendous Gestalt change in seeing that goals are specified internally, not externally. If all those first-rate control theorists could just make this one little switch, they could reinvent the rest of HCT without further prompting. How can we get them to do this? And how are we ever going to get biochemists to do this? They don't even like the concept of reference signals and I'm sure they have never thought of a biochemical system as controlling its inputs.

Dag Forssell (911007) --

You have just elected yourself to a duty which, if you fail to perform it faithfully and unerringly, will lead to hurting innumerable feelings. Bet you didn't think of that.

So anyway, Happy Birthday, Wayne. Mine is August 29. I'll be waiting.
Gary Cziko (911007) --

Re: INDIRECT CAUSATION

>the environment only SELECTS those variations which have a better fit to
>the environment.

While I agree with the basic premise here, I think the concept of "better fit" is concealing a problem. To say that the environment selects for a better fit is to give the environment a reference level: it wants organisms to fit it better. Of course the environment doesn't give a hoot whether a species exists or not. The environment actually does no "selecting." It simply sets the stage on which evolution plays itself out. Evolution, I am more convinced than ever, is a process carried out BY ORGANISMS. Rocks don't evolve.

The problem in coming up with a model that shows how organisms manage their own evolution (given the environment that exists) is to figure out how the loop is closed. It clearly can't be closed by a single organism. It's got to be a property of a population of organisms, and it has to be carried out at the level of DNA, where the states of control systems can be preserved from generation to generation. The critical events have to take place during replication. All I have now is a vague idea, but something in here is saying that there's a solution in control-system terms. We know how a systematic effect can be obtained by an organism that varies the timing of a perfectly random output process. We need to figure out what the controlled variable is, such that it can be sensed and compared with a reference state, so that the error can vary the rate of mutation. Once we have that, I think that the rest of the problem will quickly unravel itself.

Best to all

Bill P.

Date: Tue Oct 08, 1991 10:17 am PST
From: eprince
Subject: PCT and autism

Thank you for your message, which I will be downloading, printing and fully responding to soon. Just to say that your assumption that the school (which is not a bad one) employs experts in language is unfortunately not really correct. Yes, they employ a Ph.D. in communication who truly believes that she knows all there is to know about helping autistic people to communicate (note: not to use language of any kind) and, most unfortunately, that she knows what their limitations are. Basically, her approach is to say that if something has not been documented to work with a child at Katy's "level of functioning" then she will not try or encourage its being tried either. This is, as I have said, very unfortunate.
To be continued, and thank you again.

Eileen

Date: Tue Oct 08, 1991 1:41 pm PST
From: marken
Subject: PCT, behavior mod and autism

[From Rick Marken (911008)]

David Goldstein (911007) writes:

>In my opinion, Behavior Modification is the most useful
>psychological approach that I know about at the present time for
>helping autistic people to learn. At some point in the future,
>PCT may make a contribution to this area. At the present, I would
>not look to PCT as a source of new assessment or treatment.
I hope the behavior mod types are as generous to us in the future.
>I would like to refer you to the following book for helping
>autistic people develop language:

>The Autistic Child: Language development through behavior
>modification by O.Ivar Lovass. New York: Halsted Press, 1977.
Boy, this is like "ol' profs week" on CSGNet. I took child development from O. Ivar when I was an undergrad at UCLA. I was very impressed. Finally, a psychologist who could really get something done. And with children yet: children who really needed help.

Of course, I know better now. Actually, I knew something was wrong even back then, but I had to learn control theory before I could articulate it.
Behavior mod is a great example of control in action. The behavior modifier (bm) has a reference for the kind of behavior he/she wants to see. There is a discrepency between this reference and the actual behavior (the bm wants to see speech but what he/she perceives is silence). So the bm does things which should produce speech -- these are the tools of the behavior modification process. One tool is withholding something that the child wants (finding out what this is is usually the toughest part of the process). Then the bm waits for "approximations" to speech and gives the reward only when the desired (reference) approximation accurs. It's alot like steering a boat into a slip using only the motor. The boat just drafts around, it has no rudder. The direction of the boat depends on local currents and the amount of puch given by the engine. You (the bm) can control the boat by giving it power (reward) when it is pointing in the "right" direction. You withhold the power (reward) when it starts to point away.

There can be no simple rules for doing either task (training the kid or docking the boat) because the effect of your actions depends on other factors that are operating at the same time (disturbances). For example, gunning the boat when you are oriented 20 degrees left of slip may push you even further to the left if the current is also pushing the boat left. The same acceleration might move the boat right into the slip if the current is pushing the other way. Same with the kid. Rewarding a particular sound (like "hel") may lead to a repetition of "hel" that sounds more like "hello" (the reference word) or to no repetition or to a new sound completely. So there can be no rule in Lovass' book that says "In order to get the kid to say "hello" always reward him/her after saying "hel" unless he she has previously made a closer approximation to hello". Control can't work this way. That is, you cannot generate a particular response to a particular stimulus. Instead, you must continuously adjust your actions (rewarding -withholding or turning the engine on and off) to keep your perception (of what the kid is saying or where the boat is going) approaching your ultimate reference for what you want to perceive.

The problem with the behavior mod approach to dealing with organisms (ALL ORGANISMS,including those that have been catagorized as "autisic") is that it ignores the fact that the organism being controlled is ALSO a controller.
Behavior modeification is a recipe for almost certain conflict between control systems (the bm and the controllee). "Almost certain" because most bm's relaize that there is a problem when they start to get into conflict with the controllee. I think the bm is usually willing (if they are even close to being human) to readjust their references for what they want to see the controllee doing. Even Lovass said ultimately that his success depended on having the following attitude towards his autistic children .. "load 'em up wit luv" (he has a charming norwegian accent).

So I am reluctant to recomend behavior modification as the best current approach to dealing with so called autistic children. The reason is that some people might actually blindly follow its precepts -- which could be quite ugly for both parent and child.

Best Regards

Rick

Date: Tue Oct 08, 1991 2:36 pm PST
From: eprince

Subject: PCT, behavior mod and autism

Subject: Richard S. Marken's comments on BMod

"So I am reluctant to recommend behavior mdoification as the best current approach to dealing wtih so called atutisitic children. The reason is that some people might actually blindly follow its precepts -- which could be quite ugly for both parent and child."

I couldn't agree more. The problem (at t least the main one I've had with BMod practitioners) is with blindly following and not thinking or considering alternative approaches. And, although accountability is probably higher than with other approaches (others that I know of), there is still the ultimate fudge, as with their failure to deal adequately with my daughter's water and bathroom recurring phobia - - th4e fudge is that it must be something in the environment (the antecedent) that has set the phobia in motion, but unfortunately we don't know what it is because it must be very subtle....

Eileen Prince

Date: Wed Oct 09, 1991 5:46 am PST
From: LO BOUR
Subject: Re: Robinson's oculomotor model

I myself am a physicist and involved in oculomotor research for already 10 years. I picked up the discussion about the Robinson-model and want to add now some comments. In order to make the discussion not more confusing than he already is, as an 'expert' I would like to state some points.

The Robinson model is a schematic and mathematical framework in order to explain oculomotor control of saccadic eye movements in the brainstem. The model makes use of a local feedback loop in order to control the end position of a saccade.

The model is tightly linked to neuronal substrates known to exist in the brainstem.

Some attempts have been made to modify this model, since some experiments cannot be explained by the model. Jurgens has introduced an error signal not in head-coordinates but in retinal coordinates. Scudder has introduced a kind of velocity feedback and emphasized the role of long-lead burst neurons.
Van Gisbergen has extended the model to two directions (horizontal and vertical). At the moment a discussion is going on with respect to the 3D representation between different investigators such as Tweed and Van Opstal.

Anyway the Robinson-model has been for a long time been a leading model for the explanation of the burst generator in the brainstem.

Lo J. Bour PhD.
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E-mail: Bour@amc.uva.nl

Date: Wed Oct 09, 1991 6:07 am PST
From: POWERS DENISON C
Subject: Behavior mod

[From Bill Powers (911009.0730)]

A quick addition to Eileen Prince's and Rick's responses to David Goldstein. Rick's image of trying to dock a boat using only the engine (and not the rudder) is poetically apt. The rudder -- the steersman -- is responsible for direction, while the engine only pushes. In behavior mod, a great deal of credit is taken by the bm that belongs to the subject. The bm is only a cheerleader who gets the sensation of controlling the game by shouting "Yay, that's it, good going, way to go, swell fella, nice try" while all the effort and sweat and thinking is happening out on the field. If the cheerleader happens to be a millionaire benefactor of the team (i.e., is in a position to give something the team needs), the team may try to satisfy the benefactor by guessing what he wants to see happening. But of course that might mean losing the game.

Unless the client has some reason to want to please the bm, behavior modification can't work. And even when it does "work," what is really working is the client. Suppose, David, I came up to you and said "Nice going, Dave." You might be pleased, but you might wonder just what it was you did. So you have to guess. Sometimes I say "nice going" and sometimes I don't say anything. That's pretty slim guidance. It's harder than "20 questions" or "hot and cold." Which one of us is doing the hard mental work? You may end up getting me to say "nice going" pretty frequently, without having any idea what is causing me to say it. Something in what you are doing is pleasing me, and I may have the impression that you are deliberately and consistently doing it when in fact you are doing something else that only has the side effect of showing me what I want to see. It would be comical if it weren't often so serious. If the behavior modifier doesn't understand what variable the client is controlling, the behavior modifier doesn't know what the client has actually learned to do, if anything.

When you say that behavior modification is "the most useful approach," that may or may not be saying much. How useful is the most useful approach? If all it does is give behavior modifiers an illusion of success, it isn't very useful. And if we always prefer the most useful approach (regardless of the absolute amount of usefulness), how are we ever going to develop a more useful approach? Falling back on the "tried and true" may be comforting in that at least one isn't going out on a limb and can cite precedent. But what if the tried isn't true?
Best

Bill P.

Date: Wed Oct 09, 1991 2:10 pm PST
From: marken
Subject: Re: Robinson's oculomotor model

[Rick Marken (911009)]

Lo Bour writes:

>I myself am a physicist and involved in oculomotor research for already
>10 years. I picked up the discussion about the Robinson-model and want
>to add now some comments. In order to make the discussion not more
>confusing than he already is, as an 'expert' I would like to state
>some points.

Welcome Lo. It's nice to have another expert on oculomotor models on the net (I'm not one, by the way. I think Wayne Hershberger qualifies for that honor). Knowing as little as I do about it, perhaps I could take the liberty of asking some questions about the points you posted so that I could learn more about the state of our understanding of oculomotor control.

First let me ask what is the model trying to explain? Is it the ability to control eye position? Do we control eye position? Or do we control the consequences of eye position (retinal events)? Or both?

You say:

> The model makes use of a local feedback loop
> in order to control the end position of a saccade.

What is a "local feedback loop"? Does this mean that the loop does not go through the environment? Maybe a little diagram of the model would help.

> Jurgens has
> introduced an error signal not in head-coordinates but in retinal
> coordinates.

Is the error signal an input to the control system? Oris it the difference between a reference signal for retinal position and a perceptual signal that represents the position of a target on the retina? If it is the latter, what does the model say is the source of the reference for retinal position? If it is the former, how does the model change eye position in order to look at moving targets?

> Anyway the Robinson-model has been for a long time been a leading
> model for the explanation of the burst generator in the brainstem.

What is the "burst generator in the brain stem"?

Any help from Wayne or Bill Powers (I know you have the definitive eye control model sitting in your files somewhere) would also be most welcome.

Best Regards

Rick

Date: Thu Oct 10, 1991 4:31 am PST
From: POWERS DENISON C
Subject: Oculomotor models [From Bill Powers (911010)]

Lo Bour (911009) --

Thanks for the first of what I hope will be continuing comments from you -- and welcome to our conference.

Something like 10 or 12 years ago (?? Wayne ??), Wayne Hershberger brought some of the literature of oculomotor control to my attention and (whether he intended to or not) challenged me to make sense of it in terms of my version of control theory. In the course of this attempt I saw some of Robinson's work, and thought it was highly competent. It didn't seem that there was much I could add at the same level of analysis, so I looked at a more general model in an attempt to see how some of the standard oculomotor illusions might arise from the properties of possible control systems. The model I came up with, while purely speculative, did seem to be able to reproduce many details of these phenomena.

Unfortunately, my modeling efforts ran into what seemed an insuperable obstacle. This quite beautiful (even if partly fanciful) model depended in large part on the existence of stretch receptors in the eye muscles. After putting in a lot of work on the model I found belatedly that there was considerably doubt as to whether such stretch receptors exist. Perhaps there is a way to get around this problem without junking the entire model, but I haven't yet returned to this problem (or caught up with the current literature) and so do not know if this would be possible. Perhaps if I explain why the stretch receptors were so important, you will see some applicability of the basic ideas and see a way to save the model (or at least see something of use in other models).

The basis of the kinesthetic (non-visual) part of this model was a straightforward rendition of the combined stretch and tendon reflexes, with the lateral rectus muscles and associated neural systems being considered as a balanced pair of systems.

The feedback relationships proved to have an interesting property. The dual tendon signal, representing unbalanced force applied to the eyeball, could vary independently of the dual stretch signal indicating deviation of the length of the muscles from the average value of length. As a result, it was possible to alter the reference signal indicating intended direction of gaze, and have the stretch feedback signal follow it faithfully, even though the eyeball was not allowed to move. This provided for the illusion that makes a scene seem to move when the eye tries to turn against resistance. Furthermore, by making the stretch reference signal (a gamma efferent) into the output of the *pursuit* tracking system, it was possible to show that the stretch feedback signal behaved just as the subjective sense of direction of looking seemed to behave, under external mechanical disturbance and under the influence of the vestibulo-optical reflex.

Interestingly, the combined feedback effects from tendon and stretch receptors could, with reasonable values of constants, be made to nearly cancel out, essentially negating any active resistance to mechanical disturbances in the dark (as is observed). This result came from assuming rather high gains in both control loops, contrary to my first intuition. The chief effects were to separate reflex adjustments of eye position from the subjective sense of direction of looking (identified as the stretch signal, after a good deal of puzzlement). Actually, at this moment I'm suddenly not sure which signal was the subjective looking direction signal, but let that go...

This house of cards, of course, fell apart with the discovery that there are no stretch receptors. Perhaps there are equivalent circuits that will give the same result, using perhaps some "reafference" principle. But until this problem is either bypassed or otherwise solved, I haven't wanted to go into print with the model, and it sits on the back shelf. I have used the same principle in my "little-man" model of pointing behavior (stretch receptors certainly exist in arm muscles), but haven't gone on to look into this separation of automatic or "reflex" action from voluntary action. It would be interesting to see if there are any analogous illusions involved in kinesthetically-controlled pointing behavior.

One idea came out of all this that may have some validity independent of my particular model. I was struck by some data concerning saccades that take place during pursuit tracking. The saccade moves the eye quickly from one place on the moving display to another place, and pursuit tracking instantly picks up where it left off, with the eye locked onto a new location. Clearly there is an implicit conflict between pursuit tracking and the positional control involved in a saccade. Why doesn't the pursuit system resist the saccade? If the pursuit and saccade systems are independent, the beginning of a saccade should induce a huge velocity error, which the pursuit system should try to correct.

One answer seemed to lie in the visual blanking that was suggested to take place just prior to and during a saccade. Wayne and his student Scott Jordan, however, have some recent and rather compelling evidence that this blanking does not occur, at least in a situation where pursuit tracking isn't involved; it is clearly not blanking of the retinal signal, because movement of the direction of gaze can generate a series of visible images of a rapidly blinking light that is on only just before and during the saccade. So if there is any blanking, it must occur at a different functional level.

My original idea on this problem of potential conflict was that the pursuit system might simply be turned off during a saccade. This interpretation was strengthened by the properties of the control system mentioned above: the feedback relationships allowed the stretch (or was it tendon?) signal, which could circumstantially be interpreted as the basis for the sensed direction of gaze, to track a changed intended direction of looking, all without the eyeball actually moving. So the reference signal for intended direction of gaze could be adjusted to the position of an offset target, and the sense of direction of looking would change accordingly, while the pursuit system still held the eye locked into its former position (acting just like an external constraint). Then, when the pursuit system was turned off, the feedback effect from the stretch feedback would be lost, and the eye would snap to the intended position. Turning the pursuit system on again at that point would restore the lock to the moving background and again permit the intended (and sensed) direction of looking to change within the scene. This particular relationship might remain valid even if my original model doesn't work.

All of this came out of the simple feedback relationships in a neat and convincing way, so you can see how disappointed I was at finding a major premise to be false. But the fact that the model is on shaky ground doesn't take care of the potential conflict that showed itself during the development of the model -- that, at least, still requires explanation.

Another possibility has since occurred to me. If the pursuit-tracking system has, as it ought to have, an integrating output function that drives the eye at a constant rate for a given constant pursuit error signal, a higher system could insert a saccade simply by adding a momentary signal to the error signal: in short, by briefly changing the reference signal for relative rate from zero (tracking) to non-zero (moving relative to the target at some specified rate). This removes the conflict without any ad-hoc switching mechanism. The pursuit system supplies the motive power to the eye in either case: pursuit or saccades. This arrangement makes the saccadic position-control system into a higher level of control than the pursuit system. To sum up the proposition:
The output of the saccade-generating system sets the reference signal for the pursuit system. When the saccade system is not switching the eye to a different position relative to the target, its output signal is zero, setting a zero reference level for the rate at which the eye is to be moving relative to the target. This leaves the pursuit system changing eye position so as to maintain zero angular velocity relative to the target. With a stationary target, of course, the pursuit error is zero and the eye is stationary.

When the higher system receives a reference signal telling it to move the visual scene to a new position relative to the direction of looking (the control-of-input way of saying this), the error signal is converted to a brief burst that amounts to a briefly non-zero reference signal for the pursuit system. The amplitude and duration of this burst determines for how long the pursuit system is to maintain a non-zero angular velocity relative to the target. I understand that saccades take place at pretty much a constant angular velocity, so we can assume that these bursts have a constant average amplitude (i.e., rate of firing during the burst). The pursuit system moves the eye by some distance, and then receives a zero reference signal again, so it stops the angular movement of the scene relative to the retina (by stopping the eye movement). Thus whether the scene is stationary or moving, the mechanisms of pursuit and saccadic eye movement remain exactly the same, and there is never any conflict between these processes. This method of (sampled) control is not very exact, so we can expect that several saccades are needed to establish the correct new location of the fixation point. Of course the facts fit our deduction.

I think this is a plausible model (and for all I know it has already been proposed, as I haven't followed the literature for some years). With proper choice of constants it should be perfectly compatible with Robinson's analysis of eye dynamics. It has definite implications concerning the connectivity of signals in the oculomotor systems, and the kinds of neural functions we would expect to find in the brain. The output of the pursuit system should go through a neural integrator: a constant input frequency should produce a constantly-increasing output frequency (see my 1973 book, p. 30-32, for a suggestion about how neural integrators might be organized). The output of the saccade-generating system should contribute to the input of this neural integrator, and not go directly to the muscles. If functions and connections of this type can be found, this would give much more strength to the model.

I would naturally be much interested in your up-to-date critique of these suggestions.

Best regards,

Bill P.

Date: Thu Oct 10, 1991 5:51 am PST
From: POWERS DENISON C
EMS: INTERNET / MCI ID: 376-5414
MBX: powersd@tramp.colorado.edu

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

Yours of 9/18
Hello, Dag:

The basis of the aiming systems, as well as I remember, was a "stable platform" referenced to either gyroscopes or a "Schuler-tuned pendulum." A Schuler-tuned pendulum is a physical pendulum (mass both above and below the fulcrum) with a period of 24 hours. After such a pendulum comes to rest it always points straight down regardless of short-term pitching and rolling, and regardless of position on the Earth. The platform was servo-controlled to provide a constant true horizon reference. A gyrocompass gave the direction reference in azimuth.

The guns and radars were referenced, of course, to the hull of the ship. But the angles between the hull and the stable platform could be sensed, and the hull-referenced directions could be transformed into horizon- referenced directions. The reference signals for gun-pointing were varied as the ship rolled to maintain the pointing direction relative to the stable platform. So there was at least a two-level control system with a system at each level for elevation, azimuth, and yaw. At a higher level, of course, we have the gunnery officer saying which radar target is the one to aim at, and the Combat Information Officer saying what targets are to be selected among all those possible, etc. . There were also little parasitic control systems whose job it was to prevent the gun from shooting through the ship's own superstructure.

At another level, the references for the stable platform had to be corrected frequently using star sights, because the gyros were not stable for very long. This is like long-term correction of a "feed-forward" system (vestibulo-ocular reflex).

Telephone amplifiers:

Consider an amplifier with an input and an output. If you tap off exactly 1/10 of the output signal and feed it back to a comparator that also receives the input signal, it is clear that the error can be zero only if the output signal is exactly 10 times the amplitude of the input signal. If you tap off 1/100, the output signal will be exactly 100 times the input. To make the error zero, we need a high loop gain: any error has to be amplified in the forward part of the circuit by a large amount and turned into output. Raising the forward amplification does not produce greater output in this feedback arrangement; it produces smaller error.

Once the error is small enough, further increases in gain will not much affect the output amplitude (although they will continue to reduce the error). If the error is only one per cent, then the output is 99% of the required output, and can only increase by 1 more per

cent no matter how much the gain is raised. Conversely, if the gain is already very high, then a loss of gain (as from aging of the circuit components) will reduce the output only a very little. If the forward gain is 50000, and if 1/100 of the output is fed back to the comparator, the loop gain is 50000 * 1/100 or 500. The output amplitude will be just 1/500 short of 100 times the input: i.e., the gain will be 99.8. Now if the forward amplification drops to 5000, a loss of 90% of the gain, the loop gain will be 50. The output amplitude will drop to 98 times the input. So we have lost 2% of the gain when the forward amplification has dropped by 90 per cent.
This also applies to frequency response. The feedback is active all during each cycle of a sine-wave input. If the forward amplifier amplifies different parts of the sine-wave differently (i.e., there is distortion) the feedback will remove most of the distortions. If the forward gain falls off as frequency increases, again the feedback will tend to maintain the overall gain constant. Thus the response will be flat over a much wider range of input frequencies, and distortions will be greatly reduced, in comparison with a system that provides the same net gain without feedback.

The feedback ratios are set by simple resistive voltage dividers, which are linear, flat with respect to frequency, and precise.

I don't know of any modern "servo handbooks." It's been a long time since I used them (45 years!). A modern book is "Feedback Control Systems" by Phillips, D. L., and Harbor, R. D. (Englewood, Prentice-Hall, 1988). This book is typical of what control engineering students are faced with: very little help with intuition, and lots of abstract mathematics. Some control engineer could make a fortune by writing a real handbook that emphasizes how control systems work and leaves the math for advanced courses. "The art of control engineering."

Inertial navigation:

Your intuition is correct. It starts with accelerometers in x, y, z, and rate gyros in roll, pitch, and yaw. The lineal-motion signals are integrated once to yield velocity, and once more to yield position. The rate-gyro signals indicate angular velocity and their signals are integrated once to yield orientation. Needless to say, the accelerometers and gyros must be exceedingly sensitive and precise, and the integrators must have no perceptible leakage over long periods of time. Even so, the navigation by this means is good for less than 24 hours. Then all the sensors have to be readjusted with respect to known coordinates.

The human "rate gyros" are the semicircular canals. They remain accurate only over periods of less than 30 seconds. Our lineal accelerometers are the force sensors in joints and muscles. They're good for only a few seconds. That's why walking around in a blacked-out room gets painful.

Thank your daughter for the Windows program. I may have to wait until I get a larger hard disk to use it. Check follows. I didn't see a cost for the Resource Kit. Please advise.

Yes, Wayne's info is encouraging. Let's go on investigating, however, in case we can do the same things cheaper.

Best

Bill P.

Date: Thu Oct 10, 1991 6:00 am PST
From: Bruce E. Nevin
Subject: saccades

[From: Bruce Nevin (911010 0742)]

Lo Bour (911009)
Bill Powers (911010)

Speaking out of speculative ignorance (where all the interesting stuff lives :-) . . .

Might it not be that there is one control system controlling for figure (foveal tracking) and another controlling for ground? In the investigations of night vision by those folks in Arizona (I sent you the article from WER, Bill) it seemed important that they fix foveal vision on a faintly luminescent target about 20 inches in front of the eyes, and then learn to shift attention around from place to place within to the very much larger (and very much more light sensitive) peripheral field. Between saccades, tracking could still be done allowing the target to drift toward the inner perimeter of peripheral vision, then a saccade would re-center it. A saccade is then a brief sacrifice of control of perception of ground or field to enable continued control of figure, so managed so as to accomplish higher-level control of the figure-ground relationship.

Bruce Nevin
bn@bbn.com

Date: Thu Oct 10, 1991 9:19 am PST
From: John Maag

Please remove me from your list. Thank you.
John Maag
University of Nebraska-Lincoln

Date: Thu Oct 10, 1991 9:54 am PST
From: Bruce E. Nevin
Subject: people generalize across persons

[From: Bruce Nevin (911010 1247)]

I have hit upon a way of thinking about the unsettled relationship between the psychological and the social aspects of language.

The social aspects of language involve generalizations across persons because the people using language make generalizations across persons in order to set their internal reference levels for many aspects of language use. People are pretty good at this (remarkably good). There is high agreement in a given speech community about what most of the norms are, though there is far less agreement in (a) the gain on various control systems taking those norms as reference values and (b) actually controlling for those norms at all (i.e. controlling instead for alternative norms of some intersecting speech community.) It is no wonder, then, that their generalizations are not perfect (and that the norms change through time). They cannot be perfect in part because they are drawing generalizations about a moving target.

Hence, also, the validity of statistical findings such as the following (taken from the linguist digest):

>Date: Wed, 22 May 1991 09:47 CDT
>From: BERN@ducvax.auburn.edu
>Subject: Hyouston

>The distinction between Hyouston and Youston is one of the features
>Guy Bailey (Oklahoma State Univ.) and I (Cynthia Bernstein, Auburn Univ.)
>analyze in our study of Texas phonology based on a Texas Poll survey.
>Peter Gingiss' estimate is right on the mark: of 910 responding,

>[78%] said Hyouston, [11%] said Youston, [11%] couldn't be determined, and
>the rest said something else (Huston). This survey was limited to
>Texas residents.

(More on statistics presently.)

This also accounts for a fundamental characteristic of language often overlooked by nonlinguists: languages change. For example, speakers of the same dialect, if separated into distinct speech communities (socially or geographically), over time come to control for increasingly different norms. To account for the fact that autonomous living control systems do this *in* *synchrony* I believe we must acknowledge that LCSs draw generalizations across the behavioral outputs of LCSs about the norms which they (themselves and, they and we presume, the observed othes) maintain as internal reference values for social behavior, including but not limited to language.

The pronunciation of Houston provides an example of such divergence, where speakers remaining in the place of origin have in this case been more conservative:

>Date: Tue, 28 May 91 10:20 MET
>From: "Norval Smith (UVAALF::NSMITH)" <NSMITH@ALF.LET.UVA.NL>

>Subject: RE: Hyouston
>
>The funny thing is that the place that is ultimately the source of the name
>Houston - whether the place in Texas gets its name from Sam Houston or not -
>Houston in Renfrewshire, Scotland, is pronounced [hust@n].
>It is a Scots name, hoose (i.e. "house") + toon
> t@n (i.e. "town" (actually rather "settlement")).
>
>Norval Smith

(Here, @ stands for schwa, though it may be closer to a syllabic n in this word, as in "button" and our pronunciation "Hyouston".)

As regards control of perceptions relative to internal reference values, statistical measures are of little use. As regards the processes by which people set internal reference values of the "social convention" sort, measures are in order that correspond to the way individuals generalize across the outputs of other members of their population. This way of formulating the problem may suggest more apt ways of formulating statistical analysis, ways that can be modelled in CT terms.
Comments? Suggestions?

Bruce Nevin
bn@bbn.com

Date: Mon Oct 07, 1991 5:11 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: Self-explanatory

Happy Birthday to You,
Happy Birthday to You,
Happy Birthday dear Wa-ayne,
Happy Birthday to You!

Dag

Date: Fri Oct 11, 1991 8:51 am PST
From: POWERS DENISON C
Subject: Averted vision; generalizing; misc

[From Bill Powers (911011.0730)]

My computer link will be down from 1900 Friday to 0800 Monday.
Bruce Nevin (911011) --

Saccades are especially necessary at night owing to the retinal integration time needed to accumulate enough light for an image at low light levels. The night-vision method described in the article you sent me uses fixation on a point of light stationary with respect to the head, so foveated moving images would be blurred. But this would not preclude using off-center attention for guidance at night. The article (about people trotting along rock-strewn arroyos at night by learning to attend to non-foveal visual images) was fascinating. All visual astronomers learn this trick for seeing faint nebulae: they call it "averted vision." If my oculomotor model is right (and even if it isn't), there is a higher level of vision processing that permits changing the locus of attention within the visual field, relative to the fovea. I think this may be what you refer to by control of "ground" (where "figure" control refers to foveal fixation. More grist for the "attention" mill, when we get around to turing the crank.

Re: generalizing by people across people. You can divide people into two categories: those who generalize and those who don't. But seriously ...

The ability to generalize belongs, I think, at the "principle" level. At this level, I assume, we perceive in terms of generalities and averages, and what we perceive are principles, not rules. The process of generalizing can, as you propose, track a "moving target" of SLOWLY changing generalizations about how people behave or about how anything else behaves. So your speculation furthers the cause. The evidence seems solid.

John Maag (if I've caught you in time) --

Sorry to see you go. Perhaps later.

John Arcady-Meyer (direct communication)

Pressed for time -- I will reply on Monday to your query.

Best to all

Bill P.

Date: Fri Oct 11, 1991 1:22 pm PST
From: Ed Ford
Subject: lucky me

from Ed Ford (911011.1335)

(Bill Powers (911004.0600)

>We're talking, of course, about the people who review your papers,
>Rick. You and I look at the referees' comments, roll our eyes and
>gnash our teeth, and laugh incredulously at the arguments they present.

I never realized how lucky I was. You (Bill, Rick, et al) need those reviewers more than they need you, at least as far as your wanting to get yourself published in their

journals. Those with whom I work need me more than I need them (although I do appreciate the financial income they provide). Right now I am dealing with graduate students in social work who need help as they struggle while working with difficult people in their case loads, three school districts who are trying to figure out a better way of handling students and reducing discipline problems, and clients trying to deal with the conflict in their lives. None have ever heard of control theory and could care less, until they perceive the connection between the need to understand humans as living control systems and the techniques derived from this theoretical base upon which they can develop and learn more effective and error-reducing methods for working with themselves and/or their population. As they find greater success in reducing their particular errors, their acceptance of control theory is assured. In several cases, I've had students trying to teach their solidly entrenched behavior mod supervisors what control theory is all about. As one teacher said recently, "I don't quite understand your control theory, but it sure works well with my 2students."
222
Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, Arizona 85253 Ph.602 991-4860

Date: Fri Oct 11, 1991 7:22 pm PST
Subject: my cat bites
From: David Goldstein

We have a four month old kitten in the house. His name is "JC CAT" for "Jake Chelsea." At first we thought he was a she until the Vet educated us, then she became Jake.

Perhaps it is from this confused sexual identity, who knows, but this cat bites our hands and feet. This does not happen all the time. The frequency is becoming less over time.

Taking a strickly behavior modification approach, we say "no" strongly and have followed the "no" by some aversive stimulation such as: watergun squirt, pinch on the tail, ignoring the cat. None of these measures have worked.

Taking a PCT viewpoint, what experience do you think this kitten is controlling? Some of our guesses: he wants to play with us, he is teething like a baby and this feels good on his gums. There is nothing we have discovered which stops the behavior in a natural way. We do give him something else to bite on and this seems to be acceptable. So we know it is not a personal grudge and has more to do with the biting experience. I should add that this kitten does not really hurt us and seems to be showing some kind of restraint.

My goal is: understanding and finding a way to stop the biting. A friend of my daughter calls JC "the cat from hell." A friend of my son is afraid to come in the house.

Date: Sat Oct 12, 1991 11:51 am PST
From: Ed Ford
Subject: CSG Newsletter & stuff

from Ed Ford (911012.1245)

The CSG Newsletter has been delayed two weeks because Greg Williams, our Closed Loop editor, needed to get the roof on the house he's building so he could protect it from the Gravel Switch winter weather. Right on, Greg!

Anyone wishing to have a copy of the post-conference CSG Newsletter sent to them, please post a request and your mailing address. Closed Loop, the quarterly summary of the CSGnet, is mailed only to paid up members. For membership in CSG, please send $40 payable to Control Systems Group and mail to: Mary Powers, 73 Ridge Road, CR 510, Durango, CO 81301.

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

Date: Sat Oct 12, 1991 12:33 pm PST
From: TJ0WAH1
Subject: Robinson's model

[From Wayne Hershberger 911011]

Thanks, Dag, for the birthday serenade.

(Dennis Delprato 911001)
>in my opinion, Wayne Hershberger's handling of Volitional Action
>was exemplary. What a tribute to control theory! The breadth
>of that volume speaks highly of the potential of control theory.

Thanks for the kind words and the plug. I too regard the book [Hershberger, W. A. (Ed.). 1989. Volitional Action: Conation and Control. Amsterdam: Elsevier] as a tribute to the potential of control theory--just count the number of excellent chapters contributed by CSG members.

(Chris Malcolm 911007)

Your delightful anecdote reminded me of the final paragraph of my chapter in "Volitional Action: Conation and Control."

> There is a conservation principle at work. The development of hierarchical control serves to centralize choice or volition, but does not increase it. The amount of will which may be marshalled, or mustered, or brought to a focus is thus limited by the control system's ordinal size, that is, by the number of orthogonal inputs (or variables) the system is able, in principle, to control. There is no will to be found lying around free (p. 18).

TO: Gary, Rick and Bill:

RE: David Robinson's closed-loop model of the oculomotor system (Gary A. Cziko 911004)

>If Robinson work is so highly regarded and if he is a
>"first-rate control theorist" then he must be publishing in
>places that are receptive to this model of behavior. Of is this
>type of research itself "marginalized" within psychology? I
>suspect that perhaps your idea of a "first-rate control
>theorist" is different from Rick Marken's. Does Robinson look
>at controlled variables? Does he construct models of working
>systems? Or is it an adaptation a la Carver and Scheier of
>control theory to fit the traditional mold of psychological thinking?

Gary, I believe Robinson has an engineering background and would probably bristle if he were called a psychologist. He is in the Biological Engineering Department of Johns Hopkins Medical School. He is not from the mold that gave us Carver and Scheier. He publishes in the Proceedings of the IEEE, Vision Research, and various journals of neurophysiology (i.e., marginal psychology). I have read that Robinson has a generative model which simulates oculomotor phenomena faithfully, and I have no reason to doubt that.

Rick Marken (911007)

>My impression is that Robinson's model of oculomotor control is
>very much like the "manual control theory" models of manual
>control. They...ignore a few issues that PCT type control
>theorists think of as exceptionally important: 1) there is no
>attempt to test the definition of the controlled variable. It is
>just assumed that THIS (position of a line, angular displacement
>of the eyeball, whatever) is what needs to be controlled, so we
>will make up a control system that can do it and 2) there is no
>variable reference for this variable "inside the model". The
>reference state of the controlled variable is placed outside the
>system by incorporating it into the definition of the stimulus
>(ie -defining the stimulus as distance from an external target).

Rick, Robinson differs from your "manual control theorists" on both counts:

As for point 2, Robinson's model controls eye position ("input") rather than eye movements ("output"). And the reference signal for intended eye orientation has been identified (as well as hypostatized) as being a neural signal in the paramedian pontine reticular formation.

As for point 1, I direct your attention to a passage in the chapter I distributed in Durango.

> The experimental data that provides, perhaps, the best evidence in support of Robinson's current model have been those reported by Mays and Sparks (1980b). Mays and Sparks, investigating saccadic eye movements in rhesus monkeys, used electrical stimulation of the superior colliculus to move the eyes of a monkey just before he began a saccade to a spot of light flashed previously in the dark (i.e., flashed just before the electrical stimulation). Despite this electrode-induced perturbation, and the fact that the flashed target was no longer visible, the monkey's subsequent saccade brought his gaze to the target location, something clearly impossible had the movement been determined solely by retinal information.
>
> Clearly, the oculomotor system controls eye position, rather than eye movements. That is, eye movements are directly driven by an oculomotor error signal rather than a retinal error signal. According to Robinson's current model the muscular innervation driving the eye from one position to another depends upon the difference between two neural signals, each representing eye position, one sensing the position of the eyes (efference copy) and the other specifying the intended position of the eyes (p. 9).

(Gary Cziko 911007)
>Rick, I think you've put your finger on a crucially important
>print. Control theory can be made quite compatible with
>traditional approaches to psychology if the reference levels are
>seen as being determined by the environment.

Gary, I think you are right, Rick put his finger on a crucial point--although it is not a criticism that applies to Robinson.

(Bill Powers 911008)

>It isn't hard to identify obvious controlled variables and their
>reference states, if the reference states remain reasonably
>constant. The real question is how you explain their existence,
>and how you explain changes in the reference state.
>   With the reference condition defined in the environment, you
>are stuck with an unexplainable puzzle: why was exactly that
>quantitative target state the one that was effective? In visual
>tracking in particular, you have to explain not just why the
>subject looked at the target, but why the reference position for
>looking was a particular spot on the target defined with a
>precision of a couple of minutes of arc. Why THAT spot and not
>the one next to it in any direction? Why not a spot off the
>target? You also have to explain why this spot keeps jumping around.

Bill, I think this is a thread which warrants exploration. I submit that it is not just the reference value, or "state," which is missing in traditional behavioral models. The perceptual variable whose value is being controlled must often be selected from a plethora of possibilities, and this selection process also appears to be missing not only from traditional behavioral models but from our own control models as well. For instance, to look at ANYTHING is to zero the retinal eccentricity of the thing's image. Zeroing the retinal eccentricity of whatever image has been chosen implies a constant reference signal (zero eccentricity) for any instance of "looking at."

What varies, as you say, is the target-image whose retinal eccentricity is to be controlled (brought to and kept at zero). For example, our line of sight is automatically pointed at the chosen visual target just like the trajectories of the "smart bombs" used in Iraq were pointed at the military targets identified by a laser beam. What appears to be missing from our models is the target-selection (target-identification) process illustrated by the aiming of the laser beam in this military example.

You pose but do not address this problem in your comments above. As I see it, the problem is this. In a hierarchical control system, superordinate loops may select perceptual inputs (variables) to be controlled (i.e., select subordinate loops) as well as to select reference values for those selected subordinate loops. You have treated the former problem (selecting variables) in terms of reorganization, but the problem seems to me to be involved in the process of selecting visual targets--for which reorganization is overkill.

A penny for your thoughts.

A coda:

(Gary Cziko 911007)

>We all know what CIRCULAR causation is....

I don't think I do. More exactly, I doubt that there is such a thing. I would not call closed-loop control "circular causation." I guess I don't know what you mean; perhaps you would like to explain.

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 Oct 13, 1991 9:23 am PST
From: Gary A. Cziko
Subject: Magnetic Levitation

[from Gary Cziko 911013]

Hershberger, Marken, Forssell, Powers:

Last week Jeff Schiano, a control system engineer on my campus, showed me a quite amazing demonstration of a control system in action. It involves suspending a metal ball about one inch below an electromagnet. I believe someone mentioned on the net that this couldn't be done. I forget who it was. Please fess up.

The electromagnet is small and is contained in a 35mm film container. About an inch below it is a LED as a light source on one side with a photoreceptor (electric eye) on the other. The control system is set up to vary the electricity sent through the magnet to control the amount of light falling on the photoreceptor. If too little light strikes it, less current is set through the electromagnet. If too much light strikes it, more current is delivered. Place the large ball bearing carefully between the light source and receptor so that its top blocks some of the light but not all, and it just hangs there in space. You can even pull the ball down and feel it "tugging" upwards, or push it up and feel it grow heavier. It turns out that anyone can buy a similar apparatus from Edmund Scientific in NJ. It is called "Newton's Folly" (why blame old Isaac for control systems?) and costs about $120.

Jeff is a new electrical engineering professor who teaches control theory to budding engineers. He is now working on a much larger apparatus that will use sonar to measure the distance of the suspended ball. This way one can vary the reference level (the time between the transmission and reception of the echo) so that the ball can be moved up and down at will instead of hanging in one fixed spot. He will show me this in a couple of weeks when finished. It will cost between $800 and $1200 to build.

After Jeff amazed me with his toy, I pulled out my trusty knotted rubber bands and let him play experimenter as I maintained the knot over a corner of the desk as he yanked away providing disturbances. He figured that I was just "mirroring" his action. Then I had him cover his hand with a board so that I could not see it and continue. Then he was really miffed. He gave up trying to figure out what I was doing. When I told him, he said it would have taken him a very long time to figure it out. What is amazing, is that the phenomenon of control is so elusive, even among control system engineers! Makes me appreciate even more Bill Power's discovery. Maybe this discovery is worth a story, Bill, for the archives.--Gary

P.S. Jeff says that using control systems he can control the spin of neutrons! Imagine, control theory meets quantum mechanics. This is particularly intriguing since at the quantum level any measurement disrupts what you were measuring in the first place.

P.P.S. Jeff also recounted a project he did as an undergraduate in a psych. course. He hooked up his girl friend with a belt attached to a weight and pulley. For her to stand up, she had to lean against the weight. On the other side he attached a strain guage to measure her movements. She was pretty stable. Then he covered here eyes. Oscillations set in, but whe was still able to maintain her balance. Then he put 6 volts from a lantern battery from one ear lobe to the other. This disturbed here vestibular system (inner ear balance equipmen) and within seconds she was on the floor.

He got an A for the project. He never did mention what happened to the girlfriend.

Gary A. Cziko

Date: Mon Oct 14, 1991 6:06 am PST
From: POWERS DENISON C
Subject: Oculomotor systems, misc

[From Bill Powers (911014.0600)]

Ed Ford (911013) --

Yes, you're lucky. Your students don't have to unlearn other theories.
David Goldstein(911013) --

Biting cat:

<Taking a strickly behavior modification approach, we say "no" strongly
>and have followed the "no" by some aversive stimulation such as:
>watergun squirt, pinch on the tail, ignoring the cat. None of these
>measures have worked.

It took my 9-year-old cat a week to learn how to go through a cat door, and I still have to prop it open (the door). Cats do not take to external control (n = 1). My cat bites to make me stop interfering with it. It works. You cat is probably playing. If your cat were angry with you it would draw blood. Squirting the cat etc. and hollering "no" will make it afraid of you or annoyed with you but will not teach it not to bite. It needs to chase and pounce. So give it something else to chase and pounce upon. This will satisfy its reference level and will not "teach it to chase and pounce." It will stop annoying you when it gets what it wants. Try a lot of affection. Mother cats bop their kittens lightly on the nose to correct them. I believe this has worked occasionally with my cat --but you mustn't hurt it. A light finger tap. It's a cosmic-cat signal. Another cosmic-cat signal is picking it up by the scruff (loose skin over shoulders). This does not hurt it. Vets do this. The cats quiet right down, as they do when their mothers carry them this way.

Cosmic cat: Mary and I have decided that there is really only one cat, the Cosmic Cat. It replicates completely, including cat customs, cat sounds, cat postures and poses, and so on. When it comes to cats, we are sociobiologists. On Monday, Wednesday, and Friday.

>A friend of my daughter calls JC "the cat from hell." A friend of my son
>is afraid to come in the house.

This cat may have been mistreated (mine was before we got it). I've been told that lots of picking up and petting improves trust. My cat tolerates it, but still prefers to determine its own time for affection. So I let it. I am not a cat expert. It sounds as though your son's friend has more of a problem than the cat does. Figuring out what the cat wants and giving it to the cat, I have found, invariably works. Cats are in charge of cat relationships. That's why they fight.

You might also reconsider behavior mod as an effective way of changing behavior. When it works, it works because it gives the organism control, not because it stimulates it (aversively or otherwise).

Wayne Hershberger (911013) --   Robinson's eye model:

> As for point 2, Robinson's model controls eye position
>("input") rather than eye movements ("output").

>According to Robinson's current model the muscular innervation
>driving the eye from one position to another depends upon the difference
>between two neural signals, each representing eye position, one sensing
>the position of the eyes (efference copy) and the other specifying the
>intended position of the eyes (p. 9).

From this I deduce that the oculomotor system runs in the imagination mode. Is this control of input? I would say, control of *imagined* input, because the eye could be disturbed without altering the efference copy signal. Of course the eye isn't subject to mechanical disturbances under normal conditions, and the visual feedback takes up the slop from a miscalibrated imagination signal. As I recall, single saccades aren't very accurate.

All control is control of input. In the case you mention, what is under control is a signal, not eye position. Eye position follows control of the signal because the eye is well-protected against disturbances and has essentially no static friction. But the eye (in the dark) does not actively resist disturbances. So its position is not under non-visual control. An efference copy does not amount to "sensing the position of the eyes."

How does Robinson handle pursuit tracking?

Selection of visual targets:

I'm kicking myself for not making note of a Science report on this subject. It appeared this year, I believe. In it, the author showed a diagram of a two-dimensional neural map in which a bell-shaped region of activity moved around as the animal changed fixation points. The appearance was that a reference signal was injected somewhere in this map, and the region of activity moved to that place as the eyes (or a limb?) moved. I'm not confident that I have this picture quite right, but maybe someone with access to back issues could try to find it.

Some kind of internal selection seems likely to me. When you're fixated on one target, you can "attend" to a second target without moving your eyes. This implies a control system that moves the point of attention, like changing the place in the internal map from which a higher system is to receive signals for visual processing. The attention point clearly locks onto the image-representation at that place, because when you shift your eyes to the new target, foveating it, attention stays on the target instead of being fixed to a retinal position.

I once build a position-control servo with a photocell mounted on the vertical shaft of the motor. The photocell could scan around the room (about a 10-degree angular resolution). The feedback from the pot that was in the position servo provided a position signal. The digitized position signal was used as an offset address into a data array, where the amplitude of the photocell signal was stored. As the position reference level was scanned from 0 to max, an updated map was created. This array of data was basically a one-dimensional map representing the lateral light intensity distribution around about a 350-degree horizontal arc. The system was set up so that the scan repeated over and over, updating the map about once per second.

An imagination-mode control process at a second level could then lock onto any zero-crossing (the average intensity was subtracted from all array entries). If a slowly-moving object passed that point, the imagination-mode system would lock onto its edge and follow it -- by picking a stationary reference edge in the map, you could get a signal indicating where the moving object was relative to the map of the room (rather than relative to the direction of looking). This was all done as a side-excursion on the way to building the first arm-control demo that I took to the Philadelphia cybernetics meeting just before we formed the CSG. Then I sort of forgot about it. Maybe it would be worth setting up again -- it's probably relevant to the question of target selection. I remember thinking that this would be worth pursuing because it looked like a way to create a stationary map of the surroundings (in only one dimension, of course) that could be turned around and used as a way of calibrating bodily position in objective space -- changing from body-centered to room-centered coordinates.

At any rate, correcting visual eccentricity must involve two steps. The first requires an internal control system that connects a region in the visual map to an input function of higher level, like moving a selector switch or scanning an internal camera over the representation. The second step must be to bring the location of the fovea to the location of the selected point in the map. In order to do this without moving the internal map, the foveal input must be routed to the appropriate place in the map, updating that region. Thus the map remains stationary in the nervous system while the place where visual input reaches it changes with the direction of looking. Higher systems would then experience a stationary visual world. We can get some idea of how long an update lasts by scanning around a room (360 degrees) and seeing how long the impression of parts of the room behind us lasts. It doesn't seem to last very long -- a few seconds. Then it gets pretty vague, for me.

A slightly more complex model would entail moving BOTH the direction of the internal camera AND the location where visual input is routed. I don't know -- I'd have to experiment with the visual scanner thing to get the various possibilities sorted out. I don't want to start on that until some other projects have reached a plateau.

I think this is all relevant to the general problem of attention.
Circular causation:

I interpret this to mean the case in which signals and physical effects propagate around a closed loop very rapidly with respect to the time during which the signal amplitudes or physical variables can change. Causation is normal in any one element of the loop (that is, output depends on input as usual). But as the transit time becomes shorter and shorter, we approach the case in which a signal is causing its own present amplitude. This is basically what a quasi-static analysis does: it extrapolates to the limit of zero delay, and the system equations become algebraic. If the system is dynamically stable (no spontaneous oscillations), the algebraic solutions are veridicial.

Actually, more obvious examples of circular causation can be found in non-living systems. My favorite example is a double star orbit. Each star's gravitational attraction causes the path of the other to bend into an ellipse around a common focus. But the bending of the path determines the distances of separation, which influences the attraction -- all at the same instant. Causation runs both ways at once. That's why physicists seldom talk about causation. It's easier to think in terms of relationships and simultaneous mutual influences.

If you represent the processes taking place around a neural loop by dynamic equations -- using LaPlace Transforms, in which time-delays appear merely as an algebraic exponential term -- all the relationships are seen to exist at the same instant. The individual part-relationships are then solved as a simultaneous set of equations to see the state of the whole loop and predict the time-course of variations in signals anywhere in the loop at successive instants. I think this gives the truest, if least intuitive, meaning of circular causation. Everything in the loop is changing simultaneously, rather than one event taking place at a time so you can trace the propagation of momentary events sequentially. David Goldstein said it: it's like a wheel turning, not like a marble rolling around a hollow circular tube.

Basically the concept of circular causation is an oxymoron, because "causation" is not the right concept to apply to a closed loop.
Gary Cziko (911014) --

Magnetic levitation:

>... involves suspending a metal ball about one inch below an
>electromagnet. I believe someone mentioned on the net that this
>couldn't be done. I forget who it was. Please fess up.

It was I. But I said that stable levitation can't be done without active control or special shaping of magnetic fields. The system you describe uses active control, obviously, which makes it easy. The reference signal specifies how much light the photocell is to sense. The output function is the strength of the magnetic field, varied by changing the current through a coil. It's just a control system controlling light intensity at its own sensor. The levitation is a side-effect.

Rail cars that levitate above a rail by magnetic repulsion are supported by separate magnets on a frame. That makes the system stable up-and-down. It is stable horizontally only at a dip in the tracks. If you use multiple magnets far apart on a frame you can get passive levitation that is stable. The overall magnetic field is "specially shaped."

Date: Mon Oct 14, 1991 6:23 am PST
From: marken
Subject: Cats

David
Control theory may be a crock and it may be pouring here in Los Angeles, but there is one thing that's for sure -- behavior mod does NOT work on cats.
Love
Rick M

Date: Mon Oct 14, 1991 10:38 am PST
From: jbjg7967
Subject: generalizability

[from Joel Judd]

A quote I've got in a dissertation chapter which seems to bear on the "principle" perception of generalizing:

"The mental habit of building up types on the basis of one's individual, specific, concrete experiences tends still further to give apparent uniformity to the continually varying experiences."

This was written by some language pedagogists (Pillsbury and Meader) in 1928.

Date: Mon Oct 14, 1991 12:34 pm PST
From: Ed Ford
Subject: teaching PCT

from Ed Ford (911014.1300)

Bill Powers (911014)

>Your students don't have to unlearn other theories.

I'm spending Thursday through Saturday of this week at Johnson City Schools in Binghamton, NY teaching control theory to a group of educators who teach a program of outbased education to school systems throughout the country. These educators are struggling to understand both the theoretical concepts of control theory and how the practical applications that flow from this theory are more relevant than what they are teaching. Their present program includes a segment based not only on Glasser's reality therapy, but also on his ideas on control theory. One of their administrators got hold of my book, Freedom From Stress, and found our ideas on control theory more appealing, although a lot harder to learn.

But for the most part, your right, Bill. My social work students at ASU are looking for practical, down-to-earth techniques and ideas and my undergraduate students at Phoenix College have had little or no exposure to psychology.

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

Date: Mon Oct 14, 1991 12:56 pm PST
From: Ed Ford
Subject: For Joel Judd

from Ed Ford (911014.1330)

Joel Judd - please send me your address and phone number for the newsletter records. Sent you a direct message but I guess it didn't make it. Thanks, Ed.

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

Date: Tue Oct 15, 1991 11:28 am PST
From: marken
Subject: Misc notes

[From Rick Marken (911015)]

It's pretty quite out there so I just thought I'd post something.
Wayne Hershberger -- thanks for the comments on the Robinson model. I'm sure it's close to the PCT approach, but I have the same kinds of reservations about it as Bill. Still, I think PCT types have less problems publishing control models of small subsystems (unless there is strong prevailing dogma in the area) than they do in publishing stuff about new points of view about some more general phenomenon (such as language -- I hope the linguists out there test this hypothesis). Your own paper that you submitted to American Psychologist (on Skinnerian behaviorism) is a more general PCT point of view. Any news on how it's doing? I really hope it gets published there. I submitted my "Hierarchical behavior..." paper to them some time ago. No word yet.

I'm giving a talk on my PCT research activities to a group at my old alma mater (UCSB) tommorrow. I'm looking forward to it. I'll let you know how it went. I suppose everybody's still enthralled by the "He did it -- no i didn't" story that's going on. Maybe we could do a little discussion of the PCT approach to lying.

Best Regards to All

Rick

Date: Wed Oct 16, 1991 5:55 am PST

From: Bruce E. Nevin
Subject: paying attention

[From: Bruce Nevin (911016 0833)]

(Ed Ford (911011.1335))

Re "Lucky Me," the difference is that the people you are dealing with are dealing with environmental consequences of import for them. One aspect of a scientific paradigm getting tenure, so to speak, is that it insulates itself from environmental consequences that don't fit, usually by declaring them irrelevant, unscientific, or too messy for the field to deal with yet. In psychology, sociology, linguistics, etc. we need some X that matters to our audience, so that a difference in X makes a big difference to them, and they find themselves, like your people, making the following utterance: "I don't quite understand your control theory, but it sure works well with my X." The problem is that issues must be framed in terms accepted in the field in order to be of concern within the field. How did it go with Copernicus, Einstein, and other players in scientific revolutions? Didn't they make predictions (a) that could be tested in paradigm-neutral settings and (b) that accounted for neglected and puzzling phenomena that everyone accepted were there? I gather CSG folk have been doing this. Why does this not add up to environmental consequences that matter to more of your audience?
I am not going to undertake anything like this until after I complete the PhD, but I am trying to get some insight into the issues now.
[from Gary Cziko 911013]

>quite amazing demonstration of a control system in action. It involves
>suspending a metal ball about one inch below an electromagnet.
>I believe someone mentioned on the net that this
>couldn't be done. I forget who it was. Please fess up.

I had described a magnet repelling a ring or cylinder upward. In that case some external constraint is needed to keep the latter from moving laterally out of the magnetic field, sliding along the curved plane at which the repulsion of the two magnetic fields above the coil balances gravitational attraction on the ring or cylinder. This is like balancing two very slippery balls, one on top of the other. (The magnetic field in the ring or coil was due to current induced in it as the expanding and collapsing field of the coil in effect moves past it. The main field expands and collapses 120 times a second, in flip-flopping polarity, with 60-cycle AC. An operating principle of the electrical transformer.)

In the case of the demo you saw, the magnet is attracting the ball, which is below it. The lateral constraint is here provided by gravity: the metal ball is like the bob of a pendulum, which descends to the lowest point of the curved plane at which the strength of the magnetic field balances gravitational attraction on the ball.

I thought it was the problem of external constraint that Bill was talking about.

[From Bill Powers (911014.0600)]

>It was I. But I said that stable levitation can't be done without active
>control or special shaping of magnetic fields. The system you describe
>uses active control, obviously, which makes it easy. The reference signal

[From Bill Powers (911014.0600)]

>Cosmic cat: Mary and I have decided that there is really only one cat,
>the Cosmic Cat. It replicates completely, including cat customs, cat
>sounds, cat postures and poses, and so on. When it comes to cats, we are
>sociobiologists. On Monday, Wednesday, and Friday.

I have often had the impression that the physical embodiment of a cat occupies a very tiny toehold anchor in the paltry four dimensions of our physical space-time continuum, and that its attention is mostly occupied by profound concerns Elsewhere, but occasionally deigns to include us somewhat more fully in its scope. Needless to say, I wouldn't know how to test that.

Reminds me, more seriously, of a recent and an older issue concerning attention. Recently (Chris Malcolm 911007) we discussed attention being "sapped" as it were by low-level control requirements (riding a motorcycle with sticky controls). Several months ago, with Martin Taylor, we discussed heightened awareness in situations of crisis or peak athletic performance. In the latter case, it appears that we are attending to the event level rather than higher levels (category and above). In the former case, the lower-level control processes apparently are not automatized, and that is why they "nag" at our attention budget. In the latter case, are they automatized, or do we exceed our routine attention budget for the occasion, or do we neglect higher levels of control. The term "automatized" may be inappropriate here, if each step up the control hierarchy involves generalization that is implicitly something like automatization.

Bruce Nevin
bn@bbn.com

Date: Wed Oct 16, 1991 6:59 am PST
From: Francis Heylighen
TO: Hortideas Publishing / MCI ID: 497-2767
Subject: CFP-Principia Cybernetica Symposium (Namur, Aug.'92)

CALL FOR PAPERS

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 *   SYMPOSIUM:  THE PRINCIPIA CYBERNETICA PROJECT       *
 *      computer-supported cooperative development       *
 *        of an evolutionary-systemic philosophy         *
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as part of the

13th International Congress on Cybernetics
NAMUR (Belgium), August 24-28, 1992

About the Symposium
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After the succesful organization of a symposium on "Cybernetics and Human Values" at the 8th World Congress of Systems and Cybernetics (New York, June 1990), and of the "1st Workshop of the Principia Cybernetica Project" (Brussels, July 1991), the third official activity of the Principia Cybernetica Project will be a Symposium held at the 13th Int. Congress on Cybernetics. The official congress languages are English and French.

The informal symposium will allow researchers interested in collaborating in the Project to meet. The emphasis will be on discussion, rather than on formal presentation. Contributors are encouraged to read some of the available texts on the PCP in order to get acquainted with the main issues (Newsletter available on request from the Symposium Chairman).

Symposium Theme
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Principia Cybernetica is a collaborative attempt to develop a complete and consistent cybernetic philosophy, moving towards a transdisciplinary unification of the domain of Systems Theory and Cybernetics. PCP is meta-cybernetical in that we intend to use cybernetic tools to develop and analyze cybernetic theory. These include the computer-based tools of hypertext, electronic mail, and knowledge structuring software.

PCP is to be developed as a dynamic, multi-dimensional conceptual network. The basic architecture consists of nodes, containing expositions of concepts using different media, connected by links, representing the associations that exist between the nodes. Both nodes and links can belong to different types expressing different semantic and practical categories.
PCP will focus on the clarification of fundamental concepts and principles of the cybernetics and systems domain. Concepts include: Complexity, Information, Variety, Freedom, Control, Self-organization, Emergence, etc. Principles include the Laws of Requisite Variety, of Requisite Hierarchy, and of Regulatory Models.

The PCP philosophy is systemic and evolutionary, based on the spontaneous emergence of higher levels of organization or control (metasystem transitions) through blind variation and natural selection. It includes:

a) a metaphysics, based on processes or actions as ontological primitives, b) an epistemology, which understands knowledge as constructed by the subject, but undergoing selection by the environment;

c) an ethics, with the continuance of the process of evolution as supreme value.

Philosophy and implementation of PCP are united by their common framework based on cybernetic and evolutionary principles: the computer-support system is intended to amplify the spontaneous development of knowledge which forms the main theme of the philosophy.

Papers can be submitted on one or several of the following topics:
The Principia Cybernetica Project
Cybernetic Concepts and Principles
Evolutionary Philosophy
Knowledge Development
Computer-Support Systems for Collaborative Theory Building

About the Congress

The International Congresses on Cybernetics are organized triannually (since 1956) by the Intern. Association of Cybernetics (IAC), whose founding members include W.R. Ashby, S. Beer and G. Pask. The 13th Congress takes place in the "Institut d'Informatique, Facultes Universitaires Notre-Dame de la Paix, 21 rue Grandgagnage, B-5000 Namur, Belgium".
Namur is a quiet little city on the confluence of the Meuse and Sambre rivers, at the foot of a hill supporting impressive medieval fortifications. The congress atmosphere is relaxed and informal, with a lot of small symposia going on in parallel in adjacent rooms. Normally there will be a welcome cocktail, a congress dinner, and a meeting room available for coffee breaks. Participants are responsible for making their own hotel reservations, but, if necessary, student's rooms will be available.

Partial Congress Programme

The Congress will feature over 30 symposia, including the following: (CHAIRPERSON Subject)

ACALUGARITEI G. (Roumania)
Evolutions and Metaevolutions from the Point of View of the Invariants Associated to the Transformation Groups

BAHG C. (China)
Complex Systems and their Evolution

COLLOT F-C. (France)
Les notions de temps et d'evolution en Cybernetique

FRANCOIS C. (Argentina)
Les systemes humains homeostatiques ou emergents

HEYLIGHEN F. (Belgium)
The Principia Cybernetica Project : Computer-supported Cooperative Development of an Evolutionary-systemic Philosophy

JDANKO A. (Israel)
- Cybernetic Systems Approach to History
- Cybernetic Systems Interpretation of the Religious Idea : From the Primitive to the Monotheist

NICOLAU E.(Roumania)
Adaptability-Learning-Fuzziness

STEG D. (USA)
Determinacy and Indeterminacy in Complex Systems

VANDAMME F. (Belgium)
Cognitive Modelling for Knowledge and Information Technology : Manual and Automatic Tools

Submission of papers
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People wishing to present a paper in the Principia Cybernetica symposium should quickly send the application form, together with an abstract of max. 1 page, to the addresses of the Symposium chairman AND of the Congress secretariat (IAC) below. They will be notified about acceptance not later than 2 months after receipt, and will receive instructions for the preparation of the final text. In principle, all application forms should be received by December 31, 1991, but it may be possible to come in late. People wishing to present a paper in a different symposium can directly submit their abstract to the secretariat.

For submissions of papers to, or further information about, the Principia Cybernetica symposium, contact the symposium chairman:

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Dr. Francis Heylighen
PO-PESP, Free Univ. Brussels, Pleinlaan 2, B-1050 Brussels, Belgium
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For congress registration, or further information about the congress, contact the secretariat:

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International Association for Cybernetics
Palais des Expositions, Place Ryckmans, B-5000 Namur, Belgium
Phone   +32 - 81 - 73 52 09   Email cyb@info.fundp.ac.be
Fax   +32 - 81 - 23 09 45

Date: Thu Oct 17, 1991 6:03 am PST
From: POWERS DENISON C
Subject: X factor; Welcome Hugh Gibbons

[From Bill Powers (911017.0730)]   Bruce Nevin (911016) --

The X factor:

I think I got another slant on this problem in reading Francis Heylighen's post (911016) about the Principia Cybernetica Project. My initial reaction to Principia Cybernetica (Chris Joslyn, note) was pretty much "but we're already working on that!" There's more than a little bit of a threat there when someone comes along with a highly organized project and says, in effect, "You're a subset of us." The idea of being absorbed into somebody else's project gives me a sinking feeling -- "Is this all that my labor is going to amount to?" A lot of reference signals become evident only when there's a disturbance that succeeds in creating an error -- Ed Ford and I were talking about this just yesterday. I discover feelings of jealousy and competition that I didn't even know about.

All this leaves me rather amazed when I consider the other people on this net, who have surely gone through similar experiences with the impact of control theory on their own disciplines. How can all you people be so open-minded and lacking in defensiveness? Haven't you had that sinking feeling, too? Is it just me who feels at times like a nasty little kid who wants to play the game his way or not at all?

It seems to me that the rejection of control theory, *particularly* by smart productive famous expert people, is totally understandable by anyone whose has ever tried to accomplish something only to have someone else do it better. That is an awful experience, especially if you had hopes that your accomplishment would do something wonderful for you. This has nothing to do with scientific purity, paradigms, philosophy of science, or the like. Science is, unfortunately, competitive, and everyone is tainted by that conflict. My gain is your loss.

I think the only way around this problem is to find the highest level conception of science that we can find, and remember that this is really what we want. No, not even "science." How about just "living?" Don't we want a sensibly-populated world at peace with itself, free of misery and injustice and hardship (save what hardship we choose for the sake of adventure)? It's only at that kind of level where we can find ourselves solidly on the same side. There has to be some meta-context, as Joslyn would put it, that makes the gain or loss of a particular paradigm only an incident on the way to doing something more important.

Or am I the only one who has trouble hanging on to this point of view?

Hugh Gibbons --

I celebrate your arrival on CSGnet. If you tackle the archives of our discussions, you will find that your name has been dropped here and there. To everyone else, Hugh Gibbons teaches law at Franklin Pierce Law Center. That is like saying Neils Bohr taught physics in Copenhagen. Hugh, how about an introductory essay explaining what control theory has to do with law?

Best to all Bill P.

Date: Thu Oct 17, 1991 7:56 am PST
From: Bruce E. Nevin
Subject: fast loop for speech

That _is_ good news!

Welcome back. We've missed you.

Bruce

Date: Thu Oct 17, 1991 9:04 am PST
Subject: whose X?

[From: Bruce Nevin (911017 1202)]

Bill Cunningham (911016.1530)--

> Consider X as the reference that the outside audience is controlling
>for. If the evidence offered is reasonably close to X, then perception
>of the evidence can be modified to agree with X. Successful control makes
>everybody happy. However, if the disturbance is outside the control range,
>it is rejected, angering all parties.

If the audience is told "organisms control perceptions and the result is behavior" and modifies this to "organisms control behavior in response to perceptions" the audience may be happy but the CT presenter won't be.

(It might be well to expand on the technical definition of "control" here and instead say "organisms do whatever it takes to make their perceptions conform to remembered reference values. This `whatever it takes' is called behavior.")

In the analogy to tracking radars, I guess we must assume that the audience A is in the role of the tracking radar and the presenter P is in the role of the pilot. P is presenting a target and A is trying to track it. The analogy breaks down because A comes to track what it expects the target to be, rather than the perceptions actually being presented.

(It might be well to warn audiences of this, and perhaps even to present one of the category-substitution illusions or one of the familar gestalt-switch images by way of preliminaries.)

Thus, it appears that the tracker, A, is the author of the signal analogous to the false radar return from jammers. Rather than the jammer signal gradually moving away from the correct return, discrepancies between the entirely false imagined target and the immediate perception of the real target becomes progressively apparent. At that point, it may indeed be that the system (A), now out of control, "goes through wild gyrations until reverting to the search mode," that is, reorganization, or more commonly I gather A insists that the imagined target is real.

Bill Powers (911017.0730)--

>There's more than a little bit
>of a threat there when someone comes along with a highly organized
>project and says, in effect, "You're a subset of us." The idea of being
>absorbed into somebody else's project gives me a sinking feeling -- "Is
>this all that my labor is going to amount to?"

This response would follow when A gets a glimpse of the real target presented by P--the paradigm shift. Rejecting this, A hallucinates the preferred target image.

>This has nothing to do with scientific purity, paradigms, philosophy of
>science, or the like. Science is, unfortunately, competitive, and
>everyone is tainted by that conflict. My gain is your loss.

This human dynamic has everything to do with paradigms and the actual history of science, I believe.

I agree that we need some higher-level principle and system concepts. Specifically, I think we need some concepts of how people change their system concepts--some concepts relevant to fostering conversion or paradigm-shift experiences. In the past, techniques of rhetoric have been used. These often have been soiled by end-justifies-means applications, and many people do not trust those whom they perceive as using them. Often, effective rhetoric depends on drawing analogies between the familiar and the target, perhaps glossing over superficialities. An appeal to common values, articulating a vision in terms of high-level concepts, may stay relatively clean. This is a rhetorical technique that you often use, Bill. In fact, your paragraph on this exemplifies it.

>I think the only way around this problem is to find the highest level
>conception of science that we can find, and remember that this is really
>what we want. No, not even "science." How about just "living?" Don't we
>want a sensibly-populated world at peace with itself, free of misery and
>injustice and hardship (save what hardship we choose for the sake of
>adventure)? It's only at that kind of level where we can find ourselves
>solidly on the same side. There has to be some meta-context, as Joslyn
>would put it, that makes the gain or loss of a particular paradigm only
>an incident on the way to doing something more important.

I say again:
>we need
>some X that matters to our audience, so that a difference in X makes a
>big difference to them, and they find themselves, like [Ed's] people,
>making the following utterance: "I don't quite understand your control
>theory, but it sure works well with my X."

My X. Not yours, mine. Then "your CT" works well with it, and consequently I want to learn more about it.

Martin Taylor (Wed, 16 Oct 1991 15:34:58 EDT)--

Do you have a reference for that work on evolution of closer proximity of articulatory organs and cerebellum (if indeed it was cerebellum)? A name? Will the work be published someplace? I'm hoping to get some time to make contact with Lieberman, and this is the sort of thing he would be most interested in.

Bruce Nevin
bn@bbn.com

Date: Thu Oct 17, 1991 11:53 am PST
From: marken
Subject: X factor, Little Man

[From Rick Marken (911017)] Bill Powers (911017) says:

>All this leaves me rather amazed when I consider the other people on this
>net, who have surely gone through similar experiences with the impact of
>control theory on their own disciplines. How can all you people be so
>open-minded and lacking in defensiveness? Haven't you had that sinking
>feeling, too? Is it just me who feels at times like a nasty little kid
>who wants to play the game his way or not at all?

I feel that way, I just don't feel nasty about it any more. I used to want to play the game with the other kids. But they showed no interest at all in playing with me -- or even in fighting with me. I couldn't even bait them into a fight. Once thay did make fun of our game in public (Fowler and Turvey wrote the only article I know of that explicitly set out to show that Powers' PCT model could not handle a particular coordination task; they got the model all wrong and were wrong about the coordination task too). We tried to fight back at them, but they continued to blithly ignore us. So now many of the other kids think our game is full of it. They don't want a fair fight, winner take all. And they don't want to play. So I'm happy playing the control theory game with the few friends I can play with. I guess I figure if the other kids are going to ignore me, I'm going to ignore them. Besides, I think it's really a waste of time trying to explain why my game is better than theirs. As Bill Cunningham explains, PCTers should know that our game is not better than theirs is FOR THEM -- because their game satisfies their reference for the kind of game they want to play. It might be fun to have more players in our game, but I'm not a very sociable guy anyway. Besides, it's more fun for me to play my control theory game with other people who really know how to play it and who really want to play. I don't want to play with people who have been brow beaten into joining my game, or who want to play my game to spite the kids who are playing the other game, or who want to play my game in order to save their souls. I want good players who want to play. I am also less worried about being co-opted when I play with skillful people. If a group of kids decides that they've been playing my game all along -- but in a more important way -- and that my team is just a subset of their league, then that's fine with me. My team will just keep playing great control theory; unless the other team is also playing great control theory, there is not going to be a good mesh. That ought to become apparent very quickly -- and we won't be picked to be in their league any more.

> It's only at that kind of level where we can find ourselves
>solidly on the same side. There has to be some meta-context, as Joslyn
>would put it, that makes the gain or loss of a particular paradigm only
>an incident on the way to doing something more important.
>Or am I the only one who has trouble hanging on to this point of view?

I agree. It is very difficult to avoid the competitiveness of science. But that competitiveness is also part of the fun (as you noted-- people do seem to need some conflicts for their consciousness to work on). So, while I like grazing in relative solitude in the beautiful pastures of PCT, I still succumb occasionally to the "bad boy" instinct that wants to poke a bit at the establishment point of view. I guess I am just less frustrated now when my pokes yield little more than an irritated shrug.

This brings me to the talk I gave yesterday to the Human Information Processing Group at UC Santa Barbara. I think the talk went well. The audience consisted of faculty and graduate students in psychology. There were lots of questions. I think the main question was "how is this different from conventional control theory -- a la the manual control theorists or eye control models, etc". I don't know if I made the answer clear -- the difference is formally small but philosophically huge. I tried to explain that PCT empahisizes the importance of the subject's secularly adjustable references for the state of perceptual variables.I also tried to explain that a major goal of control theory is to discover the perceptual variables that subjects control and how those varioables are related to one another.

All in all the talk was very well received. The 'mindreading' demo went very well. I don't know if I explained it well enough but it was clear to the audience that the computer was detecting the intentionally moved number when they could not tell which it was by looking at the behavior of the numbers on the screen. My impression is that the audience (of very bright people) found the ideas new and interesting -- but not necessarily a challenge to the current paradigm.

All in all, it was great fun; very pleasant.

Bill Powers -- at least one person (Jack Loomis, a perceptual psychologist) was very interested in getting a copy of your "Little Man" demo, which I described to him. How can he get it? How much is it?

Thanks

Rick

Date: Thu Oct 17, 1991 1:40 pm PST
From: marken
Subject: Where's the opposition

[From Rick Marken (911017b)]

Following up on an earlier comment of my own:

> (Fowler and
>Turvey wrote the only article I know of that explicitly set out to show
>that Powers' PCT model could not handle a particular coordination task; they
>got the model all wrong and were wrong about the coordination task too).

What I meant, of course, is that this is the ONLY article I know of where anyone (other than a PCT control theorist) has tried to falsify the PCT model. This article by Fowler & Turvey should be reprinted in a collection of PCT related papers (the reference is C. Fowler & M. Turvey (1978) Skill Acquisition .... In G. Stelmach (Ed) Information processing in motor control and learning (pp 1-39) New York: Academic Press). This is really a must read for PCT people. Fowler and Turvey actually carry out the calculations to show that a hierarchical control system cannot work at all (funny, I got it to work ok). Powers wrote a reply to them -- which, as far as I know, was completely ignored. The only other place I remember seeing some mention of what PCT could not do was in a paper on operant conditioning by Meyerson and Meizen(sp?) in Psych Peview. But they just said "PCT can't do it". Fowler and Turvey really go at it -- spending at least 7 or eight pages making complete fools of themselves (in the eyes of PCTers only, apparently) by "showing their computational work" for the world to see and then slouching off to the obscurities of coordinative structure and dynamic attractor theories, as though PCT had been proven wrong.

What PCT needs is many more attempts, like that of Fowler and Turvey, to prove that it is wrong. The potential PCT revolution has suffered far more from being ignored than from being "disproven".

Hasta Luego

Rick

Date: Fri Oct 18, 1991 2:29 am PST
From: LO BOUR
Subject: Re: Oculomotor models

Dear Bill,

You had a pretty long story on your efforts modeling the oculomotor system. As far as the role of the strech receptors concerns, to my knowledge, these strech receptors do exist in extra-ocular muscles but nobody knows when or where they are used for. Anyway, under normal conditions and during daily life they are not necessary since the eyeball always excerts the same load to the muscles. Only in case of preventing the eye from moving mechanically (which is not natural of course) this occurs and as you mentioned then the visual world starts moving. The classical explanation for this phenomenon is, as far as I know, that there is such a thing as an 'efference copy' signal. This is a neural replica of the efference command going to the muscles and it is feeded back (the local feedback loop of Robinson's model) to the central nerveous system and then is compared to the desired end position. If there is a conflict between this efference copy and the expected visual motion then the world starts to move. That's why patients with a eye-muscle paresis may become dizzy.

So the nerveous does not check eye position by means of stretch receptors although there are numerous of them in the extra-ocular muscles. The check is performed on a short term basis by the efference copy signal, and on a long term base it is mainly performed visually.

The interaction between saccades and smooth pursuit really is an intruiging question. The classical concept of two independent systems is in my opinion not valid. The central nerveuos system although equipped with the possi-bility of different capabilities (what normally is called systems) in fact also is able to react as a whole. That means that the 'switching' you mentioned is possible by means of certain kind of inhibition. As an example will give you the hierachy in inhibition with respect to smotth eye movements. The most primitive smooth eye movement is indu-ced by vestibular stimulation (VOR), then there is the optokinetic response (OKN) which may suppres the VOR. Then we are able to pursue a foveally presented target superimposed on a background which moves to an other direction. In this case there is suppression of the OKN by foveal pursuit. Pursuit is on the top of the hierarchy and may switch off any system below (don't ask me how). Pursuit is also able to VOR-suppression. I don't think however, that you need stretch receptors for this switching process.

Morevover the separation between saccades and pursuit is not so strict as sometimes is thought. A small positional error may induce a smooth movement (slow control 'system').

The fact that saccades doe not induce a extra smooth pursuit may have several explanations. An important one to me seems that saccades are detected visually by a large delay (about 100 ms). The smooth pursuit system needs this visual input in order to function. So any action of the pursuit system would become too late.

Your modeling may probably usefull for innervation of limb muscles. Stretch receptors do play a very important role in the control of arm and leg muscles. But not only stretch receptors (muscle spindles) also Golgi-tendon organs, mechanical receptors play a role in the control of limb movements.

Did you publish something about your model ? or did you write anything ? about which we could discuss ? At our department we are now attempting to develop a neuronal model for lower limb motor control. So maybe one of your ideas might be of interest.

Greetings, Lo Bour

Date: Fri Oct 18, 1991 3:32 am PST
From: LO BOUR
Subject: Re: Robinson's oculomotor model

Dear Rick,

You had an awfull lot of questions which cannot be answered in a quick way. But in order to be a little more clear (otherwise read for instance Handbook of Physiology on Oculomotor Control by David Robinson) I will give you some more cues.

- The Robinson model tries to give an explanation of the control of saccades. Since saccades mostly correct for a positional error the model explains the control of eye position.

- The model is tightly linked to neural substrates existing in the brainstem. That means that it incorporates actions of certain cells which have been identified in the brainstem, such as burst-cells, omni-pause neurons, tonic neurons, motoneurons etc. The burst generator then is the neural structure in the brainstem that generates the phasic innervation necessary to make a saccadic eye movement (very fast movements).

- The local feedback loop exsists in the central nervous system and enables an immediate control on the outgoing oculomotor command. With respect to the environment on the short term there is no feedback. This local feedback loop makes use of the classical concept of the 'efference copy' signal already first proposed a century ago by Helmholtz.On he long term there is the possibilty of a visual correction (after 100 ms).

- I talked about head-coordinates and retinal coordinates. These concepts of course are important if one relates motor output strictly to sensory input. However, there also things as internal representation of the environment, remembered target positions etc. which also may lead to a saccade. The Robinson model does not pretend to give an explanation for those kinds of inputs. It is only a model for the explanation of the brain- stem actions with respect to saccades. It is more or less a final common pathway for all kind of cortical process who wants an eye movement.

Hope I fulfilled part of your problems
with the Robinson-model Lo Bour.

Date: Fri Oct 18, 1991 7:23 am PST
Subject: Whose X

From Bill Cunningham (911018.0930)

Bruce Nevin (911017.1202)

Sorry I can't extract text/insert comments for reply. That sure makes for single cohesive post.

Your point on whose X is well taken. I was thinking of the audience who is working diligently to misperceive the presenter. The presenter can be happy only if s/he perceives audience acceptance. If that is an unreasonable expectation, then presenter is doomed in advance.
I like your expansion of control. That's sort of how I've tried to explain it, but lacking the "whatever it takes is called behavior."

With respect to the analogy, yes A is the radar which tracks its perception of radar returns from P. It doen't matter to the radar whether the perceived return represents the real aircraft/position. What does matter to the radar is that the return is withing its range of control. So long as that remains true, the radar will control its perception of target/position to match its internal reference. The analogy doesn't break down exactly because "A comes to track what it expects the target to be..." Tracking (control) ends when the radar (A) realizes that it has incrementally followed a target/position to a state that contradicts its principle fixed set of references. Then all hell breaks loose--particularly if it tries to continue track. Eventually a higher order control system overrides chaos and says "You've lost track, dummy. Go back to the search mode." That's easy with radars and hard with people.

I probably should have expanded on the analogy. Radars don't just track aircraft spontaneously. They are dependent on an acquisition radar whose function is to detect the target within all the clutter and establish a position estimate whose uncertainty is within the control limits of the tracker. The acquisition system rejects potential targets which don't meet that criterion, and it generates some false targets too. The latter are ultimately untrackable, but the tracking system will try because they have been presented as "meeting the a priori track (control) criterion."
Meanwhile, back to the presenter. If the initial statements diverge wildly from preconceived references, they are rejected immediately and there is no attempt at tracking. Worse, since the complex human has more than one reference, there is a distinct possibility the audience will perceive the statements to be within the control range of the WRONG reference--and all that entails. I suspect an early rejection decision defaults to a reference that says "This turkey is full of it--track the presentation exclusively for counter arguments."

I certainly have enough bruises from other battles to show I don't have a solution to the presenter's problem. I used to abhor politicians, but have come to respect their ability to frame THEIR agenda in terms acceptable to their audience. I'm not referring to the underhanded, unprincipled, side of politics--just the process of converging two apparently divergent agendae. Unfortunately, science is a political process.

For Bill P, great empathy. I've picked up your expression "dripping on that stone." I can be pretty damned competitive. Slowly, I'm learning to reorganize my reference from "all-out victory" to "influencing things in the direction I want", aided by your remarks that I can't totally control others anyway. Reframing this way at least lowers my blood pressure.
Bill C.

Date: Fri Oct 18, 1991 9:16 am PST
From: CHARLES W. TUCKER
Subject: approaching deadline - any takers????

SOUTHERN SOCIOLOGICAL SOCIETY

ANNUAL MEETING

APRIL 9-12, 1992

NEW ORLEANS

CALL FOR PAPERS

The theme of this annual meeting is "Will the Center Hold? Linking Sociology to Its Specialities and other Disciplines." One of the questions posed by the President of the SSS that is relevant to this session below is: "How do they utilize the findings and theory from other disciplines and incorporate them into sociology?" I would like to see some papers discussing not only another area (Cybernetics) has influenced sociology or social psychology but also how the reverse is the case or a challange to the claims that there is any influence either way. Any type of paper from abstract theoretical to refined experimental is appropriate in this session but what I would like to recieve is an abstrast for such a paper or a paper that can be presented in about twelve minutes (about six double spaced

page) rather than some paper that will have to be reduced before
the meetings. The title of this session is:

CYBERNETICS AND SOCIAL CONTROL

Please send abstracts or papers to me by OCTOBER 15, 1991 by either snail or Email.

Chuck Tucker
Department of Sociology
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HOMES: (803) 254-0136 or 237-9210

Date: Fri Oct 18, 1991 9:23 am PST
From: CHARLES W. TUCKER
Subject: The SSS meetings

FROM CHUCK TUCKER 911018.12:48 EDT

I know that I "owe" a number of you several post but I am not totally prepared to send those at this time. One of the deflections that I am dealing with is a session for the SSS meeting in April, 1992. AS a notice I just sent (for 2nd or 3rd time?) indicates I have a session on "Cybernetics and Social Control". I was hoping that someone in our group would want to present a paper at this meeting on control theory and its many aspects. If not, my most recent thought is that we could cooperate on a statment for the meeting entitled: "The Myth of Social Control". This paper has been written to a great extent through our conversation on this issue on the NET and may in fact be the topic of a Closed Loop in the future but I think the issue should be presented to sociologists at one of their meetings. If anyone would like to work on this issue let me know and I will put it one the program; if I get no takers I will just leave it off the session.

Regards,

Chuck

Date: Fri Oct 18, 1991 1:38 pm PST
From: marken
Subject: Re: Robinson's oculomotor model

[From Rick Marken (911018)]

Lo Bour says:

>The local feedback loop exsists in the central nervous system and
> enables an immediate control on the outgoing oculomotor command.

Why would one want to control an outgoing oculomotor command? Especially since the effect of that command is bound to vary even if the command itself is perfectly consistent?

> With respect to the environment on the short term there is no
> feedback. This local feedback loop makes use of the classical
> concept of the 'efference copy' signal already first proposed
> a century ago by Helmholtz.On he long term there is the
> possibilty of a visual correction (after 100 ms).

If 'efference copy' is part of the Robinson model then I am surprised that it is even called a control model at all. The whole point of a control system is that it works in situations where outputs (efference) cannot possibly be expected to have anything like a precise, quantitative effect. The closed negative feedback loop obviates the need to design systems that produce precise outputs or to operate those systems in environments where those outputs will have precise effects (such as the environments provided by biological systems). I cannot imagine how a nervous system would profit from having access to information about the outputs it has generated (efference copies). If any such connections ever existed in a nervous system they were probably eliminated quickly by learning or evolution. Control systems control afference; efference is mainly needed to supply the specifi-cation for the required level of afference. In a control system organization, the only information the system needs about efference is its effect (which can be quite indirect) on afference. In fact, all the system needs to know about is the afference. It need not know anything about efference levels or how efference levels effect afference levels (it could not possibly know the latter anyway).

Maybe I'm missing something. Why would anyone who understood control and control theory develop a model that included the notion of efference copies, reefference or whatever? Why does Robinson think such a concept is necessary (if he does)?

I had hypothesized that the Robinson model was an example of an input-output model disguised as a control model. Based on your description, I now hypothesize that it is an output generation model disguised as a control model. I'm sure this is not a completely accurate characterization -- but with "efference copies", what else could be going on?

Best regards

Rick

Date: Sat Oct 19, 1991 7:31 am PST
From: POWERS DENISON C
Subject: Radar; oculomotor systems

[From Bill Powers (911019.0700)]

Martin Taylor(911017) --

Martin, welcome back, especially bearing such a nice piece of information (but that's not a condition). With a signal delay of 10 milliseconds, the control of phoneme production could have a bandwidth of something like 50 Hz maximum. I think it looks feasible. This is especially true if we assume that some of the "features" of phonemes are not actually controlled, but are simply side-effects of transitions from one controlled articulator configuration to the next. Not every effect of an output is under control: only those being perceived. The sound spectrograph, of course, can't tell the difference between controlled and uncontrolled aspects of phonemes.

I suspect that somewhere in the masses of data that must be available, the required information is sitting there waiting for a control theorist/linguist to look at it. Which variations make a difference in comprehension, and which don't? What kinds of sloppinesses are corrected, and which aren't? The artificial perceiver can ignore those that don't make

a difference. It's possible that the solution to perceptual control of speech is going to be simpler than has been suspected.
I'm thinking of a behavior like throwing a ball. We surely don't have reference-positions for every different arm-configuration between the windup and the end of the throw. Just a few key configurations, velocities, and forces, aimed and timed properly.

Bill Cunningham (911018) --

The "radar" model is nice, very suggestive. I'm reminded of Skinner's "shaping" method and wonder if something similar isn't involved. You can't get a chicken to walk in figure-eights by just rewarding it for the final result. You have to make the stages small. Basically, you let the chicken control the reward by using a behavior it's already producing. Then you change the environment a little, so a slightly different behavior is needed to control the reward. If the difference isn't too large, reorganization will have to make only slight changes to discover the new workable output. In this way you let the chicken recover control quickly after each change. This means that the chicken never loses its lock on the target completely or for long.

What the presenter has to do is make sure the audience never loses the feeling of controlling for meaning. How about a progression like this:
1. Stimulus -> response. Inputs affect outputs via the organism, just as everyone already believes. The sight of the nest seems to cause the bird to move toward the nest (Watson's big insight).

2. (Stimulus - reference) -> response. The effective stimulus is the difference between the presented stimulus and a "neutral" state -- the state of the stimulus that produces no response. Sometimes the "neutral" stimulus is "no stimulus" but sometimes the reference state is not zero -- adaptation level, for example. Distance from bird to nest gets larger or smaller. Neutral state is the distance that leads to no further movement of the bird. May be zero or nonzero. Maybe the bird stops at the edge of the nest instead of climbing into it.

3. Response -> next stimulus. This is "chaining" which everyone has heard of. The bird's movement alters the distance to the nest which alters the stimulus for the next movement.

4. Two possibilities: the difference wrt the neutral state is made bigger, or it's made smaller. This is a feedback effect. If the difference is made bigger the experiment is soon finished. If it's made smaller we can go on.

5. Introduce continuous variation of stimulus and response. The changing distance between nest and bird is the stimulus, and the changing position of the bird is the response.

6. Now make the time-steps smaller. The slightest movement toward the nest slightly affects the distance-minus-reference stimulus. Even in the middle of taking a step, the distance is shrinking and the movement is continuing. Perception works much faster than movement.

7. Introduce quantitative measures. The speed of movement toward the nest, once the bird is within a certain radius from the nest, decreases as the distance gets smaller. If it did not, the bird would overshoot the nest.

8. Introduce another dimension of control: orientation toward the nest. Same steps. Now the bird can approach the reference-position from any direction.

9. Make the target a piece of grain randomly placed on the ground. Steering toward the grain works the same way.

10. Put a pile of grain on a little moving cart that can be randomly pulled around. Note that the movements of the (hungry) bird will keep the distance and direction at their respective reference levels, even though the detailed movements can't be predicted without knowing where the cart will be pulled. Also point out that the grain itself isn't changing; only its spatial attributes (distance and direction relative to the bird) are changing.

11. Note that if the bird succeeds in staying close to the grain, its movements keep relative distance and direction of the grain nearly constant. The movements of the bird prevent the spatial attributes from changing enough, relative to the bird, to prevent its eating the grain.
And so on.

This approach feels right -- you don't start out by talking about control theory. This would be like expecting the audience to start moving in figure-eights immediately.

Of course you have to know where the audience is starting from. I once gave a weekly seminar to a bunch of bright undergraduates (Hugh Petrie and Don Campbell make it possible), and started by contrasting control theory with stimulus-response theory. One of the first questions was, "What's stimulus-response theory?" So you have to spend some time finding out what the audience already believes about behavior, if anything. If they don't believe anything, there'll be no problems. Just tell them about control theory, and they will say "Oh. Doesn't everyone know that?"

Chuck Tucker (911018) --

You didn't mention the magic words, Chuck. Who pays for travel and expenses? The idea of a presentation to sociologists about our "social control system" thread is excellent and it should be done. I couldn't afford it, though. Maybe another time.

In the light of the Cunningham radar analogy, to take a somewhat longer view, what do you see as the stages by which we could get from existing sociological beliefs to control theory? I'm really very taken with this analogy; it gives me a picture of how we might go about getting the mainstream disciplines to start bending in the right direction. One little step at a time, instead of trying to get them to make the Great Leap Forward. Maybe we're creating the resistance ourselves by asking too much. If you grab the chicken and move it in a figure eight, it will just try to escape.

Rick Marken (911018) --

Rick, I think the eye is essentially the only example of a moving part of the body that normally can't be disturbed. Efference copies won't enable true control of the eyeball, as you say, but if the eyeball responds to muscle tensions in a way that's nearly repeatable, optical feedback will take up the slop. Saccadic eye movements are pretty sloppy; they stop within a couple of degrees of the target. But pursuit tracking is accurate within minutes of arc; it uses the full angular resolution of vision. See my comments to Lo Bour below.

Lo Bour (911017,18) --

I'm delighted to learn that the stretch receptors exist in extraocular muscles! When was this finally established?

Given stretch receptors, and of course Golgi tendon receptors, all that's necessary is to combine them just as they are arranged in the nervous system: stretch signals excite the motoneuron, and tendon signals inhibit it. I'll dig up my equations (if I can find that paper) and post them. I would be very happy if someone would carry this model forward; just give me a footnote.

When I began work on this model I was worried because in the dark the eyeball shows only a weak opposition to deflecting forces. Robinson and others just assumed that this was due to the passive elasticity of muscles and sclera. I wondered how there could be stretch feedback and still no opposition to disturbance. But I went ahead anyway and just followed through with the analysis. It was a great surprise when I discovered that the stretch and tendon feedbacks could combine (with suitable adjustments of sensitivities) to produce a VERY SMALL resistance to disturbance. This required loop gains to be very HIGH, not low; the net feedback effect was due to the ratio of the two feedback sensitivities. If this ratio was near 1, the effect was that of a weak spring return: the ratio of the two feedback gains times the muscle's passive spring constant, if I remember correctly. So it is possible that the observed weak resistance to disturbance is not due just to passive components, but comes partly from the feedback loops.

Rick, take note: the eyeball does NOT resist mechanical disturbances as you would expect a position-control system to do.

Anyway, I will try to find that stuff. This model really does a lot of things correctly, including the illusions of movement, effects of the vestibular reflex, switching between pursuit and saccades, and so on --essentially everything I knew about at the time. Some details are probably wrong -- neural circuit information would be a great help in correcting such mistakes.

Greg Williams, do you want to get in on this? Come to think of it, do you still have that box of oculomotor material?

Lo, it is good to have you on this net -- you'll find a lot of interest in this subject. There isn't much about human behavior we're not interested in. If we can keep this up, we'll get that "one science of life" that I dream about.

Date: Sun Oct 20, 1991 5:49 pm PST
From: Bill CUNNINGHAM - ATCD-GI
Subject: chickens, opportunity

FromBill Cunningham (911020.1645)

Bill Powers (911019.0700)

By George, you've got it! The key phrase is "allowing reorganization at each step." This agrees completely with coaching principles for introducing and building up skills/the ability to execute them in the face of increasing resistance. Reorganizing to restore comfort at each stage is essential. But it's the audience/player that has to reorganize (shades of some coaching mistakes).

Knowing the audience start point is essential not only in the way you describe, but to provide a "step zero" in your outline. This is where you say "I'm going to start with something you know and are comfortable with. I then introduce several new conceptual tools, well known in other circles but not usually applied in your field. When you understand these tool, you will be able to see what you already know in a new light." Would you like a piece of candy, little girl?

Other comments:

Step 2. Watch out for "effective stimulus". I know exactly what you mean, but a term like "effective input" might lose them. One of my less glorious explanations went "The output of the summing junction forms the effective input for the active part of the system." Result: Lions 72, Christian 0.

Step 4. You may want to talk about positive feedback at some point. This may have some meaning to sociologists because it leads to instability. PCT is based on negative feedback and stability. Change requires instability. Come to think of it, reorganization results from recognition of instability by a higher order control system (my interpretation) that directs revision of references to restore order at the lower level. My interpretation may be blurred, but your outline doesn't address reorganization. Sooner or later, that bridge has to be crossed. For me, it is the difficult part of PCT.

Step 6. Time steps-->continuous. Possibly another toughy for the audience. Gareth Morgan argues against control in organizations, but his argument centers around what I call a ballistic system. You aim the gun as best you can and fire. There is no control of the projectile on the way. You can engage in an iterative correction, but conditions will have changed (tube warming effects muzzle velocity , etc). Even with iterataive approach, each shot is not controllable after launch. Now consider the machine gun with a tracer every 3 or 4 rounds. Faster rate of fire, faster perception of result--but still no control on a specific round after it is fired. Now consider fire hose. The individual droplets are also ballistic, but the continuous stream gives perception that impact point is under continuous control when it is the nozzle position/angle that is really under control. My understanding of S-R is that it deals with discrete, ballistic events. An iterative approach is possible, but that's not the same as control. You may have to help audience over this hump.

Possible example for sociologists: Do you know the "Trip to Abilene" story? I'll repeat if you haven't. Author is sociology professor for U Texas. Story is very funny, and used in management training sessions to illustrate "groupthink". Point of story is that group collectively makes a stupid decision because each member doesn't want to rock boat. We would say that each member is controlling for group harmony rather than for a good decision.

I just returned from a government sponsored symposium on "data fusion" which is the generic name given to the (organizational and machine) assemblage of sensor generated information into a cohesive picture of the environment--sufficient for decisions. The sensors are heterogeneous, assynchronous, and generate their own errors. Making sense out of all this is far from trivial. This particular group is working to formalize the approach. They have published a model that describes multiple levels of what we would call perception. Their model does not explicitly include decision criteria (references) at each level and omits a process for revising the decision criteria based on some higher level perception. My personal agenda is try to bend their model so that it resembles your hierarchical model--which I think it should regardless of PCT.

One presenter was what we burocrats call a "human factors" guy. I'm not sure of his exact credentials. His paper on human decision making under stress could have easily been given in PCT terms. Several of us made a point that this group is busy building & describing complex "data fusion" systems that will provide information to a real live "data fusion" system. And they don't have the foggiest idea how the live system works. Several of us told the organizers they needed to extend the model to include the human system. THE PAPER IS BLANK!! I described PCT briefly to Jeff Grossman, the human factors guy. He was interested. His agenda is to make people include the human in the overall system. It strikes me that a machine/organizational model that conforms reasonably with PCT would certainly fit nicely with a PCT model of the human. Users of the composite model would have one thing to learn. Interested? It would be a slow process, but not impossible. While the result would be initially confined to a rather restricted military set, the organizers are very aware that their work applies to many things in the information age--nuclear power control/safety, environmental monitoring, etc. This might not beat down the academic resistance to PCT, but might put it to useful work. Sorta like the bumblebee that doesn't know its theoretical inability to fly.

Bill Cunningham

Date: Sun Oct 20, 1991 8:19 pm PST
From: Bill CUNNINGHAM - ATCD-GI
Subject: Aw shucks

From Bill Cunningham 911019.2010

Flog me with a wet nozzle. No, the nozzle is not controlled, but it is under continuous influence. The cannon is influenced only at the instant just prior to firing. Any control over the cannon situation (indirect fire) lies with alligning the perception of angle measurements with calculated references. The gunner can't even see the target. You could actually do the same thing with the fire hose, but in fact we don't. If the travel time of the droplets is short, we control for zero miss distance. If the travel time of the droplets is long, we fall back to the artillery situation where the nozzle angle is controlled to some reference we *predict* will result in a wet target.

Date: Sun Oct 20, 1991 9:24 pm PST
From: Gary A. Cziko
Subject: Tolstoy on the Bee's Purpose

[from Gary Cziko 911020.2130]

A few weeks ago I had an informal chat with an esteemed senior colleague about the role of purpose in behavior. He was arguing that purpose is a pretty useless concept for understanding behavior. Needless to say, I found it very difficult to understand his viewpoint. Then just a few days ago, he sent me this excerpt from Tolstoy's _War and Peace_.

****
A bee poised on a flower has stung a child. And so the child is afraid of bees and declares that bees are there to sting people. A poet delights in the bee sipping honey from the calyx of a flower and says the bee exists to suck the nectar of flowers. A bee-keeper, seeing the bee collect pollen and carry it to the hive, says the object of bees is to gather honey. Another bee-keeper, who has studied the life of the swarm more closely, declares that the bee gathers pollen-dust to feed the young bees and rear a queen, and that it exists for the propagation of its species. The botanist, observing that a bee flying with pollen from one dioecious plant to the pistil of another fertilizes the latter, sees in this the purpose of the bee's existence. Another, remarking the hybridization of plants and seeing that the bee assists in this work, may say that herein lies the purpose of the bee. But the ultimate purpose of the bee is not exhausted by the first or the second or the third of the processes the human mind can discern. The higher the human intellect soars in the discovery of possible purposes, the more obvious it becomes that the ultimate purpose is beyond our comprehension.

Man cannot achieve more than a certain insight into the correlation between the life of the bee and other manifestations of life.

*********** NOW I better understand my colleagues attitude toward purpose. I don't have time to comment on it now, but I'm sure that people like Powers and Marken will not be able to keep their hands off this one (but do bee careful; although stinging comments may well be appropriate here)!

--Gary

Date: Mon Oct 21, 1991 10:43 am PST
From: TJ0WAH1
Subject: oculomotor system

[from Wayne Hershberger]

Rick Marken: I think you are right. It is probably easiier to publish control system models of limited phenomena, such as eye movements, than it is a generic model of behavior in the first degree. For instance, the ms. I submitted to the _American_ Psychologist_ that you asked about has been soundly rejected by all three referees, plus the editor for the special issue, which is being devoted to B. F. Skinner . As one of the referees put it, there is no reason for the journal to provide Hershberger an opportunity to continue his debate with Skinner. I guess he thought it would be unfair to Skinner because he can't talk back. It is good to hear that your ms. is still under consideration.

Bill Powers: Last week I mailed you a copy of an article by Robinson describing his oculomotor model. I would be interested in hearing your comments.

Cooper and Daniel found muscle spindles in human extrinsic eye muscles in 1949. Even those animals whose extraocular muscles have been found to lack spindles have ocular muscles supplied with a variety of afferent nerve endings. What is absent is the stretch REFLEX not the stretch receptor.

> For instance, Keller and Robinson (1971) monitored the activity of 38 motor neurons in the abducens nuclei of 3 Rhesus monkeys and were unable to find any reflexive activity when they stretched their subjects' lateral rectus muscles. Since their single-cell recording techniques probably monitored only the larger phasic fibers, the tonic fibers may evince a stretch reflex (as some research with rabbits and cats suggests: Baichenko, Matyushkin & Suvorov, 1967). However, if there is such a "tonic reflex" in primates, its utility remains obscure, because there appears to be no type of eye motion in which the phasic fibers do not participate fully.
>
> Although extraocular muscle may not itself exhibit a stretch reflex, stretching an extraocular muscle does have remote neural effects in diverse parts of the brain, including various layers of the superior colliculus, the cerebellum, the visual cortex, and the frontal eye fields (Howard, 1982). However, the functional significance of this muscle spindle afference is not yet clear (Hershberger & Jordan, in press).

Bill, I am virtually certain it was the results of Keller and Robinson's 1971 experiment that helped shelve your modeling of the oculomotor system in 1972. That implies that the lack of the reflex rather than the lack of the receptor was the problem. However, I'm puzzeled now why that should have been the case. The other experimental findings that caught us up short, as I recall, were from 2 experiments by Skavenski, Haddad, and Steinman (1972) which demonstrated that stretching the lateral rectus muscle by applying a rotational load to a scleral contact lens does not shift (displace) the visual direction of a point of light flashed briefly in the dark unless the stretched muscle contracts to compensates for the load.

In one experiment, carried out in the dark, only one eye was used--the left one, the right eye was occluded and unencumbered. The subject fixated a stationary red point of light as the eye was loaded with 4-9 g thereby tending to rotate it to the right or left about 3-8 degrees, respectively (the eye did not actually rotate; i.e., fixation was maintained). The subject moved a dolly bearing a point source of white light, so that it appeared to be located straight ahead. When the eye was torqued to the left, the white light had to be moved to the left of the red fixation target to appear to be staight ahead; and vice versa. In other words, the red fixation light imaged on the fovea appeared to be displaced to the right by a load to the left, or more exactly by an endogenous torque to the right offsetting the load. (I think I've heard you mention this experiment many times since 72).

In the second experiment, also in a darkroom, a subject fixated a red point source visible only to the LEFT eye, for about 3 s. When the red light went out, a white point source, visible only to the RIGHT eye, flashed for 125 ms. Immediately, the sequence was repeated, again and again. When the red light was visible, the subject was to move a dolly carrying the flashing white light so that eventually it would appear to be located straight ahead. Although the left eye continually fixated the red light, and the right eye was rotated about 8 degrees from this fixation axis by an angular force of 7 or 9 g, the subject's judgments of the visual straight ahead did not differ from those made when no load was applied (the subject's judgements were corrected for the shift in the retinal image in the right eye caused the rotational load applied to it).
More later, got to run.

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Hershberger, W. A., & Jordan, J. S. (in press). Visual direction constancy: Perceiving the visual direction of perisaccadic flashes. In E. Chekaluk's (Ed.)_The_role_of_eye_movements_ in_perceptual_processes_. Amsterdam: Elsevier/ North Holland. --a preprint of this was available in Durango in August, Bill, did you get a copy?).

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Warm regards, Wayne

Date: Mon Oct 21, 1991 11:40 am PST
From: POWERS DENISON C
Subject: teaching; bees

[From Bill Powers (911021.0730)]

Bill Cunningham (911020) --

Roger on Step Zero.

>Step 2. Watch out for "effective stimulus".

Right. This chould be explained in terms of temperature. The neutral temperature is obviously not 0 on any scale. It's the temperature reading at which the person tries neither to get warmer nor to get cooler. It's probably best to stick with reference *levels* (operational definition) and postpone the model's reference signals until someone asks what determines the reference level.

>You may want to talk about positive feedback at some point.

Considering the way the term "positive" has been contaminated, maybe we need to speak of "error-reducing" and "error-magnifying" feedback. Maybe it's good to make this distinction early so people will know we're only talking about the case of error-reduction.

Reorganization should probably be left until after the basic relationships are u nderstood. Likewise for the hierarchy. People always see the problems and ask the questions, though. Maybe in the beginning it would be enough to say "Yes, that has to be handled. What you're talking about is [reorganization, the hierarchy of control]. We can get to that after we get through some basic idea s."

>For me, [reorganization] is the difficult part of PCT.

Here's a program you can write yourself. Set up a dot on a screen that moves at constant slow velocity and at an angle theta. deltax is proportional to cosine theta, deltay to sine theta. Put limits on x and y: if x < 0 or x > xmax then deltax = - deltax, and ditto in y. On each program iteration, x = x + delta x, y =- y + delta y.

Check for keystroke on every iteration. If a key has been hit, read the keyboard and discard the result. Pick a new value of theta at random from the range 0 to 2Pi radians. Continue the iterations.

Before starting the iterations you can draw a target on the screen, or just mark one with grease pencil or a Postit.

You can now "reorganize" the moving dot to reach any target you like just by pre ssing the space bar. Yet all you can affect by your behavior is WHEN a random reorganization of behavior will occur. You have no control at all over the angle theta.

The "moving dot" can be a parameter of a control system. The "position" you are monitoring can be any characteristic of control such as mean error signal. The means of control is to trigger a random reorganization. If the result is worse, trigger another one right away; if the result is better, wait a while before triggering another one.

Tom Bourbon, how about discussing your experiment with this principle?
Mailing time approaches -- will take up rest later.

Gary Cziko (911020) --

Actions have multiple effects. Only the effects that are perceived and compared with a reference condition are controlled. The rest are side-effects. Tolstoy's bee behaves in ways that have different effects relevant to different people. Some of these may be produced on purpose and many are probably not. The way to tell if an effect is being produced on purpose is to introduce disturbances, and see whether behavior changes systematically so as to overcome the effect of the disturbance and produces the same effect anyway. It isn't always possible to do this in a naturalistic setting. If no natural disturbances occur, you may have to conclude that you don't know whether a given effect of action is purposive or not.

All organisms have multiple purposes; sometimes they are being pursued visibly a nd sometimes not. Even E. coli can control for the concentrations of 27 different substances. E. Coli can thus have as many as 27 purposes at a time. To ask what a bee's purpose is is to assume that there is only one, and that the bee spends all its time pursuing that purpose (and the pursuit must be unsuccessful; it continues indefinitely only if it fails to correct the error).
Attributing purposiveness to any behavior that affects you is a characteristic o f paranoia. "Why did you make me spill my coffee? Are you trying to make me look foolish?" A person who understands control theory would not make that mistake. Control theory often lets you distinguish intended from uninitended effects. When there's insufficient evidence, you withhold judgement.

>I'm sure that people like Powers and Marken will not be able to keep
>their hands off this one

There are no people like Powers and Marken.

Best

Bill P.

Date: Mon Oct 21, 1991 12:43 pm PST
From: marken
Subject: Efference copies, learning, surprise find

[From Rick Marken (911021)]

While pushing on my eye yesterday morning I remembered why Helmholtz came up with the idea of efference copies (he must have spent his sunday mornings the same way). The efference copy is needed to tell whether the image has moved because your eye moved or because the world moved. It is probably also needed to turn off processing of the image during a saccade so you don't get a blur. I seem to recall that if you curarize the ocular muscles and intend to look to the left (say) the world seems to shift to the right (as it does when you push the eye to the left).

OK, so I admit that efference copies are needed for modeling the visual system. But I still maintain that they are really not involved in visual control per se.

Re: The discussion of strategies for teaching people about control theory using the "successive approximations" approach of behavior mod. All this assumes that the audience has some very good reason to learn about control theory. Remember, behavior mod works only when the organism has a need for something (the reinforcer). I like the successive approximations that Bill Powers and Bill Cunningham have laid out. But I would be willing to bet a whole lot that, no matter how sensible and helpful the steps, you are not going to get people to learn control theory when they don't see any need to learn it. Most people don't even recognize the existence of the phenomenon that control theory explains -- the phenomenon of purpose. Gary Cziko's (911021) eminent friend certainly illustates this point.

Bill Cunningham (911020) says:

>One presenter was what we burocrats call a "human factors" guy.
That's what I am known as here at work.

>   I described PCT briefly to Jeff
>Grossman, the human factors guy. He was interested. His agenda is to
>make people include the human in the overall system. It strikes me that
>a machine/organizational model that conforms reasonably with PCT would
>certainly fit nicely with a PCT model of the human.

I once wrote a piece on the relationship between human factors and PCT in the Bulletin of the Human Factors (HF) society. Much of what HF people do is pleasantly compatible with a PCT view of organisms. The article was pretty well received, but one of the gurus of HF research wrote in to pan it. Don Norman (another HF guru of sorts) has a model of the human operator that comes as close as possible to being a PCT model without being PCT. People just don't want take the one little step to PCT enlightenment. I think they don't want to believe that the problem of purpose was solved years ago by a relatively simple model.

One last little point. While looking through some old papers I was surprised to find a copy of a paper by Wayne Hershberger, written in 1972, which criticizes the re-efference concept and offers control of afference as an alternative. It was written before Bill Powers' book and Science article were published. It refers to the Powers, Clark & McFarland monograph. It's a great paper Wayne. I was surprised to find that you fell into the abyss of PCT well before I did. Nice going.

Hasta Luego

Rick

Date: Mon Oct 21, 1991 1:31 pm PST
From: jbjg7967
Subject: humor

Here's one for the Official PCT Joke Book (isn't Greg compiling this?):

Why did the chicken cross the road?

It wanted to perceive itself on the other side.

Date: Mon Oct 21, 1991 4:17 pm PST
From: Gary A. Cziko
Subject: Robinson on Bees

[from Gary Cziko 911021]

Here's a reaction to Tolstoy's bees from entymologist Gene Robinson on my campus. Gene has done some marvelous work on the social behavior of bees in their hives. He does neat stuff like remove all the oldest bees and see how the younger bees age before their time to take up the jobs usually done by the oldest bees. Here is his response to some of the recent bee stuff I shared with him from the network.

Gene sees purpose made possible via evolution. But isn't it interesting that the Darwinian view of evolution is that there is no purpose to it, and yet it results in the evolution of purposeful systems.

Does anybody out there know anything about "behavioral ecology" and how it relates to PCT?-

-Gary

>
> Reply to: RE>Tolstoy on the Bee's Purpos
>Behavioral ecology is dedicated to the proposition that it is possible to infer
>purpose in behavior. The quote by Tolstoy shows how utterly impossible that
>would be, IF, one were not guided by certain assumptions. These assumptions
>are based on Darwinian theory, in which everything must be viewed through the
>prism of selfishness. Even pollination can thus be seen as a mutualistic
>arrangement that benefits both plant and bee; indeed, that is the only way it
>could evolve. Behavioral ecology is controversial in the sense that some say
>it is difficult to infer purpose in behavior, that is to distinguish between
>adaptive "just-so" stories, and to distinguish adaptation from historical
>processes. But generally speaking, Tolstoy would have had an easier time if he
>had been a Darwinian. He would also have had an easier time if he focused on
>the purpose of a specific behavior, rather than using a behavior to focus on
>the question of the "purpose of the bee".
> Gene

==== Gary A. Cziko Telephone: (217) 333-4382

Date: Mon Oct 21, 1991 5:31 pm PST
From: mmt
Subject: Re: control and influence

[Martin Taylor--911021 1910]
(Bruce Nevin (911008 0742))
>
>I think that to make this point we might say that causation is defined
>such that "A causes B" means "If A, then B, 100% of the time, no exceptions."

Is there really any point in redefining a word so that it can NEVER be used correctly? That's what you are trying to do here. All events are the results of combinations of circumstances, never to be repeated. For some sets of circumstances one can extract subsets that repeat, and for some of those one can determine successor subsets that usually follow, but never "100% of the time, no exceptions." Even dropping an iron ball off a bridge does not guarantee that the ball will reach the river below; a jet engine might be turned on underneath and melt the ball or blow it away. SHould we then not say that gravity causes the ball to fall? I think there is only a distinction of degree between influence and cause. If there is a high probability of a consequent over a range of external (so-called irrelevant) conditions, then say "cause" (especially if you have a theoretical reason why the consequent "should" occur). If the probability is lower (irrelevancy is less well determined), then say "influence."

In PCT terms, there are no causes, only influences. Errors do not cause behaviour that reduces error, they only increase the likelihood of that behaviour (the supporting control systems must be in place and properly organized, and working, and the environment must permit the appropriate behaviour to occur).

BTW, this idea that there could be a "cause" of something seems to me to be at the root of a lot of our social and legal problems. Who or what "caused" an accident? No-one; if any of hundreds of events had happened differently, there would have been no accident then (Aunt Betty might have dropped her handkerchief, delaying the start of the trip by 3 seconds, causing a traffic-light to turn red before you go to it, thus keeping you well out of the way of the car that crossed the median strip). Would Aunt Betty have caused the accident not to happen?

Isn't the search for cause embedded in our culture? And shouldn't we ignore it if we are seeking the way the world works?

OK. I'm back. Only 150 more messages to catch up on!

Martin Taylor

Date: Mon Oct 21, 1991 5:44 pm PST
From: marken
Subject: Evolutionary Purpose

[from Rick Marken (911021b)]

Gene Robinson gives the following reaction to Tolstoy's bees;

> The quote by Tolstoy shows how utterly impossible that
>would be, IF, one were not guided by certain assumptions. These assumptions
>are based on Darwinian theory, in which everything must be viewed through the
>prism of selfishness.

Gene Robinson is not alone in his belief that evolutionary theory provides a basis for inferring the purpose of behavior. I've been corresponding (a little) with philosopher Ruth Millikan (currently at the Center for Advanced Study in the Behavioral Sciences at Stanford) who proposes the same theory (I ran across it in a paper of hers called "What is behavior?" -- a question to which I have devoted not a little of my time). I think it's a pretty flimsy basis for inferring purpose. Control theory does a much better job. Control theory explains what purposes are (reference states of controlled variables) and how to determine the purpose of a behavior (the test for the controlled variable -- which also reveals whether a behavior even has a purpose). The criterion for purpose in control theory is very simple -- disturbance resistance. If a disturbance does not have its expected effect on a variable then the variable is probably being kept at some value "on purpose" -- it is a controlled variable. There is more to this test, but that's the essense. I don't see the same level of quantitative rigor in the evolutionary approach. Indeed, I cannot see how such an approach can distinguish between the purposeful and non-purposeful aspects of any behavior. How does an evolutionary perspective help you determine the purpose of a diver, for instance. Is her purpose to hold a certain body configuration, to turn a somersault in the air, to accelerate downward at 32 feet per sec per sec. The evolutionist may be able to come up with ad hoc stories to justify their choice -- the control theorist can suggest methods and criteria for testing hypotheses about behavioral purposes. Indeed, without doing these tests it is not only impossible to know an organism's purpose, it is imposible to know (other than by guessing) what an organism is DOING.
Best Regards

Rick

Date: Tue Oct 22, 1991 9:08 am PST
From: POWERS DENISON C
Subject: Aiming, bees

[From Bill Powers (911022.0745)]

Bill Cuningham (911020), continued --

>You aim the gun as best you can and fire. There is no control of the
>projectile on the way.

Same applies to bowling, golf, horseshoes, buying stocks, and raising kids. How does control theory handle such cases? Not by asserting that there's control when there isn't. There are two answers.

One is that you control what you can control and hope that the result is what you want. This applies in cases where you get only one or very few chances to hit the target. Cognition comes in here in a way I haven't dealt with very well. You perceive the situation and compute where to aim. The aiming you can control, but the computation is basically an open-loop proposition.
Of course there's a goal involved; you want to perceive the projectile hitting the target, the kids growing up happy. But all the error-correcting has to go on in imagination. You try to imagine the effects of various actions, and you judge whether they might lead to a result close to what is wanted. Mental models are involved. Communications from other people, information from books, memories of past experience and other people's behavior must figure into the mental model. It's hard to compress all these things into a diagram with three little boxes connected by arrows. It's a good thing that most of our behavior doesn't involve this one-shot kind of control, because it doesn't amount to much control.

When you can be sure that the environment isn't changing and that no unexpected disturbances are going to arise, or when you can be sure that human beings aren't going to get into the act and do something unpredictable, you can study the laws of nature and come up with pretty good predictions. But in cases like that you have more than one chance -- you have to, to find out what those laws are in the first place.

The second answer is that *when you have a reasonable number of tries to hit the target* a higher-order system can adjust the aiming point a little at a time on the basis of the previous result. The principle here is to slow the correction. If you want a control system with a loop gain of G, then you apply a correction based on 1/(1+G) of the error. This is called "sampled control." If you just compute how big the error is and adjust the output to erase it on t he next move, you will succeed only if there are no disturbances; the loop gain will turn out to be very low. If it's very important to maintain control, you have to correct errors in small steps; there's no way around it. You learn this when you learn to drive; beginners always overcorrect because they're scared and think they have to correct every error right away. You learn the same thing in flying airplanes. If a bump tilts the plane, chances are that the next bump will straighten it out, so relax.

S-R theory is OK as far as it goes: outputs ARE a function of inputs. But there's an offset in that function called the reference level, and the input depends on the output, too. In some cases where there are visible time-lags between an output and its perceptual effect, you can get away with following events around the loop in sequence, because events really do happen in sequence some times, especially if you've set up an experiment in which they have to. But you have to get into the equations of control in order to understand what happens when you let this sequence run and run, applying the same rules over and over. That's what the S-R theorists often fail to do: they'll trace events around the loop once, and forget that they've proposed a relationship that still exists the next time around and the time after that and so on. Most proposed rules will crash if you let them run indefinitely (the more you eat, the more you want).

>My personal agenda is try to bend their [data fusion] model so that it
>resembles your hierarchical model--which I think it should regardless of PCT.

While you're at it, how about working out the way that higher systems can select lower-order perceptions by selecting which lower-order systems are going to recieve reference signals? One way to handle this is to say we learn one selection and are stuck with it. But I think the hierarchy is smarter than that. We can try on different modes of perception and control to see which works best. How does this work? What does a selector system have to perceive that tells it there's a wrong or inferior selection? How does it determine the distribution of outputs? Is this just a natural outcome of having multiple systems at various levels, or is there some special kind of control process that's required? What do I mean by "a selector system?" Is this a possible new level of control? It is a feature of every level?

It would be great if the existing hierarchical model could somehow handle this problem -- we don't want to fix it if it's not broke. But I don't see how to do it right now. Of course the best way to approach this is to wait for an example to come up and try to cope with it until you're sure that the present arrangement can't do it. Then maybe you'll see WHY it can't, and see what to do about it.

>It strikes me that a machine/organizational model that conforms
>reasonably with PCT would certainly fit nicely with a PCT model of the
>human. Users of the composite model would have one thing to learn.
>Interested? It would be a slow process, but not impossible.

Sure we're interested.

It might also be that you will find things that others would rather not find. You say that the sensors "are heterogeneous, asynchronous, and generate their own errors" and add "Making sense out of all this is far from trivial." Have you considered that it may be fundamentally impossible? My experience with military executives is that they pay little attention to what is possible; all they know is what they need, and they assume that somehow it will be provided (or heads will roll). If you were to discover that the signal is too noisy to use by ANY means (and always has been), would that be accepted? The implication would be that intelligence never has been any good. Is such a conclusion likely to earn you a raise?

In the intelligence field, what is considered data is often mainly noise. When that's true, you can pick "meaningful" signals of all kinds out of the same input, all generated by your own imagination and beliefs. All your perceptual functions get a little tickle from the noise, and one of them is bound to be tickled the most. All you have to do then is supply the rest of the perception yourself, suppress the rest, and voila! intelligence! But in fact the signal may have been just pure noise.

Lacking any ability to insert disturbances, there's no way to test to see which perceptions originate outside you and which inside. From the little I have seen of "intelligence" reports, the result is to reveal far more about the analyst's hopes, fears, and assumptions than about the situation. Gates was accused of putting a political slant on CIA reports. Of course he did. So will anyone in that position, and not because of an evil heart. The data are so poor that most conclusions will reflect properties of the people drawing the conclusions more than any objective information about the world. I suspect that behind a lot of work of this sort is a hope that somehow bad data can be made into good data by sufficiently powerful information processing and clever hypothesizing. You may be up against a problem that can't really be solved -- in a situation where you are expected to solve it.

Cunningham (911020b) --

>If the travel time of the droplets is long, we fall back to the
>artillery situation where the nozzle angle is controlled to some
>reference we *predict* will result in a wet target.

This is an intermediate case, where the time-lag is relatively short and we have many chances per second to make corrections. In this case we learn to pick a reference-

relationship of "aiming-point ahead of target" -- i.e., we lead the target. Old-fashioned manual gunsights had aiming rings; you were supposed to judge the speed and distance of the target and choose the correct ring to use as a sighting mark, rather than the dot in the center of the sight. This reduced the control problem to configuration-control. Of course it didn't work very well. The ratio of shots fired to hits registered is very small, or was until control systems were put into the projectiles.
Gary Cziko (911020) --

Another thought on purposes. You're right, that the Tolstoyian conception of purpose stands squarely in the way. To speak of "the ultimate purpose of the bee" implies trying to guess God's Master Plan. I've corresponded a bit with Howard Rosenbrock; his idea of purpose has to do with Hamiltonians and variational calculus. Francisco Varela defines purpose as "the use to which something is put." Humberto Maturana likens it to a boat rocking along being randomly buffeted by wind and wave.

What we are up against is a HUGE blind spot. The idea that organisms are agents, that they can construct and execute purposes of their own, was tossed out of science a century ago or more. Now nobody even realizes that this is what we are talking about. We are proposing something that was previously proposed only by animists and mystics. Scientists are satisfied, at least among themselves, that they have taken care of animism and mysticism once and for all. That includes the phenomena that the animists and mystics purported to be talking about.

So your esteemed senior colleague is actually handing you a compliment by misunderstanding what you mean. He is saying, "You couldn't mean that organisms are REALLY purposive, because I know you're not a mystical nut or an anachronism." In speaking to such a person you have a double task: to get across the idea that purposes really exist in organisms, and that this is not just taking the side of discredited superstitions from a century ago. It's hard enough to get people to reassess *one* idea; to ask them to change their minds on two subjects at once may be asking the impossible.

Date: Tue Oct 22, 1991 1:18 pm PST
From: mmt
Subject: Re: Where's the opposition

[Martin Taylor 911022 12:30]
(Rick Marken 911017b)

>
>What PCT needs is many more attempts, like that of Fowler and Turvey, to
>prove that it is wrong. The potential PCT revolution has suffered far
>more from being ignored than from being "disproven".

I'm not clear what could be proved wrong. Surely it cannot be that living organisms behave so as to produce acceptable perceptions of their relationship with the world? Could that fail to be true in any organism that had survived an evolutionary process in which failure would be fatal?
If not that, then what should be disproved? That a hierarchic contro system could control? But we know that it can, and any simple demo, such as Rick's spreadsheet can show it to a sceptic.

Is it the details of the structure that need to be disproved? Perhaps. Many of my postings have been aimed at testing the capability of the presented structures to accommodate certain problems, and I have a lot more of these questions to raise when I get caught up with the backlog. But would changing the details mean proving the theory wrong?

Not in essence, and both Bill and Rick have emphasised that the details can be changed, even in such significant aspects as allowing inversion of the levels of the hierarchy.

In short, I see PCT as rather like many other tools: clearly applicable to many interesting phenomena, obviously "true" in a trivial sense, and tunable to fit most situations. Where disagreements and difficulties arise, they usually revolve around different interpretations of the details. But they do not affect the essentials, any more than the limitations imposed by mechanics or by information theory can be altered by looking at the actual behaviour of organisms. Things are as they must be.

Do you expect attempts to prove information theory wrong?
I expect that it would be easy to show where particular applications of information theory are wrong; likewise with PCT applications. But not the principles of either.

Martin Taylor

Date: Tue Oct 22, 1991 1:21 pm PST
From: mmt
Subject: Re: fast loop

[Martin Taylor 911022 1220]
(Bruce Nevin 911017 1202)

>Martin Taylor (Wed, 16 Oct 1991 15:34:58 EDT)--
>
>Do you have a reference for that work on evolution of closer proximity
>of articulatory organs and cerebellum (if indeed it was cerebellum)?
>A name? Will the work be published someplace? I'm hoping to get some
>time to make contact with Lieberman, and this is the sort of thing he
>would be most interested in.

I don't have any references, but the presenter was Sylvia Candelaria de Ram (sylvia@nmsu.edu). You might ask her. Refer to her talk at the Second Venaco Workshop on the Structure of Multimodal Dialogue. Come to think of it, I think Sylvia might like to adhere to this list. She used a lot of implications from control theory (not PCT specifically) in her presentation, and was overjoyed to find in the workshop several people who could understand what she was talking about, because no-one at New Mexico State seemed to talk her language.

It's not Sylvia's work. She was just presenting the facts as she had found them in the literature, as support for her position relating our ability to do very low-delay shadowing of speech to the phenomenon of linguistic drift.

Martin Taylor

Date: Tue Oct 22, 1991 2:10 pm PST
From: mmt
ubject: An independent witness?

On my return from Europe, one thing awaiting me was a notice of a seminar that, unfortunately, had already happened. At the end of this note, I will post the abstract.

Does anyone know of Paul H Wewerinke? Is he on this list? If not, and someone at Twente is, perhaps they could guide him to it. Otherwise, perhaps it would be worthwhile Gary communicating with the postmaster at Twente to find his e-mail address.

Here's the notice:

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Analytical Models of Man-Machine Systems.

Paul H. Wewerinke
System and Control Theory Group
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The Netherlands

Abstract

Models of human perception, information processing, decision making and control behaviour in the context of dynamic man-machine systems will be reviewed. In addition to the topic of model structure (in terms of optimal estimation-, control- and decision theory), fundamental principles and concepts of such models will be emphasised. For example, goal-oriented operations are assumed, with given constraints (due to process dynamics, control limits, environment, etc.) in a given (disturbance) environment.

It will be shown that such models can be applied to a variety of Man-Machine Systems, related for example to:

- aircraft control
- space operations
- car driving
- ship handling
- robotic systems (e.g. teleoperations)
- economic and social processes, etc.

A number of these applications will be discussed in detail (depending on time and interest). The analytical capability of the models will be demonstrated and their validity discussed.

Martin Taylor

Date: Tue Oct 22, 1991 2:47 pm PST
From: marken
Subject: Re: Where's the opposition

[From Rick Marken (911022)]

Welcome back Martin Taylor!

You write (Martin Taylor 911022), regarding my plea for more attempts to disprove PCT:

>I'm not clear what could be proved wrong.

PCT says that behavior is organized around the control of perceptual input variables. ALL current models of behavior say that this is not the case. There have been many efforts, for example, to show that feedback is of little or no importance in various kinds of behavior. If this were true then it would be a BIG problem for PCT. If people can behave

(produce controlled results) without feedback then PCT is disproven. Unfortunately, the existing studies that purport to show the lack of a need for feedback are very qualitative -- they don't measure any controlled variables and they don't provide working models of non-PCT organizations that can produce "feedback-less" control. It's mainly duck research -- with the results already presumed. But the research does suggests that these people know that the existence of feedback effects would cause big problems for their world view -- they just have no idea how big those problems actually are.
There have been many efforts to show that control can be achieved "passively" by "open loop" systems such as coordinative structures, motor programs or dynamic attractors. If this were true it would cause big problems for PCT.

Behavior theories are based on the premise that environmental variables (such as reinforcements, conditioned stimuli, etc) control behavior. Control theory says that these effects are side effects of the operation of a closed loop control system. If, for example, it can be shown that reinforcements operate as claimed (to strengthen the behavior that produces them) then it would be a big problem for PCT (which views reinforcements as controlled variables).

Indeed, if almost any finding of conventional psychology were taken at face value it would be strong evidence against control theory. If (to take a classic finding) bystanders really cause other bystanders to stay put rather than report a witnessd crime then PCT is wrong. Such a result suggests that people are not controllers, but, rather, controlled by external circumstances.

My plea is to have those who do this kind of research (virtually all social scientists) explicitly present it as a disproof of PCT. Then we (PCTers) can show how they might be wrong. The problem is that all this research is done UNDER THE ASSUMPTION that PCT is WRONG. The implications of the research for PCT are simply ignored. And it is best that they are; because, if the implications are followed through explicitly (as Fowler and Turvey made the mistake of doing) then there is the definite possibility that those who do this will have to confront the meaning of PCT or make fools of themselves (as F&T did).

>In short, I see PCT as rather like many other tools: clearly applicable
>to many interesting phenomena, obviously "true" in a trivial sense, and
>tunable to fit most situations. Where disagreements and difficulties
>arise, they usually revolve around different interpretations of the
>details. But they do not affect the essentials, any more than the
>limitations imposed by mechanics or by information theory can be altered
>by looking at the actual behaviour of organisms. Things are as they must be.

Fowler & Turvey did considerably more than claim that a hierarchical control model could not work in their particular situation (although they were wrong). They made more than a formal error in computation or derivation. Thay claimed that a PCT type model was inappropriate (that's why they made the computation-al error, probably; because they knew the result they wanted). They argued for a different functional class of model for their coordination task --one which computes optima and does hill climbing. What they were claiming (and what those in their "school" of thought continue to claim) is that behavior is organized around the control of OUTPUT -- NOT INPUT.

>Do you expect attempts to prove information theory wrong?

To the extent that it is a functional model of a phenomenon -- yes. To the extent that it is a formal system (like calculus) that can be used to describe a phenomenon -- of course not. PCT is not like information theory; PCT is a working model of living nervous systems. Info theory is a formal description of a process (information transmission) that could be carried out in many different ways. But if someone really developed a working information theory model of perception (where, for example, inputs were transformed into probabilities and manipulated according to the theorms of infomation theory) then it should be rejectable.

If a theory cannot be rejected (disproven) then I don't consider it a scientific theory. PCT can be disproven; for example, PCT can be rejected as a model of the behavior of a mass on a spring. It is also be rejectable as a model of the behavior of a human arm.

Best Regards

Rick

Date: Tue Oct 22, 1991 2:52 pm PST
From: marken
Subject: Vy a duck?

[From Rick Marken (911022b)]

In my previous post I said:

>It's mainly duck research

I meant "duck and run" research

Sorry Rick

Date: Tue Oct 22, 1991 4:34 pm PST
From: Martin Taylor
Subject: Re: Where's the opposition

[Martin Taylor 911022 16:10]
(Rick Marken 911022)
>
>>I'm not clear what could be proved wrong.
>
>PCT says that behavior is organized around the control of perceptual
>input variables. ALL current models of behavior say that this is not
>the case. There have been many efforts, for example, to show that
>feedback is of little or no importance in various kinds of behavior. If
>this were true then it would be a BIG problem for PCT. If people can
>behave (produce controlled results) without feedback then PCT is
>disproven. Unfortunately, the existing studies that purport to
>show the lack of a need for feedback are very qualitative -- they
>don't measure any controlled variables and they don't provide working
>models of non-PCT organizations that can produce "feedback-less" control.
>It's mainly duck research -- with the results already presumed.
>But the research does suggests that these people know that the existence
>of feedback effects would cause big problems for their world
>view -- they just have no idea how big those problems actually are.
>
>There have been many efforts to show that control can be achieved
>"passively" by "open loop" systems such as coordinative structures,
>motor programs or dynamic attractors. If this were true it would cause big problems for PCT.

If you claim that all aspects of behaviour are part of the perceptual control function, and that is the essence of PCT, then that is a bigger claim than I have understood in my few months of reading this list. You are saying that there can be NO open-loop actions at any level of analysis. I can't speak for Bill Powers, but for myself I would not find the discovery of some accurate open-loop actions to disturb my view of PCT, especially at the environmental interface where things happen very fast. If I read you correctly, the discovery of even one instance of open-loop action would destroy your whole PCT universe.

As for what ALL current models of behaviour say, I think you go quite a bit too far. My training is in psychology as well as in engineering, and I cannot remember ever being exposed to the idea that behaviour does not involve feedback--well, perhaps feedback might have been ignored in a lot of cases, but I can't remember its existence or usefulness ever being explicitly denied.

But you specifically identify "behaviour" with "produce controlled results," and if you make this identification you have a private definition of behaviour. Of course one cannot produce controlled results in an open loop situation, but one can produce predictable results, if the environment is stable enough. I can do many things in my house, like putting my hand directly on a doorknob, in the dark with no feedback that is obvious to me except for the final success or failure of the action. I don't think that is "controlled" behaviour, although control loops certainly play a big part in my actual movements. We DO do things without depending on feedback, sometimes. Does this deny the validity of PCT?

I don't understand also why you worry about the fact -- yes, fact -- that systems with dynamic attractors return to those attractors after a disturbance. That's a mathematical truth, and why should it worry you that some stability can exist without control, or more to the point, without an externally provided reference signal. What such systems cannot do is to shift their attractor in response to some other external condition. In fact, one way of looking at a control system is as a dynamic attractor system in which the form (or location) of the attractor can be altered in a predictable way by some external influence (not cause).
>
> Such a result suggests that people are not controllers, but, rather,
>controlled by external circumstances.
>
But all elementary controllers (except the top level) are controlled in the sense that they follow their reference signals. Where's the beef?

I sense a kind of scientific ghetto-mindedness in this, and I don't like it. I can't believe that the power of PCT stands or falls on whether there exists a situation in which people act accurately without feedback. I do believe its fundamental assumptions have to be right, and at the same time, like ALL theories, its details have to be wrong. We need to develop the structural details.

I'll close this with a quote from J.G.Taylor, with which I am sure you will agree:
"Any theory of perception we may construct must be such that it can account exactly for all types of results. That is to say, it must be so general that when two sets of values, derived from two subjects, are assigned to the parameters and variables of the theoretical system, it must generate different results that will be in accord with the reports of the subjects."
(I'd generalize "reports" to "behaviour" in the last sentence, and I don't think JG would disagree. The paragraph follows a discussion of the validity of using subjective reports as data, and what to do if two people provide widely different reports in the "same" situation.)
(By the way, in the next paragraph, J.G. explicitly "proposes to doubt" the idea that behaviour is a response to perception, and the rest of the book is devoted to showing that what is perceived is a consequence of the behaviour that that perception provides the possibility of correcting. I think this provides a counter-example to your "ALL" models of behaviour, unless you claim JG is not current, since he published in 1962.)

Martin Taylor

Date: Tue Oct 22, 1991 5:25 pm PST
From: marken
Subject: Re: Where's the opposition

[Rick Marken (911022c)]

Martin Taylor (911022 16:10) finally sees just how subversive PCT is. Hold on to your paradigms -- here we go. Martin says:

>If you claim that all aspects of behaviour are part of the perceptual control function,

Only the intentional ones.

> and that is the essence of PCT, then that is a bigger
>claim than I have understood in my few months of reading this list.
Now you see why its hard for PCT types to make friends. It is a big claim, but no bigger than the implicit claim of conventional psychology (implicit in the independent-dependent variable framework of their research methodology) that all aspects of behavior are part of a lineal cause effect chain. We only make the perceptual control claim about purposeful behavior.

> You are saying that there can be NO open-loop actions at any level of analysis.

No. The velocity of my body after I leave the ground when I jump up is an open -loop behavior. And it's handled well by an open-loop model (newton's laws). Unintended side effects of control actions are also open-loop.

>I can't speak for Bill Powers, but for myself I would not find the
>discovery of some accurate open-loop actions to disturb my view of PCT,
>especially at the environmental interface where things happen very fast.
>If I read you correctly, the discovery of even one instance of open-loop
>action would destroy your whole PCT universe.

No, one instance of open-loop control would be a big problem for me. If you do find one instance of open-loop CONTROL you would then have to develop a model that can control in that situation as well as when the loop is closed.

>As for what ALL current models of behaviour say, I think you go quite a
>bit too far. My training is in psychology as well as in engineering, and
>I cannot remember ever being exposed to the idea that behaviour does not
>involve feedback--well, perhaps feedback might have been ignored in a lot
>of cases, but I can't remember its existence or usefulness ever being explicitly denied.

Its importance has often been denied. It has only rarely been properly incorporated into behavioral models (it is usually treated as part of a causal sequence) and its implications for the nature of behavior have not been understood. If they had, the research methods texts for psychology would not look ANYTHING like they do now. I know that psychologists talk about feedback alot. But the proof is in the pudding. Just look at the research methods texts -- every one of them ASSUMES that organisms are organized as open loop systems. Find me ONE methods text in psychology that does not say that the proper way to study behavior is to manipulate an independent variable under controlled conitions and measure it's effect on a dependent variable. This ASSUMES that organisms are organized as CAUSE-EFFECT devices -- feedback or not.

>But you specifically identify "behaviour" with "produce
>controlled results," and if you make this identification you have a private
>definition of behaviour.

Nope -- very public. I've published at least two papers on this subject. This definition of behavior eliminates unintended "behaviors" (like downward acceleration due to gravity) and keeps the stuff that most people think of as "behavior" -- the intentional (not necessarily conscious) results. If everything that organisms do is "behavior" then, yes, some of it can be handled very well by physical models (like newton's laws and attractor models, etc). The problem is, how do you know when to apply the physical models and when to apply the psychological models. Conventional psychology says "whenever you want" basically. In fact, they just try to let physical models get away with being psychological models. PCT says no -- physical models apply when there is no control; psychological models apply when there is control.

>Of course one cannot produce controlled results
>in an open loop situation, but one can produce predictable results, if
>the environment is stable enough. I can do many things in my house, like
>putting my hand directly on a doorknob, in the dark with no feedback that
>is obvious to me except for the final success or failure of the action.
>I don't think that is "controlled" behaviour, although control loops
>certainly play a big part in my actual movements. We DO do things without
>depending on feedback, sometimes. Does this deny the validity of PCT?

How does the nervous system know how and when to change its organization from open to closed loop? Is your nervous system organized as an open loop system when its in a stable environment and as a closed loop system when its in a disturbance prone environment? How does it know which kind of environment it's in? Feedback?

>I don't understand also why you worry about the fact -- yes, fact -- that
>systems with dynamic attractors return to those attractors after a
>disturbance. That's a mathematical truth, and why should it worry you

I don't worry about it at all. The model just doesn't control. Just because the model mimics part of a phenomenon doesn't mean it's close to being right. Attractors do mimic certain aspects of control. So? All the more reason to be wary. Purposeful behavior mimics some aspects of cause-effect behavior. So aristotle made the mistake of modeling free fall as a purposeful process. The mistake works both ways; science has been preoccupied with avoiding purposeful models of cause-effect processes. PCT people argue that they should be just careful to avoid using cause-effect models of purposeful behavior.

>that some stability can exist without control, or more to the point,
>without an externally provided reference signal. What such systems
>cannot do is to shift their attractor in response to some other external
>condition. In fact, one way of looking at a control system is as a
>dynamic attractor system in which the form (or location) of the attractor
>can be altered in a predictable way by some external influence (not cause).

Wrong. Dynamic attractors also fail to behave properly when disturbances are continuously varying. They don't control.

>I sense a kind of scientific ghetto-mindedness in this, and I don't like
>it. I can't believe that the power of PCT stands or falls on whether
>there exists a situation in which people act accurately without feedback.

I don't know what ghetto-minded is? PCT has been ignored. So? It has been ignored largely because it explains a phenomenon that people are not interested in -- control. I suggested that we might get more attention if conventional psychologists tried to explicitly disprove the theory. If they did they would run squarely into the problems PCT people have been discussing for years -- like "What is behavior", "How do you determine if a behavior is purposeful?", etc.

It's a nice ghetto --great bagels. Everyone is welcome and we like to go outside for a visit occasionally.

>I do believe its fundamental assumptions have to be right, and at the
>same time, like ALL theories, its details have to be wrong. We need to
>develop the structural details.

I kind of agree. But there are psychologists who are claiming that PCT is not the correct model of a great deal of purposeful behavior. So they are saying that it's fundemental assumptions are wrong. I agree heartily about the details.

> (By the way, in the next paragraph, J.G. explicitly "proposes to doubt"
>the idea that behaviour is a response to perception, and the rest of the
>book is devoted to showing that what is perceived is a consequence of the
>behaviour that that perception provides the possibility of correcting.
>I think this provides a counter-example to your "ALL" models of behaviour,
>unless you claim JG is not current, since he published in 1962.)

Sounds great to me; definitely takes the wind out of my "all models" claim. But how did this influence the goals and methods of his research?

When I see fewer ANOVAs and more "tests for the controlled variable" in the Journal of Experimental Psychology, then I'll move out of the ghetto.

I guess you feel that PCT is one of many valid views of various aspects of behavior. Maybe my ghetto mentality is to feel that PCT is a whole new approach to understanding life -- one which is incompatible with what you consider to be other legitimate perspectives. Great. Just show me --where's the beef? in those other perspectives.

Regards

Rick M.

Date: Wed Oct 23, 1991 1:20 am PST
From: Bill CUNNINGHAM - ATCD-GI
TO: Hortideas Publishing / MCI ID: 497-2767

*** Resending note of   / /   :
From Bill Cunningham 911022.1110

Bill Powers 911021.0730

I like your fixes! Ref levels vice ref signals is nonthreatening. When somebody asks where they come from, they're ready for next step. Also like error reducing/magnifying. I keep forgetting that "positive feedback" is abused. Too bad somebody didn't coin a mushy phrase like "affirmative response" instead. Looking at that offering, I'm not sure which is worse!
I think you're right about deferring reorganization & hierarchy. Might go well with an "unfinished business/unanswered question" chart as questions arise--then do it when enough of the right questions await answer.

Your answer on organization is very illustrative and helpful. Two distinct parts: the random reorganization and the decision to reorganize/leave alone. The former is means of execution by lower level. The latter is upper level function, in this case me at the keyboard. Neat demo.

Date: Wed Oct 23, 1991 5:00 am PST

From: CHARLES W. TUCKER
Subject: EYE MOVEMENT AND GIBSON

CSG-EM15 [FROM CHUCK TUCKER 911023.0800]

RE: a quote from Lieberman about eye movement

I must admit ignorance of Robinson's model of the oculomotor system or any of the research on eye movement. What I do recall are my own experience of having my eye doctor give me a faulty prescription and having to try to adjust to the disturbances experienced from wearing those glasses and believing I was going crazy from those experiences even though I don't believe I can "go crazy" but rather I believe I choose to make myself crazy. But I have been reading in Philip Lieberman's recent book UNIQUELY HUMAN (Harvard Press, 1991) which is a summary of his earlier book THE BIOLOGY AND EVOLUTION OF LANGUAGE (Harvard Press, 1984) [which my friend Bob Stewart says is the most important book published since Power's BCP] and I came across a comment about eye movement and an example which falls into the category of Bill's prized "portable experiments" that I will report. Lieberman says in the section of the book on "Brain Circuits and Behavior":

"A simple experiment demonstrating vision-stabalizing circuits. Even "simple" tasks, such as looking at this book while you continually move your eyes, require the integrated activity of different parts of your brain. A circuit involving the parts of the brain that control eye, head, and body movement, the frontal regions of the brain, and the posterior parts of the brain directly involved in vision corrects for the constant motion of your eyes. Consider the bane of home movies and videos - the shifting, lurching, dizzy world that results from camera movement. Novice camera operators do not realize that they have to stabilize the camera, because the world does not suddenly move when you move your head or eye. What is not intuitively obvious is that the stability of the perceived visual world derives from an elaborate process in which the primary visual areas of the brain receive information from "higher" areas of the brain. The reason that the world doesn't appear to move is that the human visual-sensory system has been prepared to compensate for change in position by knowledge of the motoric instructions to the eyeball, neck, and the rest of your body. The motor control systems that control the direction in which you are looking and eyeball movement speak to the brain mechanisms that interpret visual images. The frontal areas of the brain that integrate motoric and sensory information form part of a circuit that is necessary to interpret the basic visual input (Teuber, 1964).

You can easily, literally "see" that the brain network that [p. 29] stabilizes vision really exists. Simply position your right index finger against the right side of your right eye and gently push it inward. The reason the image jumps is probably that there was not selective advantage for any activity that involved people's pushing against their eyes. Therefore, natural selection for the integration of eye movement from finger pushing never occurred [p. 30]."(NOTE 1: Lieberman makes no reference to any Robinson in his book)(NOTE 2: Could this finger push be characterized as a disturbance?)(NOTE 3: You may suspect that I like this statement by Lieberman because he uses the phrase "motoric instructions"; you are correct!)

RE: Gibson's ideas and Karl Pribram's recent book

Again, I am ignorant of Gibson's ideas since I have never read his work but have only read about his work from the posts about him on this NET. I recently got a copy of Pribram's recent book BRAIN AND PERCEPTION (Lawerance Erlbaum, 1991) and in the introduction found several quotes about Gibson's work that may interest some of you - they do interest me since if I agree with Pribram I won't have to bother with Gibson's work. Pribram writes:

"The results of these researches (his own) have cast doubt on viewing brain perceptual processing as elementaristic, bottom-up, reflex-arc, stimulus-response - views that still characterize many texts in neuophysiology, psychology and perception [p. xvi]."

"More fundamental disagreement has plagued this issue (neural processing in perception) than almost any other topic affecting the mind-brain relationship. At one extreme, is the common sense feeling that the contents of perception can be trusted to reliably inform the perceiver about the world in which he navigates - in philosophy this position is called naive or, when bolstered by evidence, direct realism (Gibson, 1979; Shaw, Turvey, & Mace, 1982).

At the other extreme is the feeling that we can never "really" be sure of anything, including the validity of our perceptions - in philosophy this positon is called solipsism, or when specified by evidence, autopoiesis. Autopieses is the view that our perceptual apparatus operates autonomously as a closed system (Maturana, 1969; Varela, 1979).

In between are compromise views and these also range from various materialisms (e.g., Bunge, 1980) to phenomenalist, mentalist (e.g., Sperry, 1980; Searle, 1984) and constructional (e.g., Maxwell, 1976; Pribram, 1971) positions. A recent brief review of these issues is given by Epstein (1987){Pp. 103-115 in Bridgeman, Owens, Shebilske and Wolff (Eds.) Sensorymotor interactions in space perceptions and action. North Holland: Elvesier.}.[p. xxii]"

. . . .

"A good place to begin the study of these interrelations in Gibson's suggestion that we consider brain processes to resonate to the patterns that stimulate the senses, a suggestion in keeping with the harmonic analyses indertaken in the holonomic brain theory presented in these lectures. As will be detailed, taking Gibson's suggestion serioulsly commits one to realism. But the commitment entails accepting the full implication of the ecological, "layered," approach to perception by including the layers of brain processes largely ignored by Gibson.[p. xxii]"

. . . .

"Taking the stance implied by realism ("a program of theory and research committed to realism") is akin to an act of faith : The initial sensory experiences of infants are disparate; even as adults, introspection yields perceptions differing [p. xxiv] in kind according to the sense involved. When we identify what we hear, see, and touch as referring to the "same" event, we resort to consensual validation. In humans this procedure is repeated when we identify "a redwinged black bird," as the "same" object with the "same" attributes referred to by someone else. One makes a pragmatic existential choice early on, either to distrust the process of consensual validation and retreat to solipsism, or trust and embrace a realist philosophy, and act accordingly.

Ecological psychology and the holonomic brain theory are both eminently compatible with a realist position. However, to state simply that perception is "direct" skips over several steps in the perceptual process that cannot be ignored.

One must confront the fact that the senses are stimulated by patterns of energy perceived as "light," "sound," and "touch" which do not have the same configural properties as do the objects with which they interface. This, however, does not mean that these patterns are composed of elements. Rather, a different process is at work: The configural properties that define objects become distributed and enfolded in the process of interfacing. They are thus transformed into an order which, as a hologram, is recognizably different from the preceived configuations of objects by which, in some non-trivial sense, "contains" those configurations.[p. xxv]"

. . . .

"The holonomic brain theory espouses a transformational and constructional realism and thus goes beyond the direct realism proposed by Gibson in specifying the ecological details of the sensory and brain processes involved in perceiving. Specification devolves on recognizing transformations that occur between bottom-up levels among brain systems. Top-down influences on processing procedures provide structural constraints on processing. That is what these lectures are about. [p. xxiv]"

I hope the above quotes will make a contribution to the conversation. I will try to catch up and write some other comments so I can see how that transpires.

Date: Wed Oct 23, 1991 6:08 am PST
From: POWERS DENISON C
Subject: Oculomotor control; Objections to CT

[From Bill Powers (911023.0700)]

Sorry about screwing up the format. I have to remember to load a different printer-controller into my word processor before printing to the file that gets sent. I also have to remember to type to a file instead of just sending the original. I've been preoccupied with a sudden failure of my downloading-compressing and got tired.

Wayne Hershberger --

I got the Robinson article -- much thanks. The stretch receptors are there in profusion -- also stretch-dependent signals at many places in the brain. All that's missing is the stretch reflex itself -- i.e., the muscular resistance to disturbance. You may remember that my model predicts that there will be only a weak resistance, which could easily be misinterpreted as the passive spring constant of the eye muscles or the sclera or both. Robinson does NOT use a control-system model of the kinesthetic part of the eye system. In fact, large parts of his model are open-loop (although very clever, and in some cases apparently irreproachable). I've recreated the first part of our oculomotor paper and will send it to you direct for comments before airing it on the net as a not-for-citation draft. Greg Williams is sending me more of Robinson's stuff -- a chapter from a book. After seeing that, I'll work on the visual-control part with pursuit tracking, vestibulo-optical reflex, and so on. I was pleased to see that my recreation of the model came out with equations identical to those in the first paper, give or take a sign or two.

Martin Taylor, Rick Marken (911022) --

Pretty hot stuff. I don't know whether to hit somone upside the head, cry Oi Veh, or vote Republican. Naturally I want to put my 2 cents in, but will try to keep it short.

Martin, you said

>In short, I see PCT as rather like many other tools: clearly applicable
>to many interesting phenomena, obviously "true" in a trivial sense, and
>tunable to fit most situations.

Once you understand it, of course it's "true in a trivial sense." But Rick is right in saying that vast gobs of psychologists and social scientists don't understand it and don't think it's true in a trivial or any other sense.

Even when you're groping in the dark, your hand has to hit that doorknob sooner or later or control is gone. Continuing an action when higher-level feedback is TEMPORARILY lost is certainly possible -- you switch to controlling what you still can sense, which in the dark is the kinesthetic version of where you are and what you are doing. Visual control ceases, though. If someone opens the door before you get to it, you'll go on groping ahead until you fall down the stairs.
As to strange attractors, pendulums, and the like, there are two distinctions between the behavior of such systems and the behavior of real control systems.

First, the energy that such systems use to counteract disturbances comes from the displacement that the disturbance put into the system. If you add a constant velocity to a strange attractor, the phase-plane diagram will drift upward. When you remove the velocity, the upward displacement creates the gradual return to the original fuzzy trajectory. In a pendulum, it's even easier: you disturb a damped pendulum by altering its potential energy (you give it a push). It then swings back, dissipating the energy you put in. So the pendulum is a purely passive system.
Second (really another version of "first"), strange attractors and pendulums have a very low loop gain. If either one had a high loop gain, you would find it very difficult to insert a disturbance in the first place. You would push on the pendulum and it would refuse to move. Even if it were hanging straight down, it would push back just as hard as you were pushing -- this, of course, would cause you to look at the bearing assembly from which it's hanging, because there would have to be some kind of motor there, if it isn't welded into a solid mass. If the strange attractor were a control system, then when you tried to make the trajectory deviate from its chaotic pattern, it would resist you and continue in the same chaotic pattern.

I have worked with control systems having a loop gain of one billion. Disturbances of such a system have no measurable effect.

One of the features of PCT is that it is based on control systems with high loop gain (not a billion, but at least in the 30s and up). This means that most behavior is not "error-correcting" except during fast transient disturbances. Most behavior does not represent the trajectory by which a control system gradually brings an error to zero. Instead, the system maintains inputs very close to their intended states. Variations in the controlled variables reflect variations in reference levels, not slow control.

So even "groping in the dark for the doorknob" is a process in which all the perceptual variables currently being sensed are under tight control; you maintain balance, you make groping movements, and at all times remain resistant to disturbance as the reference-positions of the parts of your body are changed.

We are so used to this tight control over the details of our behavior that we don't even use the word "control" for it. We just call it "doing." If I want to pick up a glass, I just "reach" for it, not realizing that the reaching reflects a changing pattern of position and velocity reference signals with the muscles actively making the arm position conform all along the path.

Again, Martin:

> can't speak for Bill Powers, but for myself I would not find the
>discovery of some accurate open-loop actions to disturb my view of PCT,
>specially at the environmental interface where things happen very fast.

This is something of a contradiction, because when things happen very fast control gets very much worse. Perhaps we are thinking of different tolerances to go with "accurate." When disturbances and reference signals change slowly enough, the error allowed normally matches the lower limits of perceptual resolution -- no error at all as far as we can see. An open-loop action by definition has no feedback effects. In order to come even close to producing an intended effect, such an action has to be protected against disturbances and executed with effectors whose calibration remains exactly what it needs to be. Alternatively, the action itself has to be perceived and controlled very precisely with respect to an inner pattern. Of course, the target can't move, either --in fact, the whole environment has to remain frozen in the condition that was expected when the action was planned. The open-loop control we can achieve in real environments has, at best, a very low loop gain. If the control is PERMANENTLY open-loop (you never open your eyes to see where your hand really is) it will very soon cease to work. If you do periodically close the loop and recalibrate, your ability to resist disturbances and cope with changes in the environment will have a very low bandwidth -- half the sampling frequency at best.

I think that the more you look at the details of control theory as it applies to behavior, the more you will swing to Rick's view -- perhaps even duplicating some of his overshoots.

Sending time -- best to all

Bill P.

Date: Wed Oct 23, 1991 7:39 am PST
From: Oded Maler
Subject: Re: Where's the opposition

I think there is some weak point in PCT (as I understand it):

Given some level, there is a reference signal (wherever it might come from), there is the pereceptual signal, and there is a corrective action working in the opposite direction of the difference between this two signals. This (plus the hierarchical strucuture) is, as I understand it, the essence of the theory.

Mathematically speaking, the assumption underlying this model is that the set of all possible signals is linearily ordered, and that subtraction is defined. Otherwise, the notion of difference between signals, and a corrective action are not well-defined. These conditions are fullfilled in the thermostat case, but violated already in any two-dimensional signal space. When it comes to higher-level perceptions, the notion of difference between the perceptual signal of what I want to see (e.g., my grandmother) and what I actually see are incomparable, or, at least, if they are comparable and

subtractable this is in a much more complicated sense than temperatures are. One can, of course, say, that some hierarchical decomposition can solve this problem in principle. What I would appreciate, though, is an explanation of how this might work one level above the intensity level, i.e., the perceptual&reference signals are some combinations of lower-level intensities.
--Oded

53z

Date: Wed Oct 23, 1991 1:14 pm PST
From: marken
Subject: Re: Where's the opposition

[From Rick Marken (911023)]

Oded Maler (911023) says:

>I think there is some weak point in PCT (as I understand it):
> When it comes to higher-level perceptions, the notion of difference
>between the perceptual signal of what I want to see (e.g., my grandmother)
>and what I actually see are incomparable, or, at least, if they are comparable
>and subtractable this is in a much more complicated sense than temperatures
>are. One can, of course, say, that some hierarchical decomposition can solve
>this problem in principle. What I would appreciate, though, is an explanation
>of how this might work one level above the intensity level, i.e., the
>perceptual&reference signals are some combinations of lower-level
>intensities.

At least one student seems to have had a similar problem with my description of the control model when I gave my talk as UCSB.There are several ways to approach an answer. One way is to simply look at a simulation (such as the Powers/Williams "Little Man" Demo or my Spreadsheet hierarchy) of a system that controls multidimensional variables and see how it works.

Another approach is to recognize that in PCT, control systems always control a unidimensional perceptual signal (p) relative to a unidimensional reference signal (r) by generating a unidimensional error signal (e) which may be apportioned to many lower level systems. The signals r,p and e are all variables in the same dimension -- say, impulses/sec. So r and p are always "comparable and subtractable". It is what p represents that determines what the control system controls. p is some function of environmenal variables, q.i -- so p = f(q.i). The function, f, can be quite complex; such as the one that turns the patterns of lights, sounds and smells into a p that represents a variable aspect of grandma (such as your distance from her).

From the perspective of any control system, all it is doing is keeping p = r. The "meaning" of p comes from the function, f, that transforms q.i into p. But all p's look the same from the control systems point of view. From the point of view of conscious subjective experience, however, the perceptual signal that represents distance from grandma looks a lot different than the perceptual signal representing, say, distance from an on-coming train. This gets into the question of why neural signals look and feel the way the do when they ARE you. Why does a set of voltages moving up my Vth cranial nerve at the rate of ?/sec feel like a tooth ache? Why does another signal in my IInd cranial nerve look like Michalangelo's David? Heavy question. I just think that this is the way neural signals look when you ARE them -- and the qualitative differences in experience I attribute to differences in both the nature of the function f AND where it is in the hierarchy (what kinds of signals are coming in from lower levels).

Actually, there is a kind of "Kantian" aspect to PCT in the sense that the model currently assumes that there are "built in" ways of experiencing whatever is out there; it's not the built in Kantian categories, but the perceptual classes of the different levels of the hierarchy. The PCT model assumes that the nervous system comes ready to experience the world in terms of intensities, sensations, configurations, etc. It doesn't necessarily determine which instances of these classes will be experienced; just that whatever is out there will be experienced in terms of these categories. There is some work by Franz Plooij and his wife on chimpanze development that suggests that other species also experience the world in terms of these categories. This makes sense -- evolution-wise--and it raises the interesting possibility that creatures like chimps experience the world as we do -- they just might not be able to experience it at some of the higher levels (principles and system concepts).

I'm sure Bill Powers can give a far clearer answer to your excellent question Oded.

Best Regards

Rick

****

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

Date: Wed Oct 23, 1991 2:15 pm PST
From: Gary A. Cziko
Subject: Walking: The control of vision

[from Gary Cziko 911023.1640]

Chuck Tucker (911023) quotes from Lieberman's new book: (UNIQUELY HUMAN, Harvard Press, 1991):

> You can easily, literally "see" that the brain network that
> [p. 29] stabilizes vision really exists. Simply position your
> right index finger against the right side of your right eye and
> gently push it inward. The reason the image jumps is probably
> that there was not selective advantage for any activity that
> involved people's pushing against their eyes. Therefore, natural
> selection for the integration of eye movement from finger pushing
> never occurred [p. 30]."

To make this effect even more dramatic try the following. Stand up. Close one eye. Then push on the side of the open eye with your finger. The perceived motion makes you fell a bit unsteady, doesn't it? Now, walk a straight line with one eye open and then while walking disturb the eyeball with your finger (give a nice steady push and HOLD it there). You may find that you can no longer walk a straight line. If you pushed on the right side of your right eyeball, you will likely veer to the right (because your brain thinks your body is leaning to the left, it will "compensate" and send you off to the right). I actually feel that I will fall down if I don't stop walking and don't stop pushing although some of my colleagues here can continue quite well, but still report how odd it feels.
Let's get some data from others out there on the latest PCT portable demonstration (is somebody keeping a list of these?).

--Gary

Date: Wed Oct 23, 1991 3:08 pm PST
From: marken
          EMS: INTERNET / MCI ID: 376-5414
ubject: Walking: The control of vision

[From Rick Marken (911023b)]   Gary Cziko (911023) says:

>To make this effect even more dramatic try the following. Stand up. Close
>one eye. Then push on the side of the open eye with your finger. The
>perceived motion makes you fell a bit unsteady, doesn't it?

Damn near knocked myself over when I was walking.

What a splendid demo. Everyone should try it -- but be CAREFUL. I was truly amazed at the power of this little demo. I would have imagined that walking could have been carried out quite well even if the visual input were disturbed -- but no way!

Amazing PCT demo #?

Nice going Gary.   Hasty Bagels.

Rick

Date: Wed Oct 23, 1991 3:31 pm PST
From: mmt
Subject: Re: where's the opposition

[Martin Taylor 911023 18:00]
(Rick Marken 911022c and Bill Powers 911023.0700)
(Rick first)

>Martin Taylor (911022 16:10) finally sees just how subversive PCT is.

Not yet, I don't.

>Hold on to your paradigms -- here we go. Martin says:

>>If you claim that all aspects of behaviour are part of the perceptual
>>control function,

>Only the intentional ones.

OK. That does two things. Firstly it makes the whole argument circular, and secondly it omits a lot of what people actually (observably) do. (Actually, three things--it raises the question of conscious intentionality as opposed to unconscious intentionality, which is a can of worms that Bill is careful to avoid except when he deliberately opens it.)

>>   and that is the essence of PCT, then that is a bigger
>>claim than I have understood in my few months of reading this list.

If you now restrict it to intentional behaviour, it's not a bigger claim. I think the great benefit of PCT is its clear statement of what it means to be purposeful, and of what purpose is. My reading of a lot of your (Rick's) writing is that the claim is much bigger. Certainly the posting to which I finally reacted seemed to indicate as much. If you make that restriction, then the discussion comes down to the definition of intentional behaviour--and I can almost hear you say "why, it's almost all behaviour, and all behaviour that psychologists study."

>Now you see why its hard for PCT types to make friends.

No, I still don't. And I don't really think it is true that PCT is a threatening theory.

[A lot of stuff about open-loop actions, which more or less mirror Bill's discussion of the fire-hose and the cannonball. I agree with it, so won't comment. I think I understand it well.]

>If you
>do find one instance of open-loop CONTROL you would then have to develop a
>model that can control in that situation as well as when the loop is closed.

Open loop CONTROL is a contradiction in terms. Might as well ask to find an instance of a green red paint.

>[...]
>>But you specifically identify "behaviour" with "produce
>>controlled results," and if you make this identification you have a private
>>definition of behaviour.

>Nope -- very public.

OK. Substitute "idiosyncratic" for "private." It's like Bruce Nevin trying to define the word "cause" to mean a situation that could never occur.
>[...]

>The problem is, how do you
>know when to apply the physical models and when to apply the psychological
>models. Conventional psychology says "whenever you want" basically. In fact,
>they just try to let physical models get away with being psychological
>models. PCT says no -- physical models apply when there is no control;
>psychological models apply when there is control.

You can't get away from physics. You are in a physical universe, whether you "intend" to be or not. Physical models ALWAYS apply, so long as their underlying assumptions are met. I assume that what you mean is that the wrong physical models are used by psychologists. PCT is a very physical model, and as such it cries out to be developed in its full (temporal) glory.

>>Of course one cannot produce controlled results
>>in an open loop situation, but one can produce predictable results, if
>>the environment is stable enough. I can do many things in my house, like
>>putting my hand directly on a doorknob, in the dark with no feedback that
>>is obvious to me except for the final success or failure of the action.
>>I don't think that is "controlled" behaviour, although control loops
>>certainly play a big part in my actual movements. We DO do things without >>depending on feedback, sometimes. Does this deny the validity of PCT?

>How does the nervous system know how and when to change its organization
>from open to closed loop? Is your nervous system organized as an open
>loop system when its in a stable environment and as a closed loop system when
>its in a disturbance prone environment? How does it know which kind of
>environment it's in? Feedback?

Of course, feedback. And what do you mean by "the nervous system?" The loop that would ordinary perceive the distance from doorknob to hand has feedback, too, but it in feedback that says "I have no information on how we are doing compared to the reference." And there's feedback in the higher-level loops to do with the intended percept of feeling the doorknob, or of completing the action of which opening the door is part. And there's feedback in all the muscular involvement in the movements. Bill has a better answer on this, which is the substitution of the imaginative (read efferent copy) for the normal feedback based on sensory receptors.

On dynamic attractors, Rick says:

>Wrong. Dynamic attractors also fail to behave properly when disturbances
>are continuously varying. They don't control.

I'm not clear what is meant by "fail to behave properly." The orbit always tends back to the attractor, opposing the disturbance. There is NO way to distinguish between an autonomous dynamic attractor and a control system except by changing the reference level of the control system, a concept that does not apply to the autonomous attractor and its basin. The attractor basin may have any shape. The resistance of the system to disturbance may be soft or hard, just as it may be with a control system of lesser of greater gain. (See also reply to Bill)
>>I sense a kind of scientific ghetto-mindedness in this, and I don't like
>>it. I can't believe that the power of PCT stands or falls on whether
>>there exists a situation in which people act accurately without feedback.

>I don't know what ghetto-minded is? PCT has been ignored. So?

No, it's not other people ignoring PCT. It is what I perceive in many of your postings as a behaviour whose object seems to be to ensure that PCT remains ignored. Your reference level seems to be to maintain a separation and an in-group. (I grant that this is less evident in your writings than in your postings to this group). Bill has referred to his feelings on this matter, and acknowledged conflict. That I can understand. I feel the same way when people start using (or mor commonly misusing) my Layered Protocol theory of communication. But it is not healthy for the development of the theory to behave so, and Bill explicitly recognizes that.

>[...]
>It's a nice ghetto --great bagels. Everyone is welcome and we like to go
>outside for a visit occasionally.

Nice comment. How about inviting people in, too. It IS a nice ghetto. I'd rather it was an ordinary part of town.

>When I see fewer ANOVAs and more "tests for the controlled variable" in
>the Journal of Experimental Psychology, then I'll move out of the ghetto.

I totally agree about JXP and ANOVAs. Many years ago, I expounded the notion (with which I still agree) that if I saw a significance level mentioned in a paper, my first thought was that the author did not know what he was talking about, and my next (if the first was disproved) was that the editor had insisted. (ANOVA itself isn't so bad, but its use is usually a sign of its misuse, so it has become a trigger signal as well).

>I guess you feel that PCT is one of many valid views of various aspects of
>behavior. Maybe my ghetto mentality is to feel that PCT is a whole new
>approach to understanding life -- one which is incompatible with what you
>consider to be other legitimate perspectives.

I do think PCT is a magnificent insight, and "a whole new approach to understanding life". To see the purpose of an action as the main object of study is not new, but to see it in a way that can be truly explored--that's new and most important. It has greatly affected my way of looking at the world. BUT--I cannot see that any valid way of looking at the world could possibly be incompatible with other valid ways. In my career, I have found three previous books/ideas to be foundational: Garner's approach to information, J.G.Taylor's notion that behaviour is the cause of perception, and Watanabe's approach to knowledge. I think PCT is the fourth, especially as it so close to and so compatible with JGT's ideas.

No. I don't think PCT is subversive, just wonderful.

(Now Bill P)

>As to strange attractors, pendulums, and the like, there are two
>distinctions between the behavior of such systems and the behavior of
>real control systems.

I'm not talking about "strange" attractors, here (other times, perhaps).

>First, the energy that such systems use to counteract disturbances comes
>from the displacement that the disturbance put into the system.

True, but only the disturber can tell that, and then only by careful measurement, and it doesn't affect the behaviour of the system.

>If you add a constant velocity to a strange attractor, the phase-plane diagram
>will drift upward. When you remove the velocity, the upward displacement
>creates the gradual return to the original fuzzy trajectory. In a
>pendulum, it's even easier: you disturb a damped pendulum by altering its
>potential energy (you give it a push). It then swings back, dissipating
>the energy you put in. So the pendulum is a purely passive system.

Unless you know what the structure is of the basin of attraction, and have a good model for why it is that way, you can't know that it is passive. If you say "force" rather than "velocity" you have a better case, but you still can't make the determination of whether it is a control system or an autonomous attractor system. In a gravity-free environment, you could control the pendulum to return, and an appropriately designed control system would do it with the same dynamics.

>Second (really another version of "first"), strange attractors and
>pendulums have a very low loop gain. If either one had a high loop gain,
>you would find it very difficult to insert a disturbance in the first
>place.[...]

What I think you really mean is that near the attractor there is a linear relation (to a first approximation) between force across the attractor and displacement from the attractor. But this is also characteristic of a good control system. It approaches its referent, shall we say, "Genteely". It does not return at great velocity and stop.

>I have worked with control systems having a loop gain of one billion.
>Disturbances of such a system have no measurable effect.

Attractors can have basin bottoms of any shape. If the bottom is steep, you can push all you want without showing any measurable effect.

>[...on open loop behaviour]
>So even "groping in the dark for the doorknob" is a process in which all
>the perceptual variables currently being sensed are under tight control;
>you maintain balance, you make groping movements, and at all times remain
>resistant to disturbance as the reference-positions of the parts of your
>body are changed.

>We are so used to this tight control over the details of our behavior
>that we don't even use the word "control" for it. We just call it
>"doing." If I want to pick up a glass, I just "reach" for it, not

>realizing that the reaching reflects a changing pattern of position and
>velocity reference signals with the muscles actively making the arm
>position conform all along the path.

All well understood, and completely agreed. I guess I didn't word my comments very well, or Bill would never have had to say this.
>Again, Martin:

>>I can't speak for Bill Powers, but for myself I would not find the
>>discovery of some accurate open-loop actions to disturb my view of PCT,
>>especially at the environmental interface where things happen very fast.

>This is something of a contradiction, because when things happen very
>fast control gets very much worse.

That's why I might expect some actions to go open-loop there. There is sufficient consistency in the environment over the time scale of the action that higher-level control systems can be expected to work with predicted results rather than controlled results. Like the control of the aiming of the cannon, control can be good, and accurate, at the higher levels, in the absence of control at the lower levels.

>Perhaps we are thinking of different
>tolerances to go with "accurate." When disturbances and reference signals
>change slowly enough, the error allowed normally matches the lower limits
>of perceptual resolution -- no error at all as far as we can see.
>An open-loop action by definition has no feedback effects.

Can we add "at the level of the intention to perform it"? All actions have feedback effects, whether intended or not, whether controlled or not, whether executed by a living organism or not.

>In order to
>come even close to producing an intended effect, such an action has to be
>protected against disturbances and executed with effectors whose
>calibration remains exactly what it needs to be.

Those are the conditions I was postulating.

> If the
>control is PERMANENTLY open-loop (you never open your eyes to see where
>your hand really is) it will very soon cease to work. If you do
>periodically close the loop and recalibrate, your ability to resist
>disturbances and cope with changes in the environment will have a very
>low bandwidth -- half the sampling frequency at best.

Well understood, and agreed.

>I think that the more you look at the details of control theory as it
>applies to behavior, the more you will swing to Rick's view -- perhaps
>even duplicating some of his overshoots.

I hope not. I usually try to avoid overenthusiasms. That way lies fad--and fallacy. Let the good remain good, and let us not be overwhelmed by it.
Sorry for the length of this. PCT is worth it (to me).
Martin Taylor

Date: Thu Oct 24, 1991 3:07 am PST
From: Bruce E. Nevin

Subject: CFP, modelling neural systems

***** 5800 0

Received: from LABS-N.BBN.COM by CCB.BBN.COM ; 23 Oct 91 15:00:32 EDT Received: from IZAR.BBN.COM by LABS-N.BBN.COM id aa13399; 23 Oct 91 15:00 EDT Received: from ICARUS.BBN.COM by IZAR.BBN.COM id aa12452; 23 Oct 91 14:56 EDT Received: by icarus.bbn.com (5.52/890607.SGI)
(for neural-people@izar.bbn.com) id AA02746; Wed, 23 Oct 91 14:53:40 EDT Date: Wed, 23 Oct 91 14:53:40 EDT
Message-Id: <9110231853.AA02746@icarus.bbn.com>
To: neural-people@icarus.bbn.com, machine-learning@icarus.bbn.com Subject: [jbower@cns.caltech.edu: CNS*92]
From: "Albert G. Boulanger" <aboulanger@BBN.COM>
Sender: aboulang@icarus.bbn.com
Reply-To: aboulanger@BBN.COM

Date: Tue, 22 Oct 91 21:47:51 PDT
From: Jim Bower <jbower@cns.caltech.edu>
To: connectionists@q.cs.cmu.edu
Subject: CNS*92

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First Annual
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Submission Procedures: Original research contributions are solicited, and will be carefully refereed. Authors must submit six copies of both a 1000-word (or less) summary and six copies of a separate singlepage 50-100 word abstract clearly stating their results postmarked by January 7, 1992. Accepted abstracts will be published in the conference program. Summaries are for program committee use only. At the bottom of each abstract page and on the first summary page indicate preference for oral or poster presentation and specify at least one appropriate category and and theme. Also indicate preparation if applicable. Include addresses of all authors on the front of the summary and the abstract and indicate to which author correspondence should be addressed. Submissions will not be considered that lack category information, separate abstract sheets, the required six copies, author addresses, or are late.

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CNS*92 Submissions
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DEADLINE FOR SUMMARIES & ABSTRACTS IS January 7, 1992

please post

Date: Thu Oct 24, 1991 6:09 am PST
From: Bruce E. Nevin
Subject: cause, 2 senses of purpose

[From: Bruce Nevin (911023 0741)]

(Martin Taylor--911021 1910)--

(Bruce Nevin (911008 0742))
>
>I think that to make this point we might say that causation is defined
>such that "A causes B" means "If A, then B, 100% of the time, no
>exceptions."

(Martin Taylor 911023 18:00)--

>It's like Bruce Nevin trying
>to define the word "cause" to mean a situation that could never occur.

I think you have caused me to feel empathetically just what it is like to be 150 messages out of touch with CSG traffic. Even after as short a time as one week (10/8 to 10/15) I couldn't remember the context of the bit you quoted.

You say that this definition is too strong because all effects are contingent. No quarrel. Please note that in context it is part of a suggested reductio ad absurdam argument. The question addressed was how to communicate PCT more effectively. Better proposals have come up since and I willingly abandon this one.

Re cause vs. influence, I had in mind that e.g. *within* an elementary control system (ECS) events are deterministic in a linear way, and that given a reference signal r an input perceptual signal p "causes" (in the restricted sense, 100% of the time) the comparator to output an error signal that is the difference between r and p. This is only to say that it is a linearly deterministic computational device.

Of course a stray cosmic ray could disrupt a synapse, but given those conditions without disruption we have linear, causative relations around the inner part of the loop.

Relations are nonlinear (and contingent) in the physical environment of the whole control system containing the selected ECS. Consequently, relations are nonlinear (and contingent) in the "environment" of that ECS, that is, in the influences on p and r.

In general, this suggests that relations between points considerably separated from one another in the aggregate of the living control system of which the selected ECS forms a part are nonlinear and contingent.

The "100%" definition of causation applies only with strictly controlled conditions. Such conditions obtain within an ECS just as they do within a computer chip, barring disruption of the ECS or other computational device. They do not obtain within a psychologist's laboratory, and cannot. The illusion that better control of experimental conditions would clean up the messiness of results and improve the statistics was the rhetorical target of the reductio. I believe this illusion is part of what fosters acceptance of low statistical standards in psychology.
Maybe I've got this all wrong. Corrective feedback is the motive for putting it out here.

>behaviour whose object seems to be to ensure that PCT remains ignored
>. . . [whose] reference level seems to be to maintain a separation and
>an in-group.

I have had the same concerns about self-ghettoization, hence my recurrent questions about effective communication.

On the other hand, I continue to be impressed by the patience with which veterans respond to questions that would scarcely be asked, or asked in the way they are asked, if people first read some of the published literature. Mea culpa, I know, and so it must be as we each determine the worth of investing more time and effort than it takes to scan CSG email.

[from Gary Cziko 911020.2130]

>A few weeks ago I had an informal chat with an esteemed senior colleague
>about the role of purpose in behavior. He was arguing that purpose is a
>pretty useless concept for understanding behavior. Needless to say, I
>found it very difficult to understand his viewpoint. Then just a few days
>ago, he sent me this excerpt from Tolstoy's _War and Peace_.

> purpose of the bee. But the ultimate purpose of the bee is not exhausted
> by the first or the second or the third of the processes the human mind can
> discern. The higher the human intellect soars in the discovery of possible
> purposes, the more obvious it becomes that the ultimate purpose is beyond
> our comprehension.

[From Bill Powers (911022.0745)]

>Another thought on purposes. . . . the Tolstoyian conception of purpose
>stands squarely in the way. To speak of "the ultimate purpose of the
>bee" implies trying to guess God's Master Plan.

The then pervasive conception of purpose mapped directly onto the conception of the Great Chain of Being. God's Master Plan was precisely the intent. But in the usual construal, this presupposed that humans are the "crown of creation" and the purposes of humans therefore the basis for determining the "purposes" of other creatures. Note that "purpose" throughout is identified with "function" and not with the intent of the creature itself, though not without some slippery ambivalence about "service," "fealty," etc. in keeping with feudalism.
Emergent from this was a scientific view that did not presume human purposes the highest. Tolstoy's survey of perspectives assigning purposes to the bee is explicitly scientific and ecological (Pyotr Kropotkin (1846-1921), a great ecological biologist as well as the "anarchist prince," took inspiration from Tolstoy (1828-1910), and they held one another in mutual esteem.)

Tolstoy's religious conception (of "God's purpose") is explicitly not identified with human purposes:

> Man cannot achieve more than a certain insight into the correlation
> between the life of the bee and other manifestations of life.

I suppose one transform of the conflict today is that between "Green" ecological and "greenback" commercial interests in politics and economic relations.

Last night, I came across the following in Eckermann's _Conversations with Goethe_, which I have kept on the radiator in my bathroom for some months. (The voluminous scientific writings of Goethe (1749-1832) probably deserve to be better known. They are not largely on the descending coattails of his theory of color, eclipsed by that of Newton. One of his concerns was pattern in morphogenesis, a topic of renewed interest today.)

In the entry for Sunday, February 20, 1831 (p. 387, Everyman edition, John Oxenford's translation):

    Goethe then told me of the book of a young scientist, which he could
    not help praising, on account of the clearness of his descriptions,
    while he pardoned him for his teleological tendency.
    "It is natural to man," said Goethe, "to regard himself as the final
    cause of creation, and to consider all other things merely in
    relation to himself so far as they are of use to him. He makes
    himself master of the vegetable and animal world; and, while he
    claims other creatures as a fitting diet, he acknowledges his God,
    and praises His goodness in this paternal care. He takes milk from
    the cow, honey from the bee, wool from the sheep; and while he gives
    these things a purpose which is useful to himself, he believes that
    they were made on that account. Nay, he cannot conceive that even
    the smallest herb was not made for him; and if he has not yet
    ascertained its utility, he believes that he may discover it in
    future.

"Then, too, as man thinks in general, so does he always think in particular, and he does not fail to transfer his ordinary views from life into science, and to ask the use and purpose of every single part of our organic being.

"This may do for a time, and he may get on so for a time in science; but he will soon come to phenomena where this small view will not be sufficient, and where, if he does not take a higher stand, he will soon be involved in mere contradictions.

"The utility-teachers say that oxen have horns to defend themselves; but I ask, why is the sheep without any--and when it has them, why are they twisted about the ears so as to answer no purpose at all?

"If, on the other hand, I say the ox defends himself with his horns because he has them, it is quite a different matter.
"The question as to the purpose--the question _Wherefore?_--is completely unscientific. But we get on farther with the question _How?_ For if I ask _how_ has the ox horns, I am led to study his organization, and learn at the same time why the lion has no horns, and cannot have any.

"Thus, man has in his skull two hollows which are never filled up. The question _wherefore_ could not take us far in this case; but the question _how_ informs me that these hollows are remains of the animal skull, which are found on a larger scale in inferior organization, and are not quite obliterated in man, with all his eminence. [Footnote here supposes this "glimmering of Darwinism" may be "derived from Lamarck."]

The teachers of utility would think that they lost their God if they did not worship Him who gave the ox horns to defend itself. But I hope I may be allowed to worship Him who, in the abundance of His creation, was great enough, after making a thousand kinds of plants, to make one more, in which all the rest should be comprised; and after a thousand kinds of animals, a being comprising them all--man.

"Let people serve Him who gives to the beast his fodder and to man his meat and drink as much as he can enjoy. But I worship Him who has infused into the world such a power of production, that, when only the millionth part of it comes out into life, the world swarms with creatures to such a degree that war, pestilence, fire, and water cannot prevail against them. That is _my_ God!"

Maybe this sheds a bit more light on antecedents of the rejection of teleology by science and by Gary's esteemed senior colleague.

In these passages, it appears that what science was rejecting was anthropocentrism, in which "purpose" meant "function" from a human perspective. Only much later was it a rejection of mentalism. (Yes, I know Descartes rejected Aristotelian teleology in the non-mental half of his dualism, but that's a dispute in theology, I think, and not psychology.) If Gary's Esteemed Senior Colleague (I almost wrote "ECS," but that acronym is already taken!) intends seriously to refer to the confusion of issues in the passage from Tolstoy, then the distinction between intention within the organism and utilitarian function (=correlation with intentions within other organisms, either humans or ecological neighbors) might be a good place to start. The former concerns how to account for an organism's behavior; the latter concerns how it may benefit or injure others, cp. humans. A difficulty: to start here is to initiate discussion at the problematic nexus of

perceptual _control_ with social _influence_ and the social bases for individuals' setting some of their internal reference values.
Bruce Nevin
bn@bbn.com

Date: Thu Oct 24, 1991 6:40 am PST
From: Bruce E. Nevin
Subject: more on Whorf

***** 6838 0
Received: from BBN.COM by CCB.BBN.COM ; 24 Oct 91 09:30:22 EDT Received: from ricevm1.rice.edu by BBN.COM id aa24988; 24 Oct 91 9:28 EDT Received: from RICEVM1.RICE.EDU by RICEVM1.RICE.EDU (IBM VM SMTP V2R1) with BSMTP id 4202; Thu, 24 Oct 91 08:22:41 CDT Received: from RICEVM1.BITNET by RICEVM1.RICE.EDU (Mailer R2.08 R208004) with BSMTP id 1583; Thu, 24 Oct 91 08:22:37 CDT
Date: Thu, 24 Oct 1991 08:09:20 -0500
Reply-To: The Linguist List <linguist%tamsun.tamu.edu@ricevm1.rice.edu> Sender: "LINGUIST (The LINGUIST Discussion List)" <LINGUIST%tamvm1.bitnet@BBN.COM>
From: The Linguist List <linguist%tamsun.tamu.edu@ricevm1.rice.edu> Subject: 2.700 Whorf
Comments: To: linguist@tamvm1.tamu.edu
To: Multiple recipients of list LINGUIST <LINGUIST@tamvm1>
--Linguist List: Vol-2-700. Thu 24 Oct 1991. Lines: 123
Subject: 2.700 Whorf

Moderators: Anthony Aristar <e311aa@tamuts.tamu.edu>
Helen Dry <hdry@emunix.emich.edu>

-------------------------Directory-------------------------------------
1)
Date: Wed, 23 Oct 1991 08:44 CDT
From: PETER GINGISS <ENGLAD@Jetson.UH.EDU>
Subject: Re: Whorf

2)
Date: Wed, 23 Oct 91 17:16:06 EDT
From: Willett Kempton <willett@Princeton.EDU>
Subject: Whorf and color

-------------------------Messages--------------------------------------1)
Date: Wed, 23 Oct 1991 08:44 CDT
From: PETER GINGISS <ENGLAD@Jetson.UH.EDU>
Subject: Re: Whorf

Thanks again, everyone, for the suggestions on essays. Suggestions included Whorf's Collected Essays, essays by Sapir and Bloomfield, G. Pullam's book, The Great Eskimo Vocabulary Hoax, essays by Sir William Jones and by W. D. Whitney, Carter and Nash's "Seeing Through Language," Coupland's "Styles of Discourse," and Freeborn's "Varieties of English," and works by philosophers such as Austin, Searle, Grice, and Stalnaker. I hope I have not omitted anything, here. Maybe because of the recent discussion here, several suggestions included Whorf, and indeed reading Whorf in my own undergraduatge career got me started in linguistics. Again, I was delighted with the responses.
Peter Gingiss

____ 2)

Date: Wed, 23 Oct 91 17:16:06 EDT
From: Willett Kempton <willett@Princeton.EDU>
Subject: Whorf and color

I'm a coauthor of the Kay and Kempton study discussed in several earlier messages. (I don't follow this newsgroup regularly, but a colleague passed on the thread.) As pointed out earlier, from the tangled cluster of hypotheses referred to as the Sapir-Whorf hypothesis, we tested only one question: Do the lexical categories of a language affect non-linguistic perceptions of its speakers to a non-trivial extent? (P. Kay & W. Kempton, "What is the Sapir-Whorf Hypothesis?", American Anthropologist, vol 86, No. 1, March 1984.)
Considering the complexities of proir research efforts, our primary experiment was simple: Present three color chips (call them A, B, C) to speakers of two languages, such that colors A and B are slightly more different in terms of (universal) human visual discriminability, whereas B and C have a linguistic boundary separating them in one language (English) but not the other (Tarahumara, a Uto-Aztecan language). As noted earlier, the English speakers chose C as most different, whereas the Tarahumara chose A or split evenly (there were actually eight chips and four sets of relevant triads).

I'll add a couple of points of interest that were either buried in that article, or have not appeared in print. First, as the speaker of a language subject to this perceptual effect, I would like to report that it is dramatic, even shocking. I administered the tests to informants in Chihuahua. I was so bewildered by their responses that I had trouble continuing the first few tests, and I had no idea whether or not they were answering randomly. In subsequent analysis it was clear that they were answering exactly as would be predicted by human visual discriminability, but quite unlike the English informants.
An informal, and unreported, check of our results was more subjective: I showed some of the crucial triads to other English speakers, including some who had major committments in print to not finding Whorfian effects for color (several of the latter type of informants were available on the Berkeley campus, where Kay and I were). All reported seeing the same effects. We tried various games with each other and ourselves like "We know English calls these two green and that one blue, but just looking it them, which one LOOKS most different?" No way, the blue one was REALLY a LOT more different. Again, the Tarahumara, lacking a lexical boundary among these colors, picked "correctly" with ease and in overwhelming numbers. The article includes the Munsell chip numbers, so anyone can look them up and try this on themselves.

Some of the triads which crossed hue and brightness were truly unbelievable, as it was perceptually OBVIOUS to us Engligh speakers which one was the most different, yet all the visual discriminability data were against us. (The article did not mention the hue/brightness crossovers for the sake of simplifying the argument in print.)
Our second experiment, like the original visual discrimination experiments, showed only two chips at a time. We additionally made it difficult to use the lexical categories. And we got visual discrimination-based results, even from English speakers. So there are ways to overcome our linguistic blinders. (Which we knew already, or the original visual discriminability work could not have been done in the first place.) I don't feel that the differences across these tasks was adequately explored, and represent a golden opportunity for a research project or thesis.

I didn't expect to find this. The experiment was a minor piggy-back on another project. I believed the literature and the distinguished scientists who told me in advance that we wouldn't find anything interesting. The experiment was going to be dropped from the field research, saved by a converstion at a wine party with a "naive" sociologist (Paul Attewell) who had read Whorf but not the later refutations.
A simple experiment, clear data, and seeing the Whorfian effect with our own eyes: It was a powerful conversion experience unlike anything I've experienced in my scientific career. Perhaps this all just goes to affirm Seguin's earlier quote, as applying to us as both natives and as theorists:

"We have met the natives whose language filters the world--and they are us."

- Willett Kempton
  willett@princeton.edu

____ Linguist List: Vol-2-700.

Date: Thu Oct 24, 1991 7:02 am PST
From: Gary A. Cziko
Subject: Portable PCT Demos

[from Gary Cziko 911024.0905]

I said (911023):

>>To make this effect even more dramatic try the following. Stand up. Close
>>one eye. Then push on the side of the open eye with your finger. The
>>perceived motion makes you fell a bit unsteady, doesn't it?
Rick Marken (911023b) responded:

>Damn near knocked myself over when I was walking.
>What a splendid demo. Everyone should try it -- but be CAREFUL.
>I was truly amazed at the power of this little demo. I would have
>imagined that walking could have been carried out quite well even if
>the visual input were disturbed -- but no way!

To complete the demo, three more steps should be added. First, reverse eyes to see how you now stagger to the other side to show that this is a systematic effect. Second, walk a straight line with eyes closed and then push on the eyeball. No problem (except for the normal "drift" of trying to walk straight with no visual feedback). Therefore, it is not just pushing on the eyeball which causes the instability. Finally, walk with the one eye open while making lots of saccades. No problem. Therefore, it is not just moving images on the retina which causes the problem. The problem is caused only by having the retinal image disturbed by some "outside" factor.

For even more excitement, try this while riding a bicycle (now I know why I wear a helmet). Doing it while driving a car is also amusing (make sure first that you are insured against PCT demos). Any airplane or space shuttle pilots out there?

* * * *

Let's see what my list of portable PCT demos looks like now:

1. Powers's classic rubber band demo of keeping the knot of two knotted rubber bands (elastics) over some fixed point with the subject's (controller's) finger in one loop and the disturber's finger in the other. Demonstrator can be either subject or disturber. (I know Clark McPhail has done some social demos using giant rubber bands in his sociology courses. Perhaps he could fill us in on these.)

2. The demonstration of the levels of the hierarchy described in Powers (1973) and Robertson and Powers (1989) involving hand movements.

3. Speaking while keeping the tongue in some relatively fixed position, e.g., tip touching upper or lower teeth.

4. Eyeball pushing and walking.

5. Finger pointing. This one is particularly effective after showing Powers's computer demos 1 and 2 of the phenomenon of control and the control model. Close one eye. Reach out with one arm with index finger extended and put the finger where it "touches" some distant object. Keep it there and watch how your arm seems to take on a life of its own as it actively compensates for disturbances caused by breathing, heartbeats, body movement, muscle fatigue, etc. It looks very much like the cursor in the computer demos. Then decide to move your finger a certain distance above or below where it was. When I change the target, I get a feeling of "willing" which is very different from just maintaining the finger in a certain spot. This lower-level maintenance of finger position feels like it running on a sort of automatic pilot. I do not feel as if I am actually in control of the little movements needed to maintain the finger on the target. And of course, in a sense I am not. All (higher-level) "I" can do is specify the reference signal for the lower systems and then they do their job without any further assistance from the higher levels.

Are there other portable demos that I have missed? If we get enough of these maybe PCT may start taking over classic psychology in introductory courses just because it will be so much more fun!- -Gary

Date: Thu Oct 24, 1991 7:03 am PST
From: mar
Subject: Social control & crowd behaviour

I read an article in "The Sunday Times Magazine" this weekend which I think is quite relevant to us. This is a real experience which confirms Bill's views on social control.

I'm posting some bits of the article. I hope I won't have any problems with copyright, since this is for the sake of a scientific study, not for commercial purposes.
Bill Buford, a British journalist, went to Italy during the last football world cup aiming to understand "what force triggers the violent transition of a seemingly peaceful football crowd into a rampaging mob". The journalist, who joined a group of England supporters, describes the experience:

"... we made our way to the city centre. It was four o'clock -- five hours before the match...
I had spotted a supporter I recognised from the night before. I introduced myself, adding tentatively that I was writing about football suporters.
``Six o'clock'', he said. It wasn't a reply or a greeting; I'm not sure what it was - a declaration perhaps. He then repeated it, looking straight at me. ``Six o'clock''. He said it slowly, as if I didn't speak English.
``Six o'clock'', I said.
``That's right'', he said. ``Six o'clock. Pass it on.'' A group of supporters walked past; he stopped them. ``Six o'clock, mates. Got it?'' He said it in an intense whisper. ``Pass it on''. ...
It seemed to me that in the short time I had been standing there hundreds of people had probably learned about it: the march. That was the word that everyone was using: the march.
Mutton-chops explained what would happen. On the dot of six the lads, all 4000, would set out and head down the Via Roma, walking against the traffic, in such numbers that the city would come to a standstill. That would be the march. ``Then they'll know we're here'', he said. He repeated the sentence, with emphasis: ``Then they'll know we're here. It will be f***** brilliant'', he said. ``We will take over the f***** city''. Then he added, as though for clarification: ``They won't be able to f***** stop us now''.
...

I had little idea that I was about to see this motley assembly of 4000 vagrant football suporters pass rapidly through a sequence of identities. That it would become a crowd, then a violent crowd, then a very violent crowd and this is how it happened.

Everyone was ready. There wasn't a supporter among the 4000 who did not know about the six o'clock march. By 5:45 most people had gathered in front of the railway station, which was crowded now and active with a low, steady buzz that seemed to come from the same concentrated whisper. There wasn't a single person sitting. Everyone was up - and ready. And then when six o'clock came - nothing. I looked for Mutton-chops, but couldn't find him. Everyone was looking at everyone else, waiting. A whole minute elapsed, slowly. A second minute. Nothing. And then somebody - someone who didn't seem to be known by the others - stepped out into the main street. He stepped out conspicuously, in a manner that said, ``The march will now commence''. He strutted bravely into the Via Roma and stopped.

There was a problem. No one had joined him. He hesitated and turned round quickly - right, left, right - looking for the others. They weren't there. And then a choice was made. Two others joined him, friends it seemed, who had been behind him but inhibited from taking the first step. They then stopped and looked round, panickly. No one had followed.

I thought ``Perhaps the march at six o'clock will be three nervous friends standing in the middle of the Via Roma''.

Then two others stepped out, equally deliberate. No one was singing ``Here we go''. There were no chants for England. No one had shouted ``Come on, lads'' - if only to urge others to follow. It was quiet.

Three others stepped out. And then two more. And then five. And then, suddenly, everybody. Spontaneous consent. Hundreds and hundreds at once. Everyone moved at once. The threshold had been crossed - not by a leader but by the willed consent of everyone there.

This crowd, this new entity, no longer ``they'' but ``it'', filled the Via Roma and the pavement alongside it, trapping cars, buses and lorries in places, just as Mutton-chops had predicted. The crowd, sure of itself, was moving quickly.
...
They were smiling, they were experiencing joy. Everyone was experiencing some kind of foolish joy..."

If anyone wants more reference on this:
`Among the Thugs' by Bill Buford will be published by Secker & Warburg on October 28th, price 14.99 pounds sterling.

Regards,

Marcos Rodrigues
mar@uk.ac.aber

Date: Thu Oct 24, 1991 9:15 am PST
From: mmt
Subject: Re: Portable PCT Demos

[Martin Taylor 911024 12:30]

The eye-push demo is very reminiscent of one of JG Taylor's demos. Your (Gary's) mention of bicycling brought it to mind. JG claimed (and demonstrated) that distortions of vision were corrected iff the distorted components were affected by behaviour as part of the feedback loop involving that behaviour. One of the distortions was to wear spectacles with some kind of prism, such as inverting spectacles that interchanged left with right, or up with down, or merely displaced things (say) 20 degrees to the left. He

mentions in his book the experience of seeing a narrow strip of floor in front of him (on which he would be walking) seem perfectly normal and flat, while on either side (irrelevant to his walking) the floor seemed to be sloping. At the same time, the surface of a table showed no levelling effect during the 13 days of the experiment. At the table, he indulged in no control behaviour (he says) that involved gravity, and thus there was no mechanism for the distortion to correct itself (no feedback to test reality).
But more dramatic was a film, whose present whereabouts is not known to me, of Seymour Papert (of MIT) learning to ride a bicycle while wearing left-right inverting spectacles. At first, as soon as he put on the spectacles, Papert would crash (like Rick's demo of inverting the control display relationship, Papert applied the wrong corrections). After a while, he could stay on, albeit wobbly. But then he would crash when he took the spectacles off while riding. After more training, he could put the spectacles on and take them off quite freely, while maintaining control. But then comes the kicker--JG took the prisms out of the spectacles (the frames were quite heavy), or perhaps he substituted non-inverting prisms. At any rate, Papert's view of the world was normal with or eithout the spectacles. But on putting the spectacles on, he crashed as in phase 1. JG took this to mean that the totality of sensation involved in a situation was all part of the control system, and this included having reorganized the system to include what amounted to a switch based on the weight of the spectacles on the nose (Papert "knew" what was in the spectacles, but his fully trained control systems didn't).

Papert, by the way, contributed a mathematical appendix to a chapter of J.G.'s book, so he was familiar with the theory, though it is hard to see how this could have contributed to the effects. I certainly would not have allowed myself to keep falling off a bike to support someone else's theory of perception!

If anyone in the group has contact with Papert, it is quite possible that he has a copy of this film. At any rate, he ought to have more details on his heroic part in the study. It must have been quite painful-- exceeded in heroism in the cause of psychology only by E.G.Boring's thesis work, I think (not forgetting the man whose name I forget who severed a sensory nerve in his arm to see whether he would regrow it or compensate for its loss).

Martin Taylor

Date: Thu Oct 24, 1991 1:45 pm PST
From: URROBERT
Subject: greetings from dick r [From Dick Robertson]
Hello everyone, I'm finally back on the net after being cut off Oct 1, when our system went over to a new machine. It's good to be back in CSG land. I have just had that (you should pardon the expression) reinforced over the last three days when I attended the fifthieth anniversary of my old department - The Committee on Human Development at the U of Chicago. One of my house guests said to another, "Don't point your finger at me, it makes me point my finger at you." I started out to explain to him how what any other person does can't possibly make him do any particular thing. But then I caught myself. Another impacting experience was a moment of smugness that I had listening to the report of a young researcher who is studying the development of reasoning. She has been using a story completion technique to analyze the different levels of sophistication of different age groups. She asks each group to finish a story about a young man about to go into the priesthood, having a last picnic with his mother on a beach and seeing a young woman to whom he becomes attracted. She is lame and blind. Then she asks, "How would you finish the story.?" (Why this is such a great setup to study this kind of development was never made clear.) Anyway, the researcher found that 10-11 year olds don't consider either one as a unique person, "they are both seen as static categories rather as individuals." The adolescent subjects "did tell stories with a past, present and future, but they led to rigid, fixed outcomes." And the young adults brought in concepts like, "it depends upon what each would want for themselves...." Again I was tempted to jump up and shout,

"But of course, control of category variables is achieved first, then sequence variables and then programs." However, I restrained myself once more. Nobody in the whole audience knew control theory. I suppose they would have been incredulous at the idea that the results could have been predicted in advice. This place bills itself as a place that is interested in "the development of new knowledge." Once greetings, again to everyone.

Dick Robertson

Date: Fri Oct 25, 1991 5:19 am PST
From: POWERS DENISON C
Subject: Objections etc

.LM0/.RM79/[From Bill Powers (911025.0745)]

Oded Maler (911023) --

>Given some level, there is a reference signal (wherever it might come
>from), there is the pereceptual signal, and there is a corrective action
>working in the opposite direction of the difference between this two
>signals. This (plus the hierarchical strucuture) is, as I understand it,
>the essence of the theory.

>Mathematically speaking, the assumption underlying this model is that
>the set of all possible signals is linearily ordered, and that
>subtraction is defined. Otherwise, the notion of difference between
>signals, and a corrective action are not well-defined. These conditions
>are fullfilled in the thermostat case, but violated already in any two-
>dimensional signal space.

I suspect that you haven't yet realized how many control systems we're talking about. At the first level of organization in the human behavioral system, there are at least 800 control systems operating simultaneously, in parallel: one for each major muscle group -- that is, one for each degree of freedom of movement or effort. This estimate counts only kinesthetic control systems based on stretch and tension information at the spinal motor neuron level, and does not take into account loops that involve tactile or temperature senses. In addition to these basic control systems there are many input signals at the first level that are (perhaps) differentiated, then integrated into weighted sums to create sensations. In the visual modality alone there are millions of uncontrolled sensation-signals entering the optic nerve.

At the configuration level the number of degrees of freedom for control is greatly increased, although at most 800 of them can be under control at once because of the limitation at the first level on degrees of freedom of muscle control. Each new level of control introduces a new logical type of perceptual variable, of which there can be hundreds or thousands of examples at a given level, and creates a new space with its own degrees of freedom. Even at the event level, the degrees of freedom are vastly increased over the basic 800, because now temporal patterns are controlled, and each element of the pattern could entail a different pattern of efforts at the first level -- the degrees of freedom become the basic 800 multiplied by the number of successive stages there can be in an event.

I suspect that the number of control systems that can potentially be active increases as level increases, then at some level begins decreasing again until we end up with relatively few independent control systems at the system concept level.

In my model, control of multi-dimensional variables requires one control system per degree of freedom. You may have seen matrix representations of complex control systems that look a lot simpler and neater, but when it comes to physically implementing an operation expressed in matrix notation, you end up having to expand the matrix into all its elemental operations and doing them one at a time (or in parallel) just as in my model. What is missing from my model that would appear in a matrix calculation are the interactions among degrees of freedom within one level of organization. Those have to be imagined as implicit in a set of control systems. Fortunately, control systems resist disturbances, so interactions that are not too severe are cancelled by adjustments of outputs, like any disturbance. Interactions at the input side simply redefine the perceptual function for each system so it includes more lower-level signals as inputs.

I expect that some day these interactions are going to have to be handled much more explicitly. But there is a great deal we can do toward modeling the main features of behavior before we need to consider these complications in order to improve the fit of the model to behavior.
Also, when you speak of "linearly ordering" signals, I get the impression that you're thinking of signals as discrete events, like neural impulses. A signal in my model is an analogue quantity measured in impulses per second. The rate of occurrance of impulses is naturally ordered in the dimension of higher and lower rates, from zero to whatever the channel maximum rate is. The underlying coordinate system is a continuum. Subtraction is easily defined: when an inhibitory signal and an excitatory signal converge on a single neuron, the output rate of firing is proportional to the excess of excitation over inhibition. In other words, a single neuron can be a comparator. A pair of neural comparators, with inhibitory and excitatory roles interchanged, is required for two- way comparison (for control systems that operate in both directions around zero error).

>When it comes to higher-level perceptions, the notion of difference
>between the perceptual signal of what I want to see (e.g., my
>grandmother) and what I actually see are incomparable, or, at least, if
>they are comparable and subtractable this is in a much more complicated
>sense than temperatures are.

Here we see the advantage of assuming one control system per degree of freedom. It is difficult to subtract one grandmother from another, but it is easy to see the difference in height, skin shade, distance of nose below eyes, separation of eyes, color of hair, number of kind words per week, and so on. One-dimensional variables. Control systems do not control "things" but variable attributes of things. Reference signals at any level are quantitative, not qualitative -- even when the result seems to be a qualitative variable. Also, words are ambiguous in that they don't tell us what level of perception to attend to. When you say "grandmother," are you referring to the configuration recognizeable as a given person, or to the attitudes toward grandchildren? The comparisons you make -- that is, the attributes considered -- depend on the level of perception.

Your concern here is legitimate. I can say that comparisons are made attribute-by-attribute, but I have very little that is helpful to say about how attributes of higher-level perceptions are ordered -- that is, what kind of space they exist in. Along what continua do we order slightly different class memberships? To answer that question would be tantamount to revealing the details of how perception of class membership works. How is perception of the class "dog" structured so that a dog that meows like a cat is perceived as a somewhat less convincing member of that class? I don't know the answers; that's why I periodically caution people not to take my definitions too seriously. They pay about as much attention to such cautioning as I do. When we find out how these levels really work, their definitions are probably going to change.

Rick Marken, I see (I'm going through posts one at a time) has given the same answer I do. I echo his appreciation of a pertinent question.

Gary Cziko (911023) --

A very neat demonstration of several things. When you push on your eyeball, the world seems to move -- and your lower-level control systems believe it has moved! This gives a beautiful demonstration of how vitally perception is involved in "doing." Epistemological ramifications, too.

Martin Taylor (911024 and previous) --

Causation:

Causation is a useful concept only when you have one variable that is a clear single-valued function of one other variable. If y = f(x), you can say that the state of x is the cause of the state of y. but if the function is multiple-valued, given only the state of y you can't deduce what state of x caused it. So this rules out multiple valued causal relationships as useful for scientific investigations of causation.
When y = f(x1, x2, ...xn), which is the normal case in the real world, you can attribute causation to xi only in the sense of partial derivatives: holding everything else constant (at known values -- "constant" alone isn't good enough), vary x and see its effect on y. But this assumes that the x's enter additively rather than mutiplicatively. Once again, you can't deduce from the value of y (or a change in that value) which x caused it. And you can't predict the effect of one x without knowing the states of all the others.

Finally, when you have a SYSTEM of equations describing a complex physical situation involving many variables, PARTICULARLY IF THE SYSTEM CONTAINS CLOSED LOOPS as living systems do, causation goes out the window entirely. The solutions of the equations will show that each variable is in part a function of itself or its own integrals and derivatives.
I think that the idea of causation is mainly a product of the human ability to perceive the flow of perception in terms of events, chopping the flow up into packages that can be seen in temporal sequence. We give names to the events and treat them as if they occured at a single point in time. Thus we conclude that causes come before effects, ignoring overlaps in the actual events and certainly ignoring any feedback loops that are effective within the time actually spanned by a single "event" package.

As to the "ordinariness" of control theory (which should be our ultimate goal, of course), I think we would do better in this argument by considering some real contexts in which control theory, in your opinion, is so self-evident as to be trivial, and equally important, some others in which accurate "open loop" control can be seen or a non-control explanation is to be preferred. If we keep this discussion at the level of generalities it will continue to be a "tis-so-taint-so" argument, and shame on us. What are we talking about?

Attractors:

If you mean "plain" attractors, I agree, of course. The metaphor applies perfectly well to a control system. The best control systems have an attractor whose walls look like a razor-cut in a piece of cheese.

But I prefer the control-system analysis, because it's literal. Nothing actually "attracts" the perceptual signal to the reference signal. To put it that way gives the reference signals some pretty odd, if not strange, properties. The perception is really PUSHED toward the reference signal.

>Unless you know what the structure is of the basin of attraction, and
>have a good model for why it is that way, you can't know that it is passive.

I presume you mean the physical structure; I agree. You can mimic the functional form of a control system by generating a suitably-shaped basin (movable horizontally within the phase space so as to reproduce the effect of a reference signal). A marble in that basin will behave just like the perceptual signal, particularly if you put the right grease in the basin and give the marble the right moment of inertia. But the marble will still, physically, be part of a passive system while the control system will not.

Passivity has to do with lack of power amplification. Control systems have to be dissipative systems; they draw on a pool of energy so that their output effects can be greater than the input effects that excite their senses. This is how the amplification happens. The amount of power amplification around a living control loop, even counting losses in the external part of the loop, is enormous. Most of the energy used in this process goes to overcoming resistance in the environment and producing side-effects like heat and incidental events. But the dimensionless loop gain, measured as the effect of a variable on itself, is always large -- usually at least -30 and often far higher than that.

The question is not that of imitating the behavior of a control system. It's understanding how real living control systems are actually constructed.

>>Second (really another version of "first"), strange attractors and
>>pendulums have a very low loop gain. If either one had a high loop
>>gain,you would find it very difficult to insert a disturbance in the
>>first place.[...]

>What I think you really mean is that near the attractor there is a
>linear relation (to a first approximation) between force across the
>attractor and displacement from the attractor. But this is also
>characteristic of a good control system. It approaches its referent,
>shall we say, "Genteely". It does not return at great velocity and stop.

What I would rather mean is that while an attractor (if not strange) can be designed to exhibit the same behavior in the vicinity of its basin that a perception in a control system exhibits near its reference level, an attractor is not a control system. It is an abstraction, and also a misleading metaphor.

In studying living systems, we begin by observing apparent attractors in the neighborhood of organisms. The bee seeks the flower, the child seeks the parent, the student seeks a grade. This seems very little different from saying that the dropping lead weight seeks the earth. In the case of the inaminate system, we discount the "seeking" and emphasize the attractor: the Earth generates an attraction on the lead weight and makes it fall.

But when we apply this same metaphor to the bee, the child, or the student, we put the executive power into the flower, the parent, and the grade. What's the difference? These are all "attractors", aren't they? Metaphorically, yes. In terms of system organization, no.

The mistake of behaviorism was to convert perfectly plain control processes carried out by organisms into "attractors" scattered around the environment. This led to some very strange ways of seeing the physical world. Stimulus objects and events took on "value" or "reinforcing" properties or "discriminating" capabilities. Ordinary physical things suddenly acquired the ability to "control" behavior. They could even "generalize." Behaviorism -- and practically every other life science, for that matter -- searched for the aspects of the environment that MAKE organisms behave as they do. The responsibility for goal-seeking behavior was put into the outward correlate of the inner goal instead of into the seeker.

This is my main objection to using the concept of an "attractor" with respect to behavior. It encourages the same mistake that led to behaviorism.

Another objection is that attractors, for some reason, are always presented as limit cycles. I suppose non-strange stable point-attractors aren't very interesting. The variable goes ZOT to the reference level and stops.

Living control system don't usually exhibit limit-cycle behavior. The perceptual signal just approaches the state of the reference signal and from then on follows the reference signal up and down without any fuss. If the reference signal stops changing, so does the perceptual signal. I get a strong impression that the people who focus on limit cycles think that these cycles are describing behavior, as if all behavior was just a bunch of limit (or is it epi-?) cycles. It's true that there are some limit cycles in behavior, particularly in circadian phenomena at the biochemical level. And even the behavioral systems go into a different mode once a day while we sleep. Those are perfectly good phenomena and studying them is OK. But let's not confuse them with the other 99 per cent of behavior that works like a stable control system, not like an oscillating one. Of course oscillations CAN be under control with respect to phase and amplitude; I don't want to make this too narrow. But the controlled variables would not themselves exhibit limit cycles -- the phase and amplitude would not oscillate -- if the control system was any good.

A little story. In a book on biochemical systems analysis by two Japanese (my reference turns out to have been archived on a floppy somewhere) a model of a feedback system was run to generate curves showing how concentrations changed with time. This happened to look like a very good control system controlling a concentration; there was even a chemical input that looked just like a reference signal to me. In running this model, however, the authors started it in an arbitrary state far from equilibrium, so the curves showed all kinds of oscillations and excursions that went on for almost the entire length of the time plot. But just at the end, the concentration that I took for the perceptual signal steadied down at precisely the same concentration as the reference signal; just before this point, all the oscillations had stopped and the variables came to steady values.

That, of course, is where the simulation stopped, because all the interesting "behavior" had stopped. I felt like calling the authors up and saying "Please, now vary the reference signal and see if the perceptual signal follows it -- what happened off the right end of your plot?" All their simulations were like this: just as the concentrations settled down into the control range, the plots ended. Clearly, the authors thought (at least the one who was a biochemist did) that those wild variations in concentrations were the real behavior of the system. They had a working model of a control system in front of them, and never recognized it because they thought the incidental dynamics after a cold start were the behavior.

As to chaotic attractors, I am highly skeptical about their playing any part at all in normal behavior. Most strange attractors behave like perfectly useful physical systems (oscillators, for instance) that have been driven beyond their normal ranges of operation. They wouldn't be much good for stabilizing the experienced world against disturbances. The very strangeness fuzzes out the limit cycles, which puts an upper limit on the amount of "loop gain" they could imitate -- they would act like very noisy oscillatory basins, if you get what I mean. The analog of the reference signal, in many cases, could spontaneously jump around between very different states, with no relationship to anything else. Chaos is a trendy idea; I wouldn't touch it as part of a model of behavior until it's aged a good deal more. When all the gee-whiz has cracked and fallen off, maybe there will be something left that could help with the problem of reorganization.

>>This is something of a contradiction, because when things happen very
>>fast control gets very much worse.

>That's why I might expect some actions to go open-loop there. There is
>sufficient consistency in the environment over the time scale of the

>action that higher-level control systems can be expected to work with
>predicted results rather than controlled results. Like the control of
>the aiming of the cannon, control can be good, and accurate, at the
>higher levels, in the absence of control at the lower levels.

OK, we're not so far apart here. "Accuracy" is a relative term. A person wearing a blindfold who can spin around and reach out and pick up a glass without knocking it over three times out of four is showing pretty accurate behavior, for a person wearing a blindfold.

Opening the loop ALWAYS drastically reduces the quality of control in a real world where disturbances can happen and in which local properties are subject to little unpredictable drifts. But we will take what control we can get. You can fire cannon quite accurately using calculations based on models, but the meaning of "quite accurately" suddenly shifts when you put guidance systems into the projectiles. Once you've seen how a control system can do the job with the loop closed, you wouldn't want to go back to the old blind method if you didn't have to. Also, you really wouldn't want to fire off a lot of shells without a forward observer telling you where they are landing.

>>An open-loop action by definition has no feedback effects.

>Can we add "at the level of the intention to perform it"? All actions
>have feedback effects, whether intended or not, whether controlled or
>not, whether executed by a living organism or not.

Strictly speaking, no. Feedback isn't the effect of output on input. It's the effect of a variable on itself, via a closed loop. When I move an arm sideways, my muscles inevitably move it a little up and down, too, affecting the up-down sensors. But this is not feedback. It's just a side-effect. If the up-down dimension is under control, the up-down effect is a disturbance that the up-down system will correct. The feedback occurs when a little sideways move affects the sensors involved in controlling sideways motions that affect the same sensors. In formal conversation at least, I would like to stick to this strict definition of feedback. Nobody can "give you feedback" but yourself.

>>In order to come even close to producing an intended effect, such an
>>action has to be protected against disturbances and executed with
>>effectors whose calibration remains exactly what it needs to be.

>Those are the conditions I was postulating.

All right, but I would humbly request that we stop calling this sort of action "control." We need to reserve control as a technical term. When we allow control to be used in cases where we really mean "influence" or "affect" or "determine," the whole concept of purpose gets lost or distorted.

Also we get tempted into short-term thinking. Suppose I give you a dial that adjusts a voltage that adjusts the focus on a projection TV system that a scientific audience is watching. I give you a calibration chart showing how the best-focus voltage varies with AC line voltage. I also give you a meter showing the AC line voltage. I assure you that the dial and meter are accurately calibrated and that the chart is accurate, too. I tell you to come into the projection booth every hour and make sure the TV system is focussed, and then leave by the back door so you won't disturb the scientists.

How long do you think you could do this job without at least peeking out from behind the curtains to see if the picture is REALLY in focus? How long before you start to wonder if line voltage is REALLY the only thing that could influence the focus? You can certainly adjust the dial to maintain the specified relationship with line voltage -- there you can perceive and affect the relevant relationship. But you've been told that this is done in order to adjust the focus, which you aren't allowed EVER to perceive for yourself. Would you really call that "controlling" the focus?

Model-based control works by applying an "efference copy" of your outputs to the input of the model, and controlling, in imagination, for what the model does rather than for what the external world does. This is sometimes necessary and is often useful, especially when the model yields up in real time a predicted future state of the world. I haven't put any such complications into my model, but some day they will be there because we often do things this way.

However, the models must first be constructed, and then they must be frequently updated. The updating means comparing what the model is doing with what the real world (i.e., the lower-order perceptual world) is doing. The discrepancy must then be turned into an adjustment of the model, until the model's behavior is congruent with the real behavior again. This is how real science works, isn't it? Once updated, the model can be relied on again, for a while, in the absence of access to the relevant perceptions from the real world. So the higher-order process that adjusts the model is a sampled control system. The more frequent the sampling, the better. If you can sample continuously, that's best of all -- but then, of course, you don't need the model.

So you see what I meant by getting tempted into short-term thinking. It's easy to forget about the updating phase of model-based control. Without that updating, the behavior would be truly open loop and would have no necessary relationship to anything. You could still have effects on the environment, and of course you would believe that they are consistent effects because your model tells you they are consistent, but these effects would no longer even imitate control. I think that quantum physics is teetering on the edge of this trap. The model is becoming more real than direct experience.

\>>I think that the more you look at the details of control theory as it
\>>applies to behavior, the more you will swing to Rick's view -- perhaps
\>>even duplicating some of his overshoots.

\>I hope not. I usually try to avoid overenthusiasms. That way lies
\>fad--and fallacy. Let the good remain good, and let us not be
\>overwhelmed by it. Sorry for the length of this. PCT is worth it (to me).

It's good to dip into fad and fallacy once in a while. Sometimes you come back with something you later realize is true. Like stopping to think about what a cliche really means. Let reorganization get in there once in a while and shake things up. You'll notice that I'm not threatened by considering shortcomings of control theory. If something's broke, fix it (a corrollary that often seems to be forgotten).

Don't be sorry for the length. The readers have delete keys. You haven't said anything uninteresting yet.

P.S., to Martin and others. If I sometimes seem to explain things in a reply that the correspondent already knows, it's only because I'm aware that there is a considerable range of acquaintance with control theory among subscribers to this list. It's sort of like Washington Week in Review, where the seasoned reporters always say "President George Bush said" instead of just "George Bush said .." You and I know who George is, but some viewers don't.

General note:

Invitations to attend conferences of other groups keep appearing, some to me and some in general. I accept them very rarely and only if the costs are covered (for practical reasons -- limited income). My reason isn't disinterest but a feeling that it's sort of irrelevant for me to do this. If I stood up like a visiting somebody and gave a full-flown lecture on control theory, hardly anyone would understand what I'm talking about and the rest would disagree with it. Just think back on the history of this network conference. There's a tremendous amount of groundwork to lay before PCT and HCT can be understood as I want it to be understood. The people learning it have to do a lot of hard work, and be willing to do it.

I have exactly the "students" I want right now. Smart and willing people who came into this voluntarily and stick around because they want to know more. These are the people who are going to take over. I don't have the kind of detailed acquaintance with other fields of study that is needed to build a bridge to control theory. I don't even know what problems are of interest in these fields. I don't want to get famous and have to get an unlisted telephone number. I'm too old to worry about making a name for myself or achieving a prestigious position. Think of me as a sort of grandfatherly figure, smiling and nodding blearily and getting his kicks vicariously through watching the young folks' achievements, once in a while throwing in a word of encouragement. Well, several words.

I hope that people on this net will accept invitations all over the place and take on the job of introducing control theory where it's needed. This can only be done a little at a time and of course only if you understand it pretty well. You have to know where your audience is and see what little step will help them understand PCT a little better than they do. Maybe you can even advance your careers this way, although we're a little too early in the revolution to count on that kind of result. If you can, do it with my blessing. But I don't want to do it. I'm doing what I want to do, with your help.

I assume, because to assume anything else would be self-destructive, that all this is worth doing.

Best regards, Bill P.

Date: Fri Oct 25, 1991 7:21 am PST
From: Bruce E. Nevin
Subject: developmental stages

[From: Bruce Nevin (911023 1239)]

(Dick Robertson Thu, 24 Oct 1991 16:19:57 CDT)--

>the researcher found that 10-11 year olds don't consider
>either one as a unique person, "they are both seen as static categories rather
>as individuals." The adolescent subjects "did tell stories with a past,
>present and future, but they led to rigid, fixed outcomes." And the young
>adults brought in concepts like, "it depends upon what each would want for
>themselves...." I was tempted to jump up and shout, "But of course,
>control of category variables is achieved first, then sequence variables and
>then programs."

Still working to understand CT dept: I take it this means that the application of these levels of perception to the projection of stories about imagined social relationships follows the same order as their acquisition in other areas of experience. Presumably, control of category, sequence, and program levels had been learned much earlier i.e. for tying one's shoes, wheedling an advance on allowance, speaking English . . .

This suggests a much more complex model of the developmental process than is usually considered, where acquisition of a skill associated with a particular developmental stage itself goes through stages corresponding to the development of hierarchically-linked control systems involved in practicing that skill. Why it should always be the same linear order is not obvious to me. Attempts to learn the program for tying one's shoes may foster learning certain relationship perceptions and sequence perceptions, so that the program is clear but the means for enacting it are not yet available. Relearning after a stroke is surely like this.

What's the scoop?   Fusty lumbago,   Bruce Nevin

Date: Fri Oct 25, 1991 8:23 am PST
From: Gary A. Cziko
Subject: Reality vs. Intentions

[from Gary Cziko 911025]

I had the pleasure of attending a lecture yesterday given by David Hubel (Nobel laureate of Hubel and Wiesel fame) on the visual system. I even got to ask a question and chat with him after his talk (his film which shows how a single neuron in the cat's visual cortex "crackles" only when stimulated by a luminant line of a certain length moving in a certain direction with a certain orientation on a certain part of the cat's retina is quite amazing).

Anway, I asked him about the presence of descending pathways (i.e., from the higher, more central visual areas of the brain to the more peripheral). He replied that they seem to be everwhere although no one seems to know what they do. Wherever there is an ascending pathway, there seems to be a corresponding, specific descending one. This seems to me to be good news for PCT since such pathways would be needed for communicating reference levels from higher to lower levels and for the "imagination" connection.

But what I found quite mysterious was his statement that the same neuron appears to receive signals from both below and above. My problem, therefore, is understanding how the neuron can "know" if what is "tickling" it is based on sensory information or higher-level reference levels. How does it separate "reality" from "intentions"? If the cell is getting both sorts of information, doesn't it mean that the comparator function must be handled by single cells? Is this possible?

Also, while he mentioned that even the lateral geniculate bodies (the first "stop" for visual signals after the retina) is getting information from the visual cortex, I forgot to ask him if the retinal cells themselves received signals from further upstream. Does anybody know about this? Perhaps Joe Lubin will be able to help me to understand this.-

-Gary

Date: Fri Oct 25, 1991 9:17 am PST
From: Joseph Michael Lubin
Subject: retina

[from Joe Lubin 911025.1300]

[Gary Cziko 911025]

> Anway, I asked him about the presence of descending pathways
> (i.e., from the higher, more central visual areas of the brain
> to the more peripheral). He replied that they seem to be everwhere
> although no one seems to know what they do. Wherever there is an
> ascending pathway, there seems to be a corresponding, specific descending
> one. This seems to me to be good news for PCT since such pathways would
> be needed for communicating reference levels from higher to lower levels
> and for the "imagination" connection.

Decending pathways are extremely important for PCT (HCT) and for the networks that I like to use, Adaptive Resonance Nets. For both frameworks one would expect about a 1:1 correspondence between ascending and descending pathways, and this appears to be roughly about the case.

One notable exception is the visual area 1 to LGN ration which is 10:1 (that's descending to ascending -- some people think its up to 20:1). This corresponds very well with a design I (in
collaboration) have put together for visual object recognition with object size invariance.

> Also, while he mentioned that even the lateral geniculate
> bodies (the first "stop" for visual signals after the retina) is
> getting information from the visual cortex, I forgot to ask him if
> the retinal cells themselves received signals from further upstream.
> Does anybody know about this? Perhaps Joe Lubin will be able to help
> me to understand this.--Gary

I would be very interested to hear his answer. For a long time I "knew" that there was no evidence of efferent connections to the primate retina, although there is evidence in "lower" animals. Recently I read something (I forget where) that questioned the absense in primate (and I think presented some data). I don't know why I didn't take careful note of this info. If anyone has a reference I'd appreciate it.

Within the retina there are connections (interplexiform cells) from the inner to the outer (photoreceptors) synaptic layers. These may aid in light adaptation either in the photoreceptors, or more likely, in the retinal network.

-- Joseph Lubin
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Date: Fri Oct 25, 1991 9:28 am PST
From: Gary A. Cziko
ubject: Single-Neuron Comparator [from Gary Cziko 911025b]
Earlier this morning I asked:

>If the cell is getting both sorts of information, doesn't it mean that the
>comparator function must be handled by single cells? Is this possible?

I suppose I should have read Bill Powers's [911025.0745] long post more carefully before asking--somehow he anticipated my question (feedforward?).

>Subtraction is easily defined: when an inhibitory signal and an
>excitatory signal converge on a single neuron, the output rate of firing
>is proportional to the excess of excitation over inhibition. In other
>words, a single neuron can be a comparator.

That'll teach me to read before I post.
==
Here's another demo on "apparent social control" which I used to introduce Clark McPhail's talk to my department last week.

I ask the audience to close one eye and reach out with one hand with finger extended to "touch" my hand. I then move my hand around and they all follow quite nicely. It almost looks as if my hand is connected to all of theirs (like puppets on a string). I then ask them to raise their hands as high as they can. Then for contrast I ask them to shout "Go Illini" as loudly as they can at the count of three (the Illini is the name given to our football/basketball teams). I then count to three and . . . total silence.

So they will do as I ask only if NOT doing so would create an error signal (they want to be cooperatively and audiences are certainly used to moving their hands around). But shouting a loud as you can in a classroom would create more of an error signal (or I suppose I should say an error at a higher level) than the error signal created by not following my shouting request.--Gary

P.S. I wonder what a roomfull of Republicans would do if asked to shout "long live George Bush!"?

Gary A. Cziko

Date: Fri Oct 25, 1991 11:38 am PST
From: Martin Taylor
Subject: Re: Objections etc

[Martin Taylor 911025 14:25] (Bill Powers 911025.0745)

I want to congratulate Bill on his long response to my comments. I don't think I want to disagree with any of it. Much.

>As to the "ordinariness" of control theory (which should be our ultimate
>goal, of course), I think we would do better in this argument by
>considering some real contexts in which control theory, in your opinion,
>is so self-evident as to be trivial, and equally important, some others
>in which accurate "open loop" control can be seen or a non-control
>explanation is to be preferred.

My error in wording: I used the term "trivially true" which to me has no relationship with "so self-evident as to be trivial." I meant that when it is pointed out, no long analysis is needed to see its truth. Most insights of genius have this characteristic, and most such insights are far from self-evident before they are pointed out.

>>>An open-loop action by definition has no feedback effects.
>
>>Can we add "at the level of the intention to perform it"? All actions
>>have feedback effects, whether intended or not, whether controlled or
>>not, whether executed by a living organism or not.
>
>Strictly speaking, no. Feedback isn't the effect of output on input. It's
>the effect of a variable on itself, via a closed loop.

Yes, that's exactly my view, provided we add "at a later time". Can we now add "at the level of the intention to perform it"?
>
>The question is not that of imitating the behavior of a control system.
>It's understanding how real living control systems are actually
>constructed.
>
>The mistake of behaviorism was to convert perfectly plain control
>processes carried out by organisms into "attractors" scattered around the
>environment. This led to some very strange ways of seeing the physical

>world. Stimulus objects and events took on "value" or "reinforcing"
>properties or "discriminating" capabilities. Ordinary physical things
>suddenly acquired the ability to "control" behavior. They could even
>"generalize." Behaviorism -- and practically every other life science,
>for that matter -- searched for the aspects of the environment that MAKE
>organisms behave as they do. The responsibility for goal-seeking behavior
>was put into the outward correlate of the inner goal instead of into the
>seeker.
>
>This is my main objection to using the concept of an "attractor" with
>respect to behavior. It encourages the same mistake that led to behaviorism.
>

Now I understand your objection to talking about attractor systems. It is of the same kind as the objections people have had about other concepts that have technical and popular meanings. They get misused, and mislead people. I don't think I misuse the term "attractor" in this way. Perhaps I do, because one can never be quite sure one is correct in such matters. But I think not.

Maybe if I re-introduce the evolutionary attractor concept again some time later, I will make an effort to dissociate it from the kind of thing to which you are objecting. Because I strongly believe that control systems have to have evolved out of attractor systems, and can be regarded as a subtype of attractor system. I don't want to pursue that in this post, and I'd rather it didn't generate a thread, because it is tangential to the objectives of this group (better on cybsys-l).

>As to chaotic attractors, I am highly skeptical about their playing any
>part at all in normal behavior. Most strange attractors behave like
>perfectly useful physical systems (oscillators, for instance) that have
>been driven beyond their normal ranges of operation. They wouldn't be
>much good for stabilizing the experienced world against disturbances.

Well, from the outside, I might agree, but I can't, since a few years ago we (independently of Freeman and company) had come to the conclusion that any sensitive perceiving system would, as a side-effect of its sensitivity, necessarily exhibit chaotic behaviour. Again, it isn't something that can readily be pursued here, but suffice it to say that when data is coming in, the chaotic behaviour vanishes.

>Opening the loop ALWAYS drastically reduces the quality of control in a
>real world where disturbances can happen and in which local properties
>are subject to little unpredictable drifts. But we will take what control
>we can get.

>>>In order to come even close to producing an intended effect, such an
>>>action has to be protected against disturbances and executed with
>>>effectors whose calibration remains exactly what it needs to be.
>
>>Those are the conditions I was postulating.
>
>All right, but I would humbly request that we stop calling this sort of
>action "control." We need to reserve control as a technical term.

Yes indeed. As I said to Rick in the same note, "control" without feedback is a contradiction in terms. So opening the loop does not reduce the quality of control, so much as reduce the accuracy of the intended effect because of the absence of control.

>Model-based control works by applying an "efference copy" of your outputs
>to the input of the model, and controlling, in imagination, for what the
>model does rather than for what the external world does. This is
>sometimes necessary and is often useful, especially when the model yields

>up in real time a predicted future state of the world. I haven't put any
>such complications into my model, but some day they will be there because
>we often do things this way.
>

I observed a good case in point today, playing badminton. I think this is normally a highly controlled activity, but I think also that the final stage of muscular effort becomes open-loop, at least when one is trained. Why? Because when one's muscles become tired, shots go astray. The "efference copy" ceases to become a true copy. It isn't that the muscles are incapable of doing what they are asked to do, but that they are asked to contract with a certain strength, rather than to contract in a controlled way to construct the required movement. When we discussed skilled movement a few months ago, the general claim was that the control was very strong at the lowest levels. I now doubt that, because if it were so, the tiredness of the muscles should not affect the movement precision, provided that the requisite muscular strength were still there.

Anyway, I'd like to recommend to all CSG-ers that they archive Bill's long post, if they save nothing else from this list. It is full of clear statements of important principles.

Martin Taylor

Date: Fri Oct 25, 1991 3:20 pm PST
From: marken
Subject: Re: where's the opposition

[From Rick Marken (911025)]

In response to my claim that control theory is a theory of intentional behavior, Martin Taylor (911023) says:

>OK. That does two things. Firstly it makes the whole argument circular,
>and secondly it omits a lot of what people actually (observably) do.

> If you make that
>restriction, then the discussion comes down to the definition of intentional
>behaviour--and I can almost hear you say "why, it's almost all behaviour,
>and all behaviour that psychologists study."

OK, I see your point. Let me try it this way:

After studying PCT for some time I made a striking realization: Control theory explains the phenomenon of control. This realization helped me understand why psychologists ignore PCT, by and large. Most psychologists are not really familiar with the phenomenon of control as it is exhibited in the behavior of organisms. They study behavior, but not control.

I now think that PCT is built on two very important insights: 1) that control systems control perception and

2) that the purposeful behavior of organisms IS control.

I think insight #2 may be far more important (and even less well understood) than insight #1. It was an insight that Bill Powers was in a unique position to be able to make given his training in physics and engineering. Bill noticed that consistent behavioral "outputs", like lifting a suitcase or speaking a word, are produced by variable means. He also noticed (and this was the subtle part) that the variability in the means was REQUIRED to counter the effects of varying disturbances (which are often invisible to the observer). Consistent results, the results we call "behaviors", are thus controlled results. Bill then pointed out that most of what we consider "behavior" has this characteristic -- it is a consistent result produced by variable means in a disturbance prone environment. These consistent results are CONTROLLED results. So, much of what we call behavior (of living organisms) is controlled results of action. I would argue that this is the main, factual claim of PCT -- namely, that most of what we call behavior is a controlled result (or related to a controlled result) of action. This is a substantial claim -- and possibly one with which you disagree. The claim is that what psychogists think of as "behavior" is control.

This is a problem for PCT because it is trying to explain a different phenomenon than the one that psychologists are ostensibly studying. I think the psychologists ARE studying control and won't admit it. But there is also the possibility (which you hint at) that much of what psychologists are interested in is behavior that is NOT control. So I think my debate with "conventional" psychology is not theoretical but factual -- I claim that almost everything psychologists study IS control. If I am right about this factual claim then the theoretical claim (that this phenomenon is best handled by PCT) is almost, as you say, tautological.

So my BIG claim is a factual one, not a theoretical one. If a substantial amount of what people do is NOT control then, indeed, PCT is just one of many equally valid insights into the nature of human behavior. But I think that almost everything we call behavior -- speaking, driving, loving, walking, lifting, expounding, worshiping, tracking, timimg, eye moving, sweating, hair combing, you name it -- is control. The test for the controlled variable is not just a way of studying PCT; it is also a way of testing whether you are even dealing with the phenomenon of control.

I heartily agree that if you can show that a particular behavior is NOT a controlled result (via the test) then PCT is not applicable and some other theory (like the ones currently used in psychology) might be applicable.
Regards

Rick

Date: Sat Oct 26, 1991 9:26 am PST
From: POWERS DENISON C
Subject: developmnt,objections,chaos,hi joe

[From Bill Powers (911026.1100)]

Bruce Nevin (911025) --

>... acquisition of a skill associated with a particular developmental
>stage itself goes through stages corresponding to the development of
>hierarchically-linked control systems involved in practicing that skill.
>Why it should always be the same linear order is not obvious to me.

In developing the definitions of levels, I looked for classes that had the kind of relationship that hierarchical control demands:

1. Higher-level perceptions are functions of lower-level perceptions.

2. Control of higher-level perceptions requires varying lower-level perceptions.

As a result, higher-level perceptions can't exist until lower-level perceptual signals have attained some sort of coherent behavior and are at least somewhat controllable. You can't learn how to control the sequence of a set of categories until you have signals representing the categories, and can create them in perception at will. You can't perceive/construct programs until you can perceive and control sequences of categories. And so on.

So the ordering HAS to proceed from the bottom up. This doesn't mean that development always goes from bottom to top. After you have acquired a new level, you may get into situations that require lower levels to be revised or expanded -- and then you have to build upward again from the modified lower levels. Back and forth. But generally the trend is from lower to higher. The initial process must ALWAYS be from lower to higher when you're building a new level, because the raw material has to be there.

This is one reason I'm so stubborn about saying that language is not the explanation of brain organization. You can learn words that (to adults) sound like programs, but they don't mean programs until you can perceive programs -- nonverbally.

First you learn "this" is a [this], "that" is a [that], and "the other" is [the other] (quoted terms mean the verbal symbol for the category, bracketed terms mean the nonverbal perception of the category). Then you learn to go "This", "That", or "That", "The other" to mean [this, that] or [that, the other] -- sequences. Then you find out that you can say "If this is followed by that, then that is followed by the other", meaning [(this, that) -> (that, the other)], or equivalently { not [(this,that) and not (that,the other)]}. Of course the exact logical expression depends on what you learned; lots of people aren't too good at telling the difference between if and iff (if-and-only-if).

Random thought: to kids at some stage, all people are known by the categories to which they belong: the Stone Cobras or the Snuff Saints, for example. The categories are defined by intensities, transitions, sensations, configurations, events, and relationships. There are no true individuals until the system concept level is reached, if it ever is. So when you finally realize that one Stone Cobra is not equivalent to another, categories lost their overriding significance. Ed Ford has already figured this out and teaches it to his clients.
Gary Cziko (911025) --

(Re: David Hubel) --

>I asked him about the presence of descending pathways (i.e., from the
>higher, more central visual areas of the brain to the more peripheral).
>He replied that they seem to be everwhere although no one seems to know
>what they do.

Are these pathways from perceptual function to perceptual function? Go back and ask him. If so, this is great support for Joe Lubin.

Joe Lubin (911025) --

Long silence -- you busy getting a degree or something? Welcome back.
I think I'm the guilty party -- I told you at the meeting that I'd read that some 25% of the paths in the optic nerve carry efferent signals. I just tried to find where I got that. Partial success. In my old (1947) edition of Ranson and Clark *Anatomy of the nervous system*, p. 223, we find

"The nerve also contains some efferent fibers which terminate in the retina (Arey, 1916)."

They drew a line showing an efferent nerve in a Figure on the same page.
The reference is

Arey, L. B., 1916: The functions of the efferent fibers of fishes, Jour. Comp. Neurol. _xxvi_, 213.

So it looks as though Ranson and Clark put fish connections in their diagram of the human optic nerve. I hadn't noticed that.

But this isn't the reference where the "25%" came from. Sorry -- I'll keep an eye open for it, afferently.

Gary Cziko (911025b)--

>I suppose I should have read Bill Powers's [911025.0745] long post more
>carefully before asking- [about neurons as comparators].

See also BCP, p. 28 and 84. Time to read the book again? Most people who read it again after a few years say "Gee, when did you put all that new stuff in there?"

I love the social-control demo. We need a version of it for the portable demonstration collection. How about giving a pin to someone, and giving him instructions (are you listening, Chuck?) to push it all the way into a chair cushion, a rug, and the palm of his or her hand?
Martin Taylor (911026) --

To amplify on a P.S. to my long post --

If I sometimes seem to disbelieve that people are agreeing with me, it's because I saw some implications in the way they expressed agreement that I disagreed with. That doesn't mean the disagreements were really there, only that I wanted to make sure they weren't. As I have said a boring number of times, it's much harder to reach agreement than to disagree. I'm more skeptical about agreement than about disagreement. The problem is that I have only one agenda, which is always out there on the table. But people who come into control theory from other fields bring along other theoretical frameworks, so neither my words nor theirs necessarily mean to them what they mean to me. It never hurts to check. But I don't mean to imply that others are dense. Far from it. In this forum I often feel like Johnny-One-Note in a battle of the bands.

>A week or so ago, when I got back from Europe, I sent you some e-mail
>contining something I though would be of interest to you. Right now,
>I can't for the life of me remember what it was.

Neither can I, but I'll look. It's terrible that both of us forgot something so memorable. I beg pardon for including this response to a private note on the net -- it's clumsy and somewhat expensive to send direct mail (I have to hang up and redial). Some day I'll fix those scripts.

>>Strictly speaking, no. Feedback isn't the effect of output on input.
>>It's the effect of a variable on itself, via a closed loop.

>Yes, that's exactly my view, provided we add "at a later time".
>Can we now add "at the level of the intention to perform it"?

Not yet, but I'm weakening. "At a later time" is ambiguous. How much later? This matters when you're talking about input that involves time-integrations, averaging, or any extended processing. The feedback in such cases occurs *while the perception is in process of developing* and modifies it before the computation is finished. I think this is true of essentially all perceptions, even at the first level. Even the tendon signal, as a frequency, is scarcely defined before the first change in muscle tension arrives back at the receptor to change the tension being sensed. The result is that the muscle never gets to the tension implied by the first few blips of the tension signal. At higher levels, the neural transit time you would get from just computing the path lengths and dividing by the speed of transmission and adding synaptic delays is far shorter than the "reaction time." Output can begin affecting input while the input is just starting to change. And of course

the output from just a moment ago is always affecting input. (I'm beginning to see that the informal use of "feedback" can be useful. Is the price right?).
I'm trying to guard against the common misconception (if present in anyone listening to this) of control loops in which each function takes its turn acting, the next one doing nothing until the previous one is finished. It's easy to forget that all the functions are always receiving input and producing output, even if delays are involved. Even at cognitive levels, the perceptual processes go on working while the comparison and output processes are developing their outputs. If you're in the middle of speaking a sentence and you see an eyebrow cock after the first word, you may well finish that sentence differently from the way you originally intended, or even interrupt yourself and complain or ask what's wrong. Or you may hear a fragment of the sentence you're producing and perceive on the fly (while in progress, not on the insect) that it carries a wrong meaning, and edit as you go. You can see how this screws up a cause-effect picture of how the system works, even though each individual function can be characterized causally (until Joe Lubin finishes his model of perception).

>Maybe if I re-introduce the evolutionary attractor concept again some
>time later, I will make an effort to dissociate it from the kind of
>thing to which you are objecting. Because I strongly believe that
>control systems have to have evolved out of attractor systems, and can
>be regarded as a subtype of attractor system.

But (you're going to get tired of hearing me say that) control systems ARE attractor systems, aren't they? Or better, attractor systems are really negative feedback systems, aren't they? At least some of them? And is there really any "attractor" system that really really works by "attraction?" I'm a very physical guy and I don't like to see pushing analyzed as pulling. I know what you mean by attractor, of course, but it's a metaphor, not a literal analysis of how the thing works. I have a creepy feeling about using that word, but talk some more.
>>As to chaotic attractors, I am highly skeptical ...

>Well, from the outside, I might agree, but I can't, since a few years
>ago we (independently of Freeman and company) had come to the conclusion
>that any sensitive perceiving system would, as a side-effect of its
>sensitivity, necessarily exhibit chaotic behaviour. Again, it isn't
>something that can readily be pursued here, but suffice it to say that
>when data is coming in, the chaotic behaviour vanishes.

Is it the nervous system that gets chaotic, or the model? Freeman says the same thing as you do, and in his case it's because his model (of the olfactory bulb) consists of interlinked second-order nonlinear differential equations that oscillate. Freeman's stuff doesn't read so well on the third or fourth reading. He pushes pretty hard and makes some pretty broad -- if disguised -- assumptions. Well, I think I'll sit this one out until this line of work gets farther along. All I have to go on is a funny feeling that the underlying conception of perception has something wrong with it.

What with you and Joe Lubin and Freeman all working on perception, however, I don't want to be in the position of resisting progress. Do you (or Joe) think that my original naive model of perception is about ready for junking? It has to happen some day -- the idea of separate input functions each monitoring a single one-dimensional variable was an artefact from the start. All I ask is that the phenomena be preserved.

I do wonder how your statement applies to spinal reflex loops, which certainly involve "sensitive perceptual systems." And that makes me wonder about other perceptual systems which almost always have "data coming in". When is data NOT coming in? Isn't "zero signal" data? In any continuous-variable model, zero has to have just as much meaning as non-zero, especially when control is involved and it happens that behavior holds a perception near its zero point. If the system went chaotic near that point, what would happen to control? And is that what actually happens?

On the other hand, I am intrigued by the possibilities with regard to reorganization, especially reorganization of perception, and most especially with regard to the acquisition of new input functions at new levels during development. I can't account for it -- I just take it for granted. If a chaos-based model could explain how organisms discover invariants and learn to control them, I would forgive all. Also, today's posts which seem to indicate the reality of connections going both ways between sensory systems at different levels removes some of my underlying objections. I didn't know such connections existed. When I was putting the model together, I studied neuroanatomy as much as I could stand, and tried to find a role for all the general sorts of connections that were known then. That's where the imagination connection came from. There were internal connections from motor nuclei to sensory nuclei, so I had to figure out what they would be for. Now there's something new to consider, and I don't know what these connections are for. I never did figure out what those visual efferents are for, even in fish. I'm happy to leave that to any investigators who understand control theory (but not to those who don't -- there's too much room for misinterpretation when you leave out closed loops).

Badminton:

>...when one's muscles become tired, shots go astray. The
>"efference copy" ceases to become a true copy.

Nice.

>When we discussed skilled movement a few months ago, the general claim
>was that the control was very strong at the lowest levels. I now doubt
>that, because if it were so, the tiredness of the muscles should not
>affect the movement precision, provided that the requisite muscular
>strength were still there.

As I discovered with the eye model, and later with the Little Man dynamic model, strong feedback and tight control don't necessarily produce the effect you expected. The reason isn't that strong feedback doesn't produce tight control, but that the system isn't controlling what you thought it was. In the arm model, the tightest control isn't on position, but on acceleration. The next loop, the dynamic stretch loop, controls velocity tightly, but not position. The combination of stretch and force feedback results in very low gain for position control. You have to get all the way to the third level (or fourth?) before position control is much good, and even then you have to add the visual loop to make it really tight. Maybe this is where model-based control should be introduced.

Touch receptors send signals directly to spinal motor neurons. They complete control systems for touch control, which is a very tight control system. But fine touch control is relevant only when position isn't controlled: you can't have both at once. Similarly for force control (effort): here the loop involves only the tendon receptors, motion being prevented by whatever the force is being exerted against. Now acceleration control becomes force control (same sensors) and the loop gain is very high for moderate forces. If you control kinesthetically for force, the result is very different from when you control kinesthetically for position. The difference is in how higher systems are using these lower ones. Its a very versatile system, quite beautiful.

Rick Marken (911025) --

>So I think my debate with "conventional" psychology is not theoretical
>but factual -- I claim that almost everything psychologists study IS control.

That's the crux of it. I think, though, that theory is still in the background, because you can't recognize control unless you know how control works. The SR viewpoint is based on recognizing a different model at work -- see, the input is causing the output. Even before we claim that control is a phenomenon, we have to present the phenomena unlabelled.

Show a variable behavior having a consistent effect, and a constant stimulus suddenly having a different effect. Then present TWO theoretical explanations and see which works. This will possibly have the effect of revealing the SR viewpoint *as a theory*, and show that this concept of causality is NOT a fundamental tenet of science itself.

I don't think that behaviorists (or others) realize the extent to which even their raw observations are guided by a theory of cause and effect. It is damned hard to see a fact as a theory if you've never had an alternative interpretation of the same experience. It can be mortifying to realize that you had been arguing as if you knew exactly what was going on, only to realize later that there was a different and perhaps even more plausible explanation. It's like discovering that your father wasn't actually beating your mother up during those private Sunday-afternoon "naps." It is to die. You don't even have to believe the alternate explanation -- just changing the status of your conviction to that of a subjective belief is shocking enough, particularly for an "objective" scientist.
------------------Snow on the mountains yesterday. Sunshiny and cool today. Good company on the net. Who could ask for anything more?

Best to all,   Bill P.

Date: Sat Oct 26, 1991 12:23 pm PST
From: URROBERT
Subject: transfer of training and open loops

[From Dick Robertson]   Bruce Nevin (911023 1239)] replying to my note:

>>the researcher found that 10-11 year olds don't consider
>>either one as a unique person, "they are both seen as static categories rather
>>as individuals." The adolescent subjects "did tell stories with a past,
>>present and future, but they led to rigid, fixed outcomes." And the young
>>adults brought in concepts like, "it depends upon what each would want for
>>themselves...."
>>control of category variables is achieved first, then sequence variables and
>>then programs."

says:

>Still working to understand CT dept: I take it this means that the
>application of these levels of perception to the projection of stories
>about imagined social relationships follows the same order as their
>acquisition in other areas of experience. Presumably, control of
>category, sequence, and program levels had been learned much earlier
>i.e. for tying one's shoes, wheedling an advance on allowance, speaking
>English . . .

>This suggests a much more complex model of the developmental process
>than is usually considered, where acquisition of a skill associated with
>a particular developmental stage itself goes through stages
>corresponding to the development of hierarchically-linked control
>systems involved in practicing that skill. Why it should always be the
>same linear order is not obvious to me. Attempts to learn the program
>for tying one's shoes may foster learning certain relationship
>perceptions and sequence perceptions, so that the program is clear but
>the means for enacting it are not yet available. Relearning after a
>stroke is surely like this. What's the scoop?

It's a good question, I wish I knew the answer, I can only offer a couple of speculations. It seems to me that there is both a generic aspect and a specific aspect to the application of new levels of perceptual control as they get organized.

I taught myself to type by the Gregg method many years before I tried to learn t play tennis. As I recall my typing experience I first learned the separate configurations of letters (keys), then relations between hand configurations and keyboard configurations, then sequences of finger punches for words and then finally I could concentrate most of my attention in the communication program. Later, in my experience of starting to learn tennis I seemed to observe myself going through a comparable "developmental sequence" (allowing appropriately for task differences). I first concentrated on positioning my body for various strokes, then ball-body trajectory relationships, then internal-command types of category controls ("I must run back-, forward-," etc. Not that I would always he myself saying these words in my head, but I felt them that way, and sometimes di say them internally). Then, finally on strategy (read Programs). I couldn't directly transfer the existing systems at each level to the new task, although I grew more acceptant of the necessity of mastery in a stepwise fashion as I re-experienced the process more often.

Likewise, I think I have noticed kids learn new skills by transferring generic control abilities to new tasks, but still have to develop the applications hierarchically. I seem to recall reading about young people in "primitive cultures" (who hadn't grown up with tricycles and then bicycles) having more trouble learning to steer cars than do western youth. Yet we know that western youth don't automatically do it perfectly just by transferring the bike program. I think this problem is what Piaget was referring to when he postulated the alternation of assimilation and accomodation in his scheme, and probably account for the fact that some learning psychologists racked up mounds of studies showin that there isn't much "transfer of training" while others produced all kinds of evidence that there is. If some kind of "local" reorganization goes on in the learning of new skills, it doesn't seem surprising to me that it would go throug stages resembling Powers' generic hierarchy--the logic of circuitry reorganizati could well be pretty uniform throughout the system. This is starting to get out my depth--I don't think it has been explored very far--I hope we can hear from t hard wiring guys about it.
Bill Powers's comment about the increasing scope of potential commands upon low order systems as higher orders develop seems relevant here.

(Martin Taylor) says:

>>Model-based control works by applying an "efference copy" of your outputs
>>to the input of the model, and controlling, in imagination, for what the
>>model does rather than for what the external world does. This is
>>sometimes necessary and is often useful, especially when the model yields
>>up in real time a predicted future state of the world.

>I observed a good case in point today, playing badminton. I think this
>is normally a highly controlled activity, but I think also that the final
>stage of muscular effort becomes open-loop, at least when one is trained.
>Why? Because when one's muscles become tired, shots go astray. The
>"efference copy" ceases to become a true copy. It isn't that the
>muscles are incapable of doing what they are asked to do,

If the muscles are tired, it seems to me that they ARE incapable of doing what t are asked to do.
Don't you mean that the control system (for the activity in question) is still capable (i.e. that it is still sending the proper reference signals) but if the muscles are fatigued, the loop gain has dropped to the point where the correctio of some of the lower orders are falling behind the changing RS that are coming i I'm nor sure whether that really conforms to the definition of open-loop or is merely increasingly poorer control.

Happy hunting
Dick Robertson

Date: Sat Oct 26, 1991 1:06 pm PST

From: Heidi Sveistrup
Subject: Re: Development

I'd like to understand the developmental progression of control of category variables followed by sequence variables and finally by programs.

(Dick Robertson Thu, 24 Oct 1991 16:19:57 CDT)--

>the researcher found that 10-11 year olds don't consider
>either one as a unique person, "they are both seen as static categories rather
>as individuals." The adolescent subjects "did tell stories with a past,
>present and future, but they led to rigid, fixed outcomes." And the young
>adults brought in concepts like, "it depends upon what each would want for
>themselves...." I was tempted to jump up and shout, "But of course,
>control of category variables is achieved first, then sequence variables and
>then programs."

(Bruce Nevin 25 Oct, 1991)

>Still working to understand CT dept: I take it this means that the
>application of these levels of perception to the projection of stories
>about imagined social relationships follows the same order as their
>acquisition in other areas of experience. Presumably, control of
>category, sequence, and program levels had been learned much earlier

>i.e. for tying one's shoes, wheedling an advance on allowance, speaking
>English . . .
>This suggests a much more complex model of the developmental process
>than is usually considered, where acquisition of a skill associated with
>a particular developmental stage itself goes through stages
>corresponding to the development of hierarchically-linked control
>systems involved in practicing that skill. Why it should always be the
>same linear order is not obvious to me. Attempts to learn the program
>for tying one's shoes may foster learning certain relationship
>perceptions and sequence perceptions, so that the program is clear but
>the means for enacting it are not yet available. Relearning after a
>stroke is surely like this.

To set the stage for my question - I am interested in the control of posture. One of the techniques I am using to understand postural control is to identify how people regain a stable posture after they have been exposed to an externally induced disturbance. One technique is to have a subject stand on a small platform that can be translated in the forward or backward direction. This work has been done by a number of different people (most notably, Lew Nashner) who identified some fairly stereotypical responses to the platform perturbation. If the platform moves backward thereby pulling the subjects feet out from under her, she will respond by activating the muscles on the back of her leg and trunk (gastrocnemius, hamstrings, and trunk extensors) in a distal-to-proximal order starting with the most distal muscle at 80-100ms after the platform movement with the more proximal muscles following at ~20 ms intervals. If the platform moves in the opposite direction (pulling the feet forward with respect to the center of gravity - similar to what happens when you're standing on a bus that accelerates unexpectedly) the muscles on the front of the leg respond. The response is again a distal-to-proximal activation of the tibialis anterior, quadriceps and abdominals starting at 80-100ms with 20ms delays for the more proximal muscles.

My particular interest is in the acquisition of postural control observed in infants as they make the transition through a series of behaviors. The infants in my study are first tested when they are able to stand while holding onto furniture and they continue to come into the lab until they are able to stand without support (independently standing) and finally independently walking. I would like to address the question of the progression I have seen in the EMG responses. In the earliest stage, the infants activate the most distal muscle in response to the movement of the platform but I have not observed a full distal-to-proximal activation pattern similar to the adult pattern in any of the youngest infants. As the infants progress through stages, the "appropriate" muscle responses are observed with greater frequency and by the time the infants are walking independently, the three muscle sequence is elicited every trial.

I have two general questions:

1. Does the control of individual muscles, followed by control of groups of muscles, followed by elaboration of the groups of muscles equate to the the statement that "the control of category variables is achieved first, then sequence variables and then programs."

In other words, in postural control, can individual muscles be equated with category variables, muscle synergies (ie. three muscle sequence) be equated with sequence variables and the elaboration of synergies (or the incorporation of synergies into more complex movements) be equated with control of programs?

2. From my limited understanding of PCT, the acquisition of a new skill (ie. standing independently versus holding onto a coffee table) is dependent on the development of a reference value (at least for skills that are acquired over ontogenetic time). How would a "new reference value" for standing be formed? or is the "reference value" for vertical independent stance encoded somewhere?

Heidi Sveistrup
HeidiSv@oregon.uoregon.edu

Date: Sat Oct 26, 1991 7:29 pm PST
From: Gary A. Cziko
Subject: Malpeli on visual feedback

[from Gary Cziko 911026]

I sent a copy of my post on Hubel and the visual system to Joe Malpeli <j-malpeli@uiuc.edu> who does work on the visual system on my campus. Here is his response:

>In birds, the retina does indeed receive feedback from the brain, but
>apparently not in mammals. What is known about feedback circuits is that they are very
>widespread and mirror the "feedforward" circuits. Very little is known about
>their function, however. First of all, it seemed reasonable to most
>physiologists to study the connection that they thought were likely to be
>responsible for the main information transmission routes, leaving the more
>subtle effects for a later date. Also, the feedback circuits are unlikely
>to be understood (in terms of their real function in perception), in the
>paralyzed, anesthetized animal. For example, many studies have been done on
>the feedback from cortex to the LGN. Some have found inhibitory effects, some
>excitatory, some both. However, none tell us when these pathways are activated
>with regard to perceptual, attentional or oculomotor state - because none of
>the dynamic regulation of the system that is likely to occur in the normal
>animal is intact in the anesthetized animal. It is now possible to record
>single cells in the brains of normal, awake animals trained in perceptual or
>oculomotor tasks while they perform the tasks. It is very difficult in these
>preparations, however, to isolate the effect of feedback circuits.
>
>One of the main issues is simply the relative "power" of the feedforward and

>feedback circuits. I don't have time to go through the whole story now, but
>basically, most people have thought that information about the visual image
>was carried by the feedforward circuits, whereas the feedback circuits had a
>more subtle role such as gain control or perhaps directing attention. In
>physiological terms, it has been proposed that only the feedforward circuits
>can actively drive cortex. However, recent findings in my laboratory contradict
>this notion, demonstrating that feedback circuits can exert a very powerful
>influence on cortex. See Mignard and Malpeli, Paths of information flow
>through visual cortex, Science vol 251, pp. 1249-1251, 1991.

I also sent him a copy of Joe Lubin's response with another specific query on the retina.
Malpeli's response:

>I don't have much to add. There are the "internal" feedback circuits in the
>retina that he mentions. The question of feedback from the brain in mammals
>has been one that has interested people for a long time and some have claimed
>that they exist, but it has been difficult to prove. I don't know the
>references. You should be aware that the retina is considered an extension
>of the brain rather than part of the peripheral nervous system. Since for
>other sensory systems there is feedback to very early stages, it is perhaps
>surprising that it is difficult to show feedback from the brain to the retina -
>but that's the way it stands.
====

Bill, when Malpeli says

>For example, many studies have been done on the feedback from cortex to the LGN.
>Some have found inhibitory effects, some excitatory, some both.

could this be the workings of a "double-sided" reference signal?-

-Gary

Date: Sat Oct 26, 1991 10:17 pm PST
From: POWERS DENISON C
Subject: Chaos and perception

[From Bill Powers (911026b)]

Martin Taylor (911025) --

Chaotic thoughts are keeping me awake tonight. Out of this jumble came, finally, something coherent: a question.

What is it that a model of perception has to explain?

I have always assumed, since I started this line of work, that it has to explain not so much the brain's activities as the world we experience, of which "the brain" is a subset. It has to explain the feel of these keys as I type, the forms of the letters on the screen, the shape and size of the blackboard on the wall, the shadows from the light burning overhead, the chair and the rug and the furniture. It has to explain how I can see a chair and also experience "furniture." It has to explain the very thoughts I am experiencing now, behind these typing movements. It has to explain the experience of imagining an entity named Martin Taylor, whom I have never seen or spoken with.

It must also explain why this world presents itself to me in three dimensions, stereo sound, and living color, chock full from edge to edge of continuously-present smoothly changing noise-free colors, shades, objects, motions, relationships, and operations in progress. Whatever properties the perceptual model has, however we imagine perception to be implemented in the brain, the model must provide something that can be experienced exactly as this world we live in, the bodies we live in, and the minds we think and imagine with are experienced.

That's what I have against models that contain chaotic processes. My objection has nothing to do with the level of abstraction, the use of mathematics (or my meager capacities in that regard), or metaphors. My problem is that I can't see the connection between the way chaotic systems behave, from what little I know of them, and the world of experience.

My model of perception is a continuous analog model because continuous analog signals seem to me to have the right kinds of properties to be the things I experience. Even though I can't describe the functions that produce these signals, I can describe, or so I have assumed, what these functions produce: this, the world. I think that any model of perception has to fit this world. Even a model based on chaos must at some point produce a smooth continuous analog world.

Most models of perception take the world of experience for granted and put it outside of us; this means that the brain, too, is outside of us, so all we have to do is postulate some kind of brain activity that is excited by energies from the world impinging on the sensory nerves connected to that gray blob over there. We can be disembodied privileged observers who can see what goes on inside the brain and also what goes on outside it. We can propose models of brain activity that only have to depend coherently on objective events, those events we choose to select as "salient" aspects of an otherwise unconsidered (but still perceivable) background. If we can select what is to be represented and if the world to be represented is outside, then it doesn't matter whether the model of the brain's internal processes has the properties of the world. The world can be solid, clear, continuous, detailed, and smoothly variable, while the model's operation can be murky, chunky, distributed, statistical, intermittent, discrete, or chaotic.

If, on the other hand, we assume that the world we experience is itself the output of our perceptual functions, and not the reality that excites our senses, then we can no longer let the models behave differently from the world. The models must provide something to experience that is exactly, in every detail to the limit of sensory resolution and in all the sensory modalities, the world we do in fact experience.

To me, this is the first and last question about any proposed model of perception: does this model behave as the world I experience behaves? If it doesn't, then it must be wrong. Even if the model is mathematically beautiful and the principles behind it general and satisfying, if the model doesn't behave like the world then it describes an imaginary system, not the one that actually exists.

So that is my answer to the question. What is yours?

Best

Bill P.

Date: Sun Oct 27, 1991 4:07 pm PST
From: POWERS DENISON C
Subject: Disturbed movement; push-pull signals

[From Bill Powers(911027.1430)]

Heidi Sviestrup (911026) --

I've seen your name on the lists, Heidi -- good to hear from you at last.

Probably the first thing to do in applying control theory to the "platform" experiment is to translate from stimulus-response to control theory terms. The disturbance is clear, and so is the action, but what are the controlled variables? What does the disturbance affect that each action affects in the opposite direction?

When the platform slides backward, a couple is applied to the whole body tending to pitch it forward, face-down. Generally, the first variable of posture to be disturbed would be the angle at the knee joint, then the hips and finally the spinal cord and neck. So the "stereotyped sequence" may simply be a matter of the way the disturbance propagates up the body. If you pushed forward between the shoulder-blades instead, the disturbance would still tend to pitch the body forward, but now you would expect the upper systems to resist first -- the ones most directly affected by the disturbance. The anterior neck muscles that keep the head from snapping backward would probably tighten first.

To pin down the controlled variables, you have to find variables that would be changed by the disturbance if the body did not change its action, and that are prevented from changing by the actions that do take place. The most obvious guess won't necessarily be the right one.

Consider, for example, the effect of the backward slide of the platform on the angle at the knee joint. The inertia of the upper body and upper leg will tend to keep them stationary in the first instant, so the backward slide first tends to move the feet backward with the upper leg still stationary. In other words, the knee joint tends to bend. If this angle were under control, you would expect an effort to extend the leg at the knee. This, however, would tend to pull the foot forward, stiffening the leg against the pull of the platform; the reaction force would increase the pitch of the upper body forward. So control of knee-joint angle -- resistance of the disturbance of knee-joint angle -- would have the wrong effect. The action wanted is compliance with the movement, which will minimize the pitching disturbance applied to the upper body.

On the other hand, you might find a two-phase reaction similar to the "portable demonstration" described in my 1960 paper with Clark and MacFarland --reprinted in _Living Control Systems_. The initial disturbance at the knee joint would be resisted because the joint-angle control system operates faster than higher-level systems (and anyway the disturbance arrives there first). Then the disturbance caused by resisting the platform's movement would cause higher-level control systems to alter the knee-angle reference level to create compliance with the platform movement and halt the pitching of the body. So I would expect a stiffening at the knee for a fraction of a second, followed by compliance with the movement of the platform. And of course, much later, a step forward. The overall controlled variable is an upright posture.

Without a dynamic model of the rest of the body, it would be pretty hard to predict what the remaining indirect disturbances would be, and which way the forces would be applied to oppose them. Generally, I would expect that holding the head upright would take first priority because that's where the inertial guidance (vestibular "reflex") originates.

From your description I can't tell which way the first muscle effort acts -- muscles on "the back of the leg" are connected around the knee joint in some peculiar ways, so some of them are extensors. You'd have to measure the horizontal forces where the foot meets the platform to see which way the effort goes. Maybe this information is available.

Finally, there's an overall prediction that can be made, which is that when a horizontal acceleration acts on the feet, the muscles will act so as to leave the body balanced, with a line from the center of gravity through the footprint aligned with the effective direction of gravity (i.e., vertical acceleration due to gravity added vectorially to the horizontal acceleration of the supporting surface). For small accelerations this can be done with just the lower-leg muscles applying torques at the ankle joint. When the bus starts up slowly enough you just lean forward at the ankles by cocking the feet upward briefly, moving the center of support rearward. Maybe this would be the initial action when the platform begins to move forward (for a rearward movement you'd rise on your toes at first, moving the center of support forward).

As to the labels for the levels involved, I think that category, sequence, and program are far too high in the hierarchy for the sort of behavior you're studying. Standing and balancing (once established) should not require any levels of control higher than the sixth, relationship control. Walking requires mainly the event level, the fifth, with the relationship-control system maintaining balance by fine adjustments of the walking reference signals. Nothing higher than the midbrain should be involved in postural (configuration) control except for choosing the reference-posture. I would say that the relevant postural systems would be those concerned with controlling muscle tension, stretch (length and velocity), joint angle, and vestibule-based orientation (at the same level as joint angle control). In protracted experiments, visual information must get into the act because the purely vestibular information can't maintain a stable reference for more than a few seconds. There are clearly lots of control systems here to investigate. The relationships among them will get pretty complex; there are dozens of systems involved.

The "reference value for standing up" would define a target perception, probably visual but partly kinesthetic too, of an erect posture in relationship to some external frame of reference like the horizon or a room (level 6). Errors would be translated into changes in reference levels for detailed limb configurations, and errors in configuration would be translated into reference-efforts involving sets of muscles. From there on down you're into the spinal systems that control stretch and force in individual muscles. The "event" level would not come into play for just standing still.

I know we have a CT kinesiologist on the list (Larry Goldfarb), so perhaps he can chime in and refine my very crude outline. I can't be very helpful because these processes need to be studied from the CT standpoint, and haven't been. The available data is very probably all based on the SR model, so nobody bothered to look for controlled variables. If that's true, every experiment that has been done has to be done over by someone who knows how to test for controlled variables, unless by luck the required data were accidentally recorded. In your experiments with infants you have a great opportunity to observe controlled variables by keeping track of what is stabilized by the "responses" you see. You're applying disturbances in just the way a control theorist would do it. All you have to do is predict what the disturbances would do if there were no change in the actions, and verify that the actions prevent those effects. Observations like these, and not the abstract model, are the best way to find out how these systems are really organized.

One piece of instrumentation might be useful, if you're not already using it. Four strain gages, two under the heels and two under the ball of the feet, could reveal how torques at the ankle joint shift the center of support for very small (less than the size of the footprint) excursions of the platform. The position of the center of support should turn out to be a controlled variable, remaining stationary as the platform moves over a period of a second or so. This would also show how pushes against the upper part of the body are resisted when the feet don't move. The center of support should move rapidly in the direction of the push.

I wish we were further along with computer simulations of postural control, but all we have right now is a model for pointing behavior with a simplified arm. Setting up the dynamics of a physical model of the whole body with all its joints is beyond me -- we need an interested mechanical engineer or physicist who has the knowledge needed for building this part of the simulation. The control-system part will be far simpler in comparison. If I just had a model of the system dynamics I could come up with a lot of the answers you're asking for, by finding out what's required of the control systems to make the thing stand up. The dynamic relationships are just too complex and interactive to give intuition a chance.

Gary Cziko (911026) --

Yes, when you find both inhibitory and excitatory effects running in parallel in the same general path, the logical conclusion is that two-way control is involved. You would expect to find the control systems receiving such balanced pairs of signals to occur also in pairs, one for controlling on one direction and the other for controlling in the opposite direction. In fact you should find balanced pairs of systems from there on down. Also you should find that when the signal in the inhibitory path increases, the signal in the excitatory path decreases, and vice versa.

Of course "pairs" are just the minimum configuration. Control of arm position requires something like seven non-orthogonal degrees of freedom to be controlled, so you can have signals that covary in ways other than just direct opposition. The muscles of the arm aren't arranged in a neat X-Y coordinate system, but we have no trouble creating X-Y movements. See Part 3 of my Byte articles from 1979 or so.

Time for important stuff. Game 7.

Best   Bill P.

Date: Sun Oct 27, 1991 4:13 pm PST
From: POWERS DENISON C
Subject: Objectivity and perception

[From Bill Powers (911027.1200)]

Last night's turmoil over models of perception has borne a little more fruit this morning. The Little Man in my Head evidently kept working on this problem while I was away. The result is this brief essay:

OBJECTIVITY AND PERCEPTION

The aim of scientific objectivity, it is said, is to remove as far as possible all subjective bias on observations of the real world. In the physical sciences this is done through the use of instruments, reducing observations to simple judgements of coincidences. But I want to put off that part of the subject, and look more closely at the concept of observing without bias. The model of perception that is assumed makes a great deal of difference in the meaning of "observing" and of "bias."

The model I assume is this: the world we experience consists of signals in the brain created by the interaction of the nervous system with the world outside it. This means that neural signals are not ABOUT the world of experience; they ARE the world of experience. What they are about is another matter that calls for considerable investigation.

If the world we experience exists in the brain, we must then ask what objectivity could possibly mean. I think it means a certain attitude toward experiences, toward perceptions.

If you see a man carrying a briefcase hurrying along under an umbrella through the rain, you can interpret what you see in different ways. You might see a man trying to get to work on time, or someone late for an appointment, or a thief who has just stolen a briefcase and an umbrella. To see these things you clearly have to add imagined information to what you are actually observing. The same would apply if you saw a man who seemed anxious, or angry, or oblivious to the world. The most objective way of reporting what you see would eliminate all imagined information, all that is not actually in the scene before you.

To be even more objective, you would have to examine the details of what you are seeing. The man seems to be hurrying, but all you are really observing is that he moves more rapidly than others. "Hurrying" is a characterization added to what you see. He seems to be carrying a briefcase, but it could be some other object. "Briefcase" is an interpretation of the shape you see. He seems to be carrying it, but perhaps it is shackled to his wrist. "Carrying" is an interpretation of the relationship between his hand and the object. He seems to be under an umbrella that shields him from the rain, but perhaps the umbrella is a signal to someone he is to meet and isn't being used to keep him dry.

To be most objective of all, you have to ignore all these characterizations, because no matter how you characterize what you see, the characterization always goes beyond the perception; a different characterization is always possible. To be completely objective, one must simply observe without the accompaniment of an internal explanation or characterization of the observation.

This is almost impossible to do. It is possible, however, to broaden the scope of what one thinks of as observation to include not only the scene being observed, but the internal explanations-interpretations-characterizations that come along with it. If one observes both, then it is clear which set of experiences is the interpretation added to the observation. Or at least it becomes more clear.

So the most completely objective observation is that which is totally subjective and silent. It is simply attending to appearances as they actually present themselves, without any attempt to add to them or manipulate them rationally, without saying that they are real or unreal, without theorizing, associating, or explaining. Doing this to the extent that is possible takes practice and discipline leading to a state of mind much like what Zen practitioners seek through meditation.
Now we can reintroduced the subject of instruments, asking on the way why it is that such instruments are used.

The object of scientific explanation is to explain experience. More exactly, it is to explain why some parts of the experienced world are related to other parts as they are. Why is it that when there is a flash of lightning, there is quite often, after a delay, a roll of thunder? All we experience is the sequence of events; any other relationship between them is hidden. Science is an attempt to guess the connections between the flash of light and the sound, to explain the sound as a natural or necessary result of the process that created the flash of light.
Past experience tells us that the world does not appear exactly the same to everyone; furthermore, observations are inevitably tinged by explanation and interpretation, which creep in under the cover of innocuous words like "hurrying." So to eliminate these subjective differences, science employs instruments.

To measure the flash of light, a scientist would use a photoelectric cell, which responds to light by generating a small current that can be indicated on a meter or recorded on magnetic tape. But what does it mean to say that the photoelectric cell "responds to light?" It means that when the photoelectric cell shows a response, a human observer sees light. When we examine the two things being compared here, we see that they are very similar: both the perception and the meter reading are outcomes of receptor processes, one organic and one inorganic. Both outcomes depend on something else, but the human outcome is a brain signal measured in impulses per second and the light-meter outcome is the angle of deflection of a needle. Neither outcome is in units of "light."

Instruments, therefore, provide us with consistent indications of something going on on the other side of the instrument, but they don't identify to us what it is that is being measured. Instrument readings are more objective than eyeball observations only because they are more repeatable and are not influenced by interpretation prior to the reading. They are not more objective in the sense of bringing us closer to a pure description of reality itself. The basic correlate of an instrument reading is not some real physical variable, but either another instrument reading or a human perception.

Now what about the claim that instrument readings are more objective because they reduce observations to a simple discrimination of coincidence? The claim would be that the photocell measurement of light intensity is more objective than the visual estimate because the photocell always responds the same way to the same light intensity. All that the human observer has to do is read the meter face carefully, or for a digital instrument, write down the number on the display.

But what do we have then? Suppose the reading is 12.5678241. Just writing down that number is reminiscent of the joke that goes "We interrupt this program to bring you a late score: six to nothing." The number by itself is meaningless. At the very least, you have to know that it is the reading from a photocell, not from a thermometer. To use it in relation to any other meter reading, you must know how the meter is calibrated: what are the units of this number? Foot-candles? Lumens? Ergs per second? What is the spectral range being measured? And to use this reading in the context of science, you must also explain what it is that is being measured: the absorbed part of a flux of photons, a flow of energy, a squared amplitude of magnetic and electrical vibrations at a certain frequency or with a certain wavelength. You must, in short, reveal the complete model of what the meter supposedly measures.

There is no way, in fact, to reduce an observation to a coincidence of a meter needle with a mark. If it is reduced that far it ceases to mean anything.

What gives meaning to the meter reading is exactly the same thing that gives meaning to an uninterpreted human perceptual experience. It is the structure of interpretations and theories that depicts a world on the other side of the receptors. That world does not exist in unadorned, uncommented observation. It exists only in the adornments and comments added by human intellectual processes.

When we try to understand human perception scientifically, we automatically introduce something other than direct experience. We introduce a term "perception," indicating that there is a perceiver, a consequence of perceiving, and something to be perceived. This is like introducing a photocell, a photocell reading, and light-energy. There is an "inside" component and an "outside" component linked by a physical device. All three of these components are theoretical entities, part of a model of processes that underlie direct experience.

The popular conception of a light-meter is that the light is what exists, while the meter reading is only an indication of it. But considering how scientific modeling actually works, the priorities must be reversed. It is the meter reading that is given; what it indicates, and how the indication is derived, is a matter for theory and conjecture. We must reason backward from the meter reading, taking into account the theoretical properties of the meter and the photocell (and doing the same for many other kinds of meters), to deduce what lies at the origin of the reading: reality, the world.

In a model of perception itself we must do the same thing. The physical device we place between reality and perception is the nervous system. The properties of the nervous system are a matter of theory and interpretation of observations. The given is the perception, the experienced part of the process: the way the world appears. What remains to be deduced, by reasoning backward through the assumed properties of the perceiving device, is the external world.

It is possible to observe objectively only the outcome of this theoretical perceptual process. We can see whether the model consisting of a nervous system with its properties and a physical world with its properties (derived through studies with instruments) can be made to produce an outcome that matches what is in fact experienced. Objectivity then consists in observing what the model actually does, with as little interpretation as possible, and how the world actually appears, also with as little interpretation as possible. But objectivity has nothing to do with reporting on the world that is represented by our models.

Date: Mon Oct 28, 1991 5:16 am PST
From: Oded Maler
Subject: Re: Objections etc

(From Oded Maler 28-Oct-91)

Bill Powers [(911025.0745)] gave a patient explanation of my question on multidimensional signals. In order to be sure that I understand it, let me rephrase the model. I'm talking about the lowest levels where signals are intensities (no matter how encoded) or immediate combinations of intensities, that is, no temporal patterns, sequences etc. (and, of course, no conciousness, intentionality). Also I use the term 'level' in the straightforward sense (not to be confused with your 7 (plus minus 2..) levels). Thus, a system is in level i+1 if it gets its perceptual signals from systems at level i, and its "actions" are the transmission of reference signals back to those i-level systems.

Now suppose we have two lowest-levels systems each receiving a one-dimensional signal from the "real" external world, compares it to its reference signal and transmits some action to the outside world, that somehow, based on the dynamics of the physical world, causes the perceptual to move closer to the reference signals. So far so good, and it seems a reasonable description of thermostats, muscle stretch reflex, etc.

Now, suppose we build above these two systems a higher-level system, whose "perception-space" is the product of the spaces of the two lower-level systems. That is, it has two input channels, thru which the lower systems transmit their perceptual signals. Not surprisingly, the reference signal of this system is two-dimansional as well, and what it does is to substract these signals coordinate-wise, ans transmit downward to each of the two lower systems an updated reference signal according to the distance in the corresponding dimension.

This works, that is, it will bring the two-dimensional signal near the reference signal (given, of course, the underlying assumptions about the action-environment-perception loop). The problem with this view is that there is no advantage for hierarchical organization. The "fan-in" and "fan-out" (the dimensionality of the I/O space, the number of channels) of every system at level i+1, is the sum of the dimensionalities of all its level-i sub-ordinate systems, and in principle (that is, from a modeling point of view) we could supress all the intermediate levels and connect the systems at a given level n, directly with all the sensors and effectors they control.

I agree that my description abstracts away some realization problems, especially those related to timing, encoding of signals etc., and these details might show the advantage of the hierarchical organization.

The alternative approach to the above, is where the dimensionality is reduced across levels. This way, the higher level system perceives, say, a weighted sum, of the two primitive sensations, compares it to a one-dimensional reference, and than what? How does it distribute the correction among its two sub-ordinates? And suppose it somehow works, how does it work in higher levels? I don't say it can't but it needs a special world (the basic effector-environment-sensor loop) in order to operate successfully. I will stop now, since we all know that open-loop control is a kind of autism.

--Oded

p.s.

>Think of me as a sort of
>grandfatherly figure, smiling and nodding blearily and getting his kicks
>vicariously through watching the young folks' achievements, once in a
>while throwing in a word of encouragement.

>When you say
>"grandmother," are you referring to the configuration recognizeable as a
>given person, or to the attitudes toward grandchildren?

Date: Mon Oct 28, 1991 8:52 am PST
From: CHARLES W. TUCKER
Subject: PBS show on Childhood

Dear CSG'ers, FROM CHUCK TUCKER 911028.0858
Tonight will be the third show in a wonderful series entitled "Childhood" on your local PBS station. This one will cover a time period from six months to three years; that time when the human organism learns more and changes more than any time in its life (my judgement here, not theirs). The other shows were very consistent with our ideas of the development of the human being and I think I would recommend them (I have to see the rest of them) for presentations re: PCT.

I would like some comments on the show from y'all on the NET.
Chuck

Date: Mon Oct 28, 1991 1:58 pm PST
From: Martin Taylor
Subject: Re: Chaos and perception

[Martin Taylor 911028 15:20] (Bill Powers 911026b)

>What is it that a model of perception has to explain?

A slippery question, this. If, by perception, you mean all the things of which we are conscious, then it is the same question as the old philosopher's question of what is consciousness. But if you take any other approach to "perception" you presuppose a theoretical framework that incorporates its own answer to the question. Within the PCT framework, perception is defined as the full set of inputs to all the control systems--several hundred or several thousand individual and distinct one-dimensional values. In this context, the model of perception has to explain why, in specific conditions, these values show the dynamic variations they do. But this isn't at all what you suggest is an appropriate answer to the question:
>
>It must also explain why this world presents itself to me in three
>dimensions, stereo sound, and living color, chock full from edge to edge
>of continuously-present smoothly changing noise-free colors, shades,
>objects, motions, relationships, and operations in progress.

I think that JG Taylor's approach, that perception is a consequence of the behaviour that controls the perception (so compatible and complementary to your own theory) comes close to suggesting (not explaining) why the world presents itself in these ways. They are all ways of signalling requirements for behaviour that have led some of our ancestors to survive and produce offspring (the last phrase is my augmentation of JG's ideas--he deals mainly with what you would probably call "reorganization.").

But that paragraph doesn't answer the base question, and the more I think of it, the more it sounds like questions about causality. It presupposes some kind of inflow-outflow model in which there is a "thing" called "perception" that must be explained. Is PCT a theory within which such a "thing" could be identified? As I understand it, the answer is both yes and no. Yes, in that the multi-dimensional set of variables that serve as inputs to the control systems could be identified as that "thing" if they could ever be measured;

No, in that the whole activity is a matter of process, not of state. Control is a dynamic activity, and if one explains it, then one will have explained perception, at least within that conceptual framework.

In another post, you deal with subjective vs objective measures or observations. I think that the argument applies strongly here. To me, the term "objective" makes no sense when taken seriously, just as "cause" makes no sense when taken seriously. Both words are useful in casual discussion. "Objective" means something like "has a similar effect on the mental models of most 'properly prepared' observers." I became a Bayesian in principle when I was in graduate school, because I could see no way that frequentist statistics made any philosophical sense. The only solid foundation for probability had to be subjective probability, based on all the existing understanding held by the person judging the probability. It is quite possible for "properly prepared" people to agree on how an observation "should" change subjective probability, and for observations to cause them to converge on an agreement. Communications among them can change the background on which they base their prior probabilities, so that they can converge in that way, without observation (the "religious" approach). But subjectivity is the only "objective" approach. If we then apply this to the base question, about what a model of perception must explain, we can see that the answer involves a convergence of backgrounds through communication, and a convergence of understanding through observation. Both the meaning of "perception" and the nature of the models will change during this process.
Change the question to "how do we behave as we do?" and I think you will find your answer to the first question.

Martin Taylor

Date: Mon Oct 28, 1991 3:52 pm PST
From: psy delprato
Subject: Journal of Mind and Behavior

[FROM: Dennis Delprato]

The following appeared in the most recent issue of *The Journal of Mind and Behavior.* It may be of interest to some participants in CSG-L.

Teleological Submissions

We invite research scientists who conduct empirical studies on the assumption that human beings are agential, teleological organisms to submit their experimental reports to Joe Rychlak at the address provided below. Any scientist engaged in the active process of testing experimentally his or her theoretical premise that individuals are capable of self-determination, free will, personal identity, and so on has an assured outlet in *The Journal of Mind and Behavior*, so long as his or her empirical work meets customary standards of rigorous research.

Papers of a strictly theoretical emphasis pertaining to agency should be submitted to Ray Russ. We have an open policy toward such theoretical papers at present. We want to promote empirical researches in addition to such theoretical examinations. It is our view that at present too many experimental journals have the policy of forcing contributors to use the language of mechanism when this may not be the preferred alternative, nor capture the data in the truest sense of accurate description.

We will be looking for the finest examples of rigorous, empirical research in which human beings are described in agential terminology. By the same token, we will be open to articles that present researches negating a view of agency. It is our aim to further empirical consideration of an aspect of behavior that it is now unrecognized or unappreciated in the human sciences.

Mail all manuscripts (see inside front cover for complete manuscript preparation details), in quadruplicate, that pertain to agential, teleological behavior, and which are empirical in design, to:

Joseph F. Rychlak, Ph.D.
Associate Editor, JMB
Department of Psychology
Loyola University of Chicago
6525 North Sheridan Road
Chicago, Illinois 60626

All other manuscripts, both empirical and theoretical, including those pertaining to (theoretical) teleology, should be submitted in quadruplicate, as usual, to the editor, Ray Russ.

Date: Mon Oct 28, 1991 4:32 pm PST
From: Ed Ford
Subject: Understanding the variable & priorities

from Ed Ford (911028.1320)

For those who are trying to teach non-scientific types about control theory, and especially the variable, I've found (I'm sure many of you theorists have been this route) a simple way to help people grasp this rather quickly. After explaining control theory including perceptions, reference levels, perceptual errors, actions, variables, and disturbances, etc., I show this chart.

```
             60 -|
                 |
             50 -|
                 |
  ref.level  40 -| ----------------------------x------------x-----GOAL                   |
|     |
             30 -|      x     P.E.    |                 x            x                   |
|     |                 |
             20 -|           x    P.E.                |
                 |                       |                 VARIABLE                  10 -|
|
                 |                 |
              0                    -|-----|-----|-----x-----x-----|-----|-----|-----|--
M     TU    W     TH    F     S     SU    M
```

Take a couple committed to spending quality time in their marriage 40 minutes a day (or it could be a student committed to studying 40 minutes a day). They set a goal of 40 minutes, the actual time they do spend varies from day to day, and difference between that actual time and what they want is the perceptual error. Usually, unless there is a lot of anger or upset, I usually spend about 20 minutes explaining control theory and I follow this up with a demonstration of the two rubber bands knotted together (see Bill's B:CP). I then ask them to use a chart to keep track of how they accomplish their task.

The second time they come in, I'm now able to further explain control theory by using the chart which reflects their mutual attempts to control for what they both say they want, which is to spend a specific amount of alone time together. Their goal is the reference level, the actual time spent as it varies from day to day is the variable. Obviously, if a line is drawn connecting the x's, the variable stands out in greater contrast to the reference level, giving visual meaning to the word 'variable'. The difference on any one day between their goal and the time they've achieved is the perceptual error. I think it is a great way to show how the mind controls for input rather than output. What is on this chart is actually what is going on in their mind, which also argues for getting people to set very specific goals. This allows for more efficient resolution of conflicts within the mind. Hazy goals bring hazy results. Also, it isn't a case of what each person thinks of the other person, or what each other does, or blaming someone or something, but rather what they want, how important it is in relation to other goals, getting the proper feedback to assure a greater chance of success, and getting a better view of feedback in terms of a greater length of time.

In talking with Bill, another area becomes more obvious. Establishing the strength of the intent of each person or a great test for each person's stated priority. If one spouse is concerned about missing any day at all, or not spending at least the full amount of time to which they are committed, and the other spouse is not greatly concerned about missing a few days, this shows which is more committed and how other areas of his/her life are important in relation to the building of the marriage. In other words, when you find someone willing to live with more error than the other, that person can easily be perceived as 'not caring enough about the relationship', which obviously translates into revealing one's actual priorities.

I've gotten a lot more "aha's" now that I've been using the above both in my classes and with those with whom I have worked.

Gary - Your advice on getting the list worked. Thanks, Ed

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Date: Mon Oct 28, 1991 4:47 pm PST
From: marken
Subject: Behavior

[From Rick Marken (911928)]

I said:

>>So I think my debate with "conventional" psychology is not theoretical
>>but factual -- I claim that almost everything psychologists study IS
>>control.

and Bill Powers (911026.1100) replied:

>That's the crux of it. I think, though, that theory is still in the
>background, because you can't recognize control unless you know how
>control works.

I'm going to have to work on this. I think that people did recognize control before it was understood. William James seems to have known what it was (based on his description of Romeo's behavior in comparison to the behavior of iron filings). Tolman seemed to know what it was (based on his experiments showing that organisms will use highly variable means to compensate for disturbances to a particular end). I think the engineers who developed control theory knew what it was.

I agree that it can be difficult to recognize control under normal circumstances. Even when one is in a position to test for control by pushing on a potential controlled variable, it can be difficult to verify that there is control (rather than, say, passive stability) without looking for the right variables (like opposing outputs). And one knows where to look for the variables that would indicate control when one knows how control works. But I think there are cases were the phenomenon of control can be seen without knowing how it is happening -- but I need to give this some more thought.

> Even before we claim
>that control is a phenomenon, we have to present the phenomena
>unlabelled. Show a variable behavior having a consistent effect, and a
>constant stimulus suddenly having a different effect. Then present TWO
>theoretical explanations and see which works. This will possibly have the
>effect of revealing the SR viewpoint *as a theory*, and show that this
>concept of causality is NOT a fundamental tenet of science itself.

I think this is what many of our PCT demos are all about. The "e coli" demo comes to mind, for example. There a highly variable behavior (time between bar presses) has a very systematic effect (the dot stays near the target). Also, if the subject is instructed to change the intended target location of teh dot every so often then we find that the same stimulus (movement of the dot relative to a particular point on the screen) has very different effects at different times.

I prefer to think of the problem that PCT has with conventional psychology as a difference of opinion about phenomena rather than theory. I realize that the phenomena and theory cannot be separated, really. But I think it is easier to say to "conventional" psychologists "I'm interested in a different phenomenon than you are -- so you go ahead and study your stuff and I'll study mine" than it is to say "You think you are studying phenomenon A but you are really studying phenomenon B. So your theories are all wrong because they apply to the wrong phenomenon -- A rather than B". I think both statements are basically correct, but I think the first is far less contentious -- and may be closer to the "non-disturbing" way to peak the interests of conventional psychologists in "our phenomenon".

Just some random musings.

Hasta Luego Rick

Date: Mon Oct 28, 1991 4:52 pm PST
From: Martin Taylor
Subject: Re: Chaos and perception

[Martin Taylor 911028 16:00]
(Bill Powers 911026b)

>
>That's what I have against models that contain chaotic processes. My
>objection has nothing to do with the level of abstraction, the use of
>mathematics (or my meager capacities in that regard), or metaphors. My
>problem is that I can't see the connection between the way chaotic
>systems behave, from what little I know of them, and the world of experience.
>
>My model of perception is a continuous analog model because continuous
>analog signals seem to me to have the right kinds of properties to be the
>things I experience. Even though I can't describe the functions that
>produce these signals, I can describe, or so I have assumed, what these
>functions produce: this, the world. I think that any model of perception
>has to fit this world. Even a model based on chaos must at some point
>produce a smooth continuous analog world.
>

Well, I think there must be some misconception here. The use of that word "Even" in the last sentence is most peculiar. Almost any model based on chaos will produce a smooth continuous analog world (unless it is a discrete digital simulation, of course, but that wouldn't apply to the real thing).

The issues addressed by the functions that lead to chaotic behaviour are independent of the control hierarchy as such. They deal with the kinds of dimensional reduction that people (me, Oded, and a few others) have been asking. They ask about developing categories out of data that have no inherent categorical boundaries, and they derive from requirements for rapid response.

The results of the chaotic processes should be stable data that can be used rapidly by the control processes. The argument, in essence, is much the same as you used in arguing that it is better to think of the control systems that lead to steep attractor basins than to think of the basins themselves: Control systems have an energy supply independent of the reference signal and the input signal, and the application of this energy can be induced by very low energy events. Likewise, we argue that the perceiving system should not be driven by the energy of the input, but should only be influenced by that energy. It should be a high-energy system itself, awaiting only some signal to move into any of a (possibly wide) variety of prepared configurations. But a high-energy system will not normally be stable at a fixed point and at the same time be prepared for a variety of movements (into one of many attractor basins). It does not follow its input, but is directed by it. As we understand this situation, the requirement is that the boundary among these attractor basins will be fractal, and hence the orbit of the uncommitted perceiving function will be chaotic. Every point on the orbit is on the boundary of all the potential basins of attraction.

There is a degenerate case in which the fractal boudary reduces to a single point, but we prefer to think of the more general situation.

It is quite possible we are wrong in this understanding, but that's the essential underpinning of how we came to the idea that cognitive/perceptual functioning should be expected to display chaotic dynamics. It is not at all inconsistent with the idea that behaviour controls perception, or that intentions (stable) drive the behaviour that produces the desired perception. And it should inherently produce a smooth continuous analog world. We call that world in the relative absence of sensory input "dreaming."

Martin Taylor

PS. See the my contribution to the multiple book review of Penrose's The Emperor's New Mind, Behavioral and Brain Sciences, Dec 1990, p685.

Date: Tue Oct 29, 1991 6:25 am PST
From: POWERS DENISON C
Subject: Multilevel control & dimensionality

[From Bill Powers (911028.0800)]

Oded Maler (911028) --

>The "fan-in" and "fan-out" (the dimensionality of the I/O space, the
>number of channels) of every system at level i+1, is the sum of the
>dimensionalities of all its level-i sub-ordinate systems, and in
>principle (that is, from a modeling point of view) we could supress all
>the intermediate levels and connect the systems at a given level n,
>directly with all the sensors and effectors they control.

This would be true if the input transformations at the second level were of the same kind as the input transformations at the first level. To illustrate in a limited way what happens when the transformations are different, let's indulge in some algebra.

Suppose we have two first-level systems, each one sensing and controlling the magnitude of a single external variable. Let the external variables be x and y. To simplify this analysis, let's stipulate that the two first-level systems are good control systems and make their inputs match the reference signals r11 and r12 that they receive from above: that is, the perceptual signals (p11 = x) and (p12 = y) match the reference signals faithfully. Let's also assume that all the systems to be considered are dynamically stable, so we can solve the algebraic equations simultaneously for the steady-state condition of the whole system. Note that the first "subscript" is the level, the second one is the system at that level.

Now let's set up two second-level systems. The first senses the difference between the two lower-level perceptions, and the second senses the sum. To make this more interesting let's add some weights that aren't unity:

(1a)	(1b)
p21 = 2*p11 - p12	p22 = p11 + 3*p12

The error signals are the reference signals minus the perceptual signals:

(2a)	(2b)
e21 = r21 - p21	e22 = r22 - p22

The output signals are the error signals times the gain G, which we assume is the same in both systems (it doesn't have to be):

(3a)	(3b)
o21 = G * e21	o22 = G * e22

Now we have to connect the outputs of the second level to the reference signals of the first level. We do this by making sure that the feedback around every loop comes out negative. We do NOT weight the outputs to appportion them correctly for the different weights given to the inputs. All we do is pick the right signs.

Consider the input to the first level-2 system. The perceptual signal p11 from the first level-1 system enters positively, meaning that an increase in p11 causes an increase in p21 which causes a decrease in e21 and o21. We thus use a positive sign for the effect of o21 on r11. An increase in p12 from the other level-1 system, on the other hand, causes a decrease in p21 and an increase in e21 and o21, so the contribution of o21 to r12 must be negative to keep the overall feedback around that loop negative.

For the other second-level system, p11 and p12 both have a positive effect on p21, so the output of the second system at level 2 will add to the reference signals in both level-1 systems. Therefore

(4a)	(4b)
r11 = o21 + o22	r12 = -o21 + o22

Since we're assuming that the two level-1 systems make their perceptual signals match their reference signals perfectly, we have p11 = r11 and p12 = r12. We can write immediately

(5a)	(5b)
p11 = o21 + o22	p12 = -o21 + o22

We have gone all the way around all loops, so we have the complete system of equations.

Let's solve for the two second-level perceptual signals, p21 and p22.

Substituting equations (5) into equations (1) we get

(6a)
$$p21 = 2*(o21 + o22) - (-o21 + o22) \text{ and}$$

(6b)
$$p22 = (o21 + o22) + 3*(-o21 + o22)$$

The output signals are gain times error, and error is reference -perception, so for each o we can substitute G*(r - p), yielding

(7a)
$$p21 = 2*G*(r21-p21+r22-p22) - G*(-r21+p21+r22-p22) \text{ and}$$

(7b)
$$p22 = G*(r21-p21+r22-p22) + 3*G*(-r21+p21+r22-p22)$$

We have used all the relationships and can now simplify eq.

(7a):
$$p21 = 3*G*r21 - 3*G*p21 - G*r22 + G*p22$$

Collecting terms in p21:

$$p21 + 3*G*p21 = 3*G*r21 - G*r22 + G*p22, \text{ or}$$

$$p21 = \frac{3*G}{1+3*G}[r21 - (r22 - p22)/3]$$

In the same way we get for p22

$$p22 = -2*G*r21 + 2*G*p21 + 4*G*r22 - 4*G*p22, \text{ or}$$

$$p22 = \frac{4*G}{1+4*G}[r22 + (r21 - p21)/4]$$

We can get a solution for p21 or p22 by doing some horrendous substitutions, but there's an easier way. In the end we will be making an approximation that results from letting the loop gain G become very large. If we make that approximation now, we can reach the solution much more quickly.

Suppose that G is so large that 4*G/(1+4*G) or 3*G/(1+3*G) is essentially 1. We can then say

$$p21 = r21 - (r22 - p22)/3. \text{ and}$$

$$p22 = r22 + (r21 - p21)/4.$$

The solutions are

$$p21 = r21 \text{ and}$$

$$p22 = r22.$$

The two reference signals r21 and r22 are independent variables adjustable to any value. This solution says that with very large loop gain, each of these second-level systems will keep its own perceptual signal in a close match to its reference signal. Each system will do this regardless of the reference setting of the other system.

Therefore a still-higher system can vary these reference signals independently, and thus vary the value of the external variables x and y so that

$2x - y = k1$ and $x + 3y = k2$,

where k1 and k2 are the reference conditions of these abstract environmental variables -- k1 and k2, of course, are determined by the second-level reference signals.

Note: just to check that the approximation is valid in the partial solution, I finished out the solution for p22:

$$p22 = r22 \, \frac{1+(1/6)(4*G/(1+4*G)}{(1+3*G)/3*G+(1/6)(4*G/(1+4*G))}$$

$$- \, r21 \, \frac{1/(1 + 4*G)}{(1+3*G)/3*G+(1/6)(4*G/(1+4*G))}$$

In the limit, as G goes to infinity,

$p22 = r22.$

These solutions are handled a LOT more easily in simulation. Rick Marken has developed spread-sheet programs that show at least three systems at three levels (maybe more, I've forgotten), which converge on solutions quickly and maintain them in real time, with disturbances.

In case you've forgot what we were doing after all these manipulations, we were showing that one system in the second level of systems can control a function of BOTH first-level variables, while another system at the second level controls a DIFFERENT function of the same two lower-level variables. There could be any number of systems at the second level, but o nly two of them could act at a time, and the input functions must be linearly independent.

This example is somewhat trivial in that it exhausts the degrees of freedom of the environment. A more realistic arrangement would have more degrees of freedom in the environment than there are control systems at each level. In that case, controlling a function of two variables would only define a trajectory in the n-space outside, and disturbances that alter the external variables along these trajectories would not be resisted. A change in the second-level reference signals would change the positions of the trajectories along lines orthogonal to the trajectories. And of course the component of any disturbance that acts orthogonally to the trajectories would be resisted as usual.

The first level in this example controlled simple magnitudes. The second level controlled weighted sums of magnitudes. Note that the first level will automatically resist disturbances that act on either physical variable alone. Furthermore, variations in the sensitivity of output effectors will be removed by the feedback effects: the magnitudes of x and y will come to the specified values even if the output function changes its characteristics, and even if the link from action to the variables changes or becomes nonlinear (montonicity and sufficient loop gain have to be preserved, however). All this assures that the first-level perceptual signals will track their respective reference signals despite changes in the environment.

We could add a third level that controls time functions of the weighted sums, a fourth level that controls the pattern in which various time-functions are executed, a fifth level that controls logical relationships among variables, a sixth level that controls for class membership, and so on. By introducing a new type of input function at each level, we create new kinds of perceptual variables and provide for their control. At each level, we could include uncontrolled lower-order variables as inputs to the input function, so that disturbances could occur at higher levels that would not be filtered out by the lower levels.

As you say, in principle any level of control could be expressed as a very (VERY) complex function of the environmental variables, and all the intermediate systems could be dropped. But this would not allow us to explain cases in which there is apparent control of one of the intermediate variables. It would also create enormous duplication of functions, because each shared lower-level system would have to show up in each highest-level system as a set of computations representing the role of the lower-level system. Finally, it is highly unlikely that evolution could arrive directly at the exact complex computation needed to achieve higher-level control: if the levels are separated as here, the progression to higher levels of control becomes much more feasible and likely.

In case the point passed without notice, it was NOT necessary to give the outputs of the second-level system weights to take into account the different amounts of contribution of the external variables to the inputs at level 2. Only the signs had to be adjusted for negative feedback. The apportioning of output effects took place automatically, and was completely guided by the INPUT weights.

This feature of a control-system hierarchy completely removes the ambiguity that is found in top-down organizations, where outputs MUST be weighted in order to have complex effects, but in which many different weightings could have the same effect. In the control-system organization, the command hierarchy and the input hierarchy are BOTH organized, quite automatically, by the forms of the input functions. The output functions can be far simpler, and can actually be very crude as long as their gains are high enough. You can actually put weighting factors into each output effect, changing relative contributions to the lower-level reference signals, and if the loop gains are high enough these weights will not affect the outcome at all. I should mention, however, that some output complexities are needed to account for changes in type of variable in more complex or higher-level examples. But the reason for them and the effect they play in organizing behavior is entirely different from what is needed for the conventional organization of top-down models.

One last remark. When the environment has many more degrees of freedom than the control systems perceive and control, the kinds of input functions that are developed become almost optional. Many different sets of trajectories of control can thread through the n-space of reality and end up controlling it adequately for the purposes of the organism. It doesn't matter, for example, whether you control in r-theta or x-y space; you're still controlling position. The concept of *constructing* a reality takes on new meaning with these ideas in mind. It isn't that the input functions create the quarks or strings (or whatever) in which the real degrees of freedom of reality exist. It's only that they create functions of collections of those hypersmall variables that the organism then learns to control. The number of possible functions is probably unexpressible in aleph-null infinity. It is perfectly possible that no two organisms construct exactly the same functions on their inputs -- yet all are capable of keeping the local environment under control in the regards that make survival possible. I hinted at this situation in a figure in my 1973 Science article, by showing that the output of a control system affects some anonymous set of "v"s, physical variables, from which the perceptual signal is derived by the input function.
I think that the many-to-one transformations that connect the outside world to the world of perceptions introduce possibilities of variation from organism to organism that no organism, operating strictly from inside itself as it must, could detect. Every organism

necessarily projects happenings in the outside world into the space defined by its own perceptual functions. No matter how much "triangulation" is carried out, the perceptual dimensions in which triangulation takes place are necessarily fewer than those that exist in reality. The reality we experience is a projection from a world of n dimensions into a world of m dimensions, where n is vastly greater than m. So we can postulate a reality, and study it flattened out into our own perceptual spaces, but we can never know it in full. James Clerk Maxwell said in his bell-ringer analogy, "If the machinery above has more degrees of freedom than there are ropes, the coordinates which express those degrees of freedom must be ignored. There is no help for it."

On the other hand, we do pretty well within the confines of our brains, and there is reason to think that however inventive we become, we will never run out of new and interesting ways to construct new aspects of reality and learn to control them.

Best

Bill P.

Date: Tue Oct 29, 1991 8:33 am PST
From: marken
Subject: Multilevel Control & Dimensionality

[From Rick Marken (911029)]

Re: Bill Powers' post dated (911928.0800).

WOW!!!!!

What a great way to wake up in the morning.

Thank you Bill.

****

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Date: Tue Oct 29, 1991 8:52 am PST
From: marken
Subject: Correction

[From Rick Marken (911029b)]

Of course, I was referring, in my "WOW" post to Bill's post of (911028.0800), not (911928.0800). Posting on the latter date would have been a feat even beyond Bill's remarkable capabilities. Regards Rick

Date: Tue Oct 29, 1991 11:55 am PST
From: Bruce E. Nevin
Subject: constituting higher levels

[From: Bruce Nevin (911029 1221)]

This morning on the train my thoughts reverted to the vexed question of the relation of perceptual control on level 7 (category) and higher to language, itself a matter of perceptual control on at least levels 1-9.

I was influenced, no doubt, by a paper received lately from Tom Ryckman (Philosophy, Northwestern U.), an expanded version of a talk he gave at the Boston Colloquium for the Philosophy of Science earlier this month at BU. (I will send you a copy, Bill. If anyone else wants a copy, holler. I lean on it in obvious ways in the first two paragraphs below. Sounds like we should look at Harris's 1990 Oxford U. Press book _A Theory of Language and Information_. I haven't been able to get a copy yet.)

Language is commonly thought (a) to have a code-like structure and (b) to function as a code--to encode meanings for the sake of transmitting them to a recipient who decodes something like the original meanings. Martin has been articulating such a view.

The idea (b) that language functions as a code is the modern-dress rendition of the "conduit" metaphor of communication, familiar since the Stoics and even their earlier antecedents, and established virtually as a presupposition since Locke's formulation. It is an image of a process of translation from "ideas" or "mental representations" to physical tokens such as those of speech or writing and then back to "mentalese". A "message" or "belief" existing in some unspecified form of representation in the mind of the speaker A is encoded into a stream of physical tokens (e.g. phonemes or letters), and the hearer B decodes the message from this medium of transmission into a consitituent set of ideas or mentally represented meanings. B understands A iff the ideas or mental representations in the mind of B are sufficiently similar to those in the mind of A (ignoring here such things as the role of language in establishing the fact of agreement--we have fatter fish to fry).

The identity claim (a) that language *is* a code entails some claims about the character of these mental representations: a code is a 1-1 mapping between well-formed expressions in a language or language-like system and the elements of a cipher. In this view, language has a subordinate and dependent role as a mere culture-specific cipher for the universal "mentalese" of mental representations--in which latter must inhere all the restrictions as to which combinations of words may cooccur. (Certain programmatic statements in the AI community about "semantics-driven" and "syntax-free" approaches to natural-language processing trade on this supposition. They founder as soon as they encounter the actual complexity of language.)

Both the language=code claim and the conduit metaphor, then, presuppose that a grammatical structure (by which well-formedness is determined) pre-exists in the postulated mental representations antecedent to language. This Universal Grammar must specify semantic and syntactic conditions for well-formedness of mental representations. (It is noteworthy perhaps that even the UG of Generative Linguistics has shied away from making such a strong claim.)

This amounts to the the claim that there is a metalanguage external to language, using which we can determine which combinations of elements of language occur and which do not, and to account for which we do not have to rely on the "background vernacular" (Tom's happy phrase) of our shared language.

One problem with this claim of an external metalanguage is that it must in turn have a metalanguage specifying its elements and their relations. The implicit infinite regress must stop somewhere with a language that contains its own metalanguage. But natural language itself manifestly contains its own metalanguage, so by Occam's Razor an external metalanguage is superfluous. Problems of another sort arise from the well-known arbitrariness of languages, and (fundamentally related) their propensity for change and diversification even where the physical and ecological environments of their speakers are identical, problems which point to the ways in which language is socially constituted and not a biological endowment. There are other difficulties, but a shared basis for stating them must be established first, and I will not mention them further here.

Fortunately, there is an alternative to mental representationalism, in which language encodes propositions couched in some sort of "mentalese," an alternative in which socially established conventional restrictions *constitute* the information in utterances, rather than merely encoding information constituted by other means.

Perceptual Control Theory appears at first blush to come down squarely on the side of mental representationalism. Clearly, there exist perceptions, and control systems control for them. Clearly, our words refer to our perceptions. Perceptions are mental representations. Language encodes these representations or meanings for the sake of transmitting them to others. The metalanguage for describing the perceptual hierarchy is a science sublanguage, the metalanguage for a still-being-formulated science of life. Like any science sublanguage or language-like system (such as mathematics or logic), it depends on the background vernacular of shared language for its semantic and grammatical basis, but the circularity is not so obvious, nor so obviously pernicious as for a metalanguage of language directly.

But let's look a bit more closely at that. First off, perceptions are not mental representations of external reality, they *are* our perceived reality, as Bill has often pointed out. More precisely, the environment of any elementary control systems (ECSs) comprises perceptual signals and reference signals, and nothing else. I know this is well established for this audience. But if you failed to note the sentence above, "Perceptions are mental representations," as a gaffe, consider the lapse to be your personal testimony as to the hypnotic persuasiveness of the metaphor against which I am arguing.

Secondly, PCT in fact does not give a coherent account of language (yet). And indeed, it is notoriously problematic for CSGers to distinguish *perceptions* on levels 7 and up from *language* describing those perceptions. Even on the category level, level 7, Bill (surely our most accomplished adept) says that it is only with greatest difficulty that he can get even a fleeting sense of the perception "dog" as distinct from the word "dog". I think this is for very good reason, as I will try to indicate.

I believe that the perceptual control hierarchy cannot constitute a "mental representation" metalanguage for language because language is precisely a principal means by which we constitute our higher-level perceptions. Language is not a representation of perceptions: it is a major part of our means for perceiving (on levels 7 and above).

Let me here invoke the truism that on category level and higher are all the perceptions that we consider symbolic or semiotic. A symbol or sign is ordinarily taken as a representative of a thing symbolized, hence the "mapping" of mental representationalism. But what I am arguing here is that in PCT, contrary to mental representalism, the mapping is not from some complete system of mental representations to language, but from lower levels of the perceptual hierarchy to the category level (and thence higher), where the higher-level perceptions are themselves meaningful elements of language (in company, probably, with less systematic symbol systems). The category "dog" is precisely the word "dog," the mapping to particular perceptions of dogs is precisely the mapping from category level to lower levels, and the mappings over generalizations about dogs are generalizations from lower-level perceptions to category level and higher, which we may put into explicit utterance if we choose.

We are impelled to this conclusion also by the fact that language itself is evidently the metalanguage for higher levels of perception. This had been discussed as a frustrating dilemma: we can't get at the "real" perceptions on higher levels (so we thought) without language getting in the way. My claim here is that we should instead embrace the unity of language with the higher levels of perception. They are not separate. Instead, language is a social means for constituting higher levels of perceptual organization than we could possibly achieve as isolated, asocial individuals, and for doing so in a way that enables more complex and more adaptable cooperative effort than is possible for any species lacking language.

I have said that language has a constitutive role in establishing higher levels of perception. I should say, rather, that language and higher levels of perception are co-constituted by the same social means.

The category level entails the perception of repetition--"another one". As soon as you expect that this banana will taste as good and be as nourishing as the one just eaten, you are dealing with a category, such that the two are repetitions of the same category, "banana." Any of set of perceptions (yellow, long, curved, in a bunch, under that kind of leaf, etc.) might serve as a perceptual cue to investigate more closely, without standing as a symbol for the category. Probably the most general categories come first--the gesture of putting something in the mouth might plausibly stand for food. This might be abbreviated, conventionalized. But it is in fact more likely that the gestures and vocalizations of an infant will be abbreviated and conventionalized as a first communicable representation of "food" or "hungry" or "feed me," in the manner that Bruner has shown for humans and (I believe) as primate ethology also shows. Thus, it appears to be the social matrix of communication of the needs of a dependent infant that shapes the establishment of the category level, rather than some sort of asocial generalization across lower-level perceptions. This social matrix at once constitutes the information on the category level (and higher), and the means for transmitting that information, concurrently.
Before the food/hunger/feed category is established, there is no category perception, only the (later-)associated lower-level perceptions (low blood sugar, pain in stomach, etc.). As the mother and infant together shape inchoate cries and gestures into a conventionalized utterance and gesture of request, they *concurrently* shape the food/hunger/feed category. Both come into existence simultaneously. The category perception and the language-like "representation" of it are aspects of a single, indivisible unity. The fact that different and more sophisticated language is acquired later does not vitiate the point--different and more sophisticated categories (not to mention sequences, programs, principles, etc.) are concurrently acquired. And the less sophisticated stage remains, unused but unobliterated, against a time of social or physical incapacity (trying to find a bakery in Naples when you know no Italian and there are no English speakers around, comes to my personal memory).

What has been confusing about language, perhaps, is that there is a mapping from the category level and higher to particular socially conventionalized vocalizations or inscriptions, and that this mapping is arbitrary and socially set, such that any similarity between "dog" and intensity, sensation, transition, configuration, event, or relationship perceptions of dogs that may have been discernible in some primordial Ur-language has been lost for millenia, and comparably any resemblance between an infant's effortful noise and an adult's word of request (Bruner). It is a fact that the linguistic aspect of the category level (and higher) is an arbitrary social fact, linked with lower-level perceptions only by virtue of being co-constituted in the individual culture-learner as category perceptions. There is no need to make it any more complicated than that.

It is a great merit of Perceptual Control Theory that it moves beyond Cartesian dualism and shows in great measure just how mind is immanent in nature and not transcendent, apart from it. A non-dualistic account of language follows obviously from it, and is in my opinion a prerequisite to progress in those intermediate levels that link little-man arm-movement demos to the very differently successful work of therapists and the issues of sociology that have exercised us in months past.

Bruce Nevin
bn@bbn.com

Date: Tue Oct 29, 1991 3:21 pm PST

From: Gary A. Cziko
Subject: Fly Balls

It's APPLICATION time on CSGnet!

Bill Powers:

You mentioned Game 7 in one of your recent posts. This brought back unpleasant memories of playing Little League baseball and making a fool of myself in right field. I could never seem to find the spot where the ball would get close enough to the ground to be caught.

Could you tell me what I was doing wrong. Is there a simple perceptual trick to catching fly balls? Is it different for line drives? What controlled perception would have brought me and the ball to the same spot on the field?-

-Gary

Date: Tue Oct 29, 1991 3:22 pm PST
From: Duane White by way of Gary A. Cziko g-cziko@
Subject: My Resent Subscription to CSGnet

[from Duane White]

Hello,

I thought I would reply to the welcome message I received, and give some background about myself and my interests.

I am a graduate student in Electrical Engineering at Texas A&I University in Kingsville, Texas. My areas of interest, concerning my degree, are control systems, computer hardware and interfacing, robotics, and AI (specifically Neural Nets).

The topic for my Thesis is: "A Comparison of Classical Control and Neural Networks." What I plan to do is:

1. Build an Interface card for a PC to allow me to interface with and control a REMOTEC RM-10A robotic arm.

2. Use classical feedback control (PI or PID) to control the manipulation of the robotic arm.

3. Develop some form of Neural Network control for the arm to accomplish similar tasks as in step 2.

4. Make some qualitative analysis and comparison of 2 and 3.

At my present stage, (1), I know almost nothing about neural nets except the most basic concepts. Right now I'm in what you could call a research phase, looking for any information that could be helpful.

Any suggestions would be greatly appreciated,
Duane White
Texas A&I University
KSRC001@TAIVM2.TAIU.EDU

Date: Tue Oct 29, 1991 3:42 pm PST

From: Martin Taylor
Subject: Re: constituting higher levels

[Martin Taylor 911029 17:00]
(Bruce Nevin 911029 1221)

I was told by David Hill in Calgary that my use of the word "code" in describing my approach to language could be misleading. Bruce's reference to me in

>Language is commonly thought (a) to have a code-like structure and (b)
>to function as a code--to encode meanings for the sake of transmitting
>them to a recipient who decodes something like the original meanings.
>Martin has been articulating such a view.
>
followed by

> a code is a 1-1
>mapping between well-formed expressions in a language or language-like
>system and the elements of a cipher.
and
>Both the language=code claim and the conduit metaphor, then, presuppose
>that a grammatical structure (by which well-formedness is determined)
>pre-exists in the postulated mental representations antecedent to
>language. This Universal Grammar must specify semantic and syntactic
>conditions for well-formedness of mental representations.

show that David Hill was correct. I totally dissociate myself from the views Bruce imputes to me. But I don't know what other word than "code" I can use to describe the kind of transformation that occurs between levels.

Bruce makes me out to be a total symbolist, whereas I see myself as much less symbolically oriented than he is with respect to language. I could never imagine that the higher level perceptual processes could be conceived as if they were linguistic, as Bruce does, or that the notion of a grammar applies very well to systems in which there is perpetual reorganization, as must be the case for high-level control processes in a world of far more degrees of freedom than we can control for (Powers 911029).

I don't use the term "code" to represent a 1-1 mapping, or even an invertible mapping. It is a transformation, often of kind, just like the transformation from event to sequence. It represents the realization of one set of intentions with respect to the partner's perceived understanding through another set of intentions (reference signals, if you will). As for the partner decoding the original meanings, this may well be the intention, but it cannot happen unless the participants are culturally very close. More normally the intention will be to affect the partner's knowledge or behaviour in some more or less predictable (and hopefully controllable) way. I'm not sure that the term "meaning" is very meaningful in this context. One can define and use it technically or one can use it colloquially (as with "control" and "cause").
My position is that we must use physical media to influence a communicative partner, because we can use telepathy neither to transmit a message, nor to determine the state of the partner's mind, which would allow us to predict the effect of a transmitted message. Therefore we need to encode our messages, and we need feedback in order to determine whether the intentions of the various levels of transformation (I say code) are being realized (whether the partner's state or behaviour seems to be approaching the desired referent).

There is no more a 1-1 mapping of intention onto realization than there is in the muscle movements required for me to pick up the pencil on the desk beside me, and for the same reason. I would say that the muscle movements code the intention to pick up the pencil, but that code is appropriate only at this particular moment, and is changed as feedback shows the current discrepancy between intention and result. The same applies to the codes used in transforming a message-passing intention into phenomena in physical media. There is a difference, however, in that it is usually true that one of the intentions encoded is that the intention be recognized, and to that degree the code is AT THAT MOMENT invertible by the receiving partner (or at least is intended to be). In dealing with inanimate objects, the environment affected by an action is not expected to interpret the intention that was encoded in the action. Only the actor knows whether the affected perception approaches the reference.

And "This Universal Grammar must specify semantic and syntactic conditions for well-formedness of mental representations" sounds to me more like a clause generated by a fifth-order Markov process than a "meaningful" concept that could be accepted or refuted. It doesn't fit into any model of mental process that I hold. (Or linguistic, for that matter).

Martin Taylor

Date: Wed Oct 30, 1991 8:33 am PST
From: POWERS DENISON C
Subject: Perception etc.

[From Bill Powers (911028.0900)]

Martin Taylor (911028) --

It's really interesting how I can read a perfectly clear paragraph from you and wonder what you mean. This isn't your fault -- it's just the same old problem of transferring meaning from one cranium to another.

>If, by perception, you mean all the things of which we are conscious,
>then it is the same question as the old philosopher's question of what
>is consciousness.

To me there's a difference. "What is consciousness?" means, to me, "what is the mechanism (if that's what it is) that receives the information of which we are conscious?". That mechanism is not the information; it's the thing that receives and works with the information (just as a camera is not the picture it takes). In the same way, perception can refer to a process or the information handled by the process. In the plural -- perceptions -- or with the definite article -- a perception -- I take the term to indicate the content of experience, not that which forms or receives it. In the singular it's ambiguous and context has to show whether we mean the device or its activities. I suppose the word "consciousness" leads to the same problems.

>Within the PCT framework, perception is defined as the full set of
>inputs to all the control systems--several hundred or several thousand
>individual and distinct one-dimensional values.

I don't know whether your mental picture of this is the same as mine. Here's mine.

At the periphery of the nervous system, I assume not hundreds or thousands but an uncountable number of individual and distinct one-dimensional values of the physical variables capable of stimulating nerve-endings. Just inside this boundary are millions of signals that are functions of local subsets of the external variables (very ambiguous functions, in that the many-to-one convergence from multiple physical variables to a single one-dimensional signal is extreme). This produces the first level of perceptionS, namely the signals that are the outputs of the first sensory layer.

So one divergent wording I see in the above is that you characterize perceptions as inputs where I think of them as outputs. I would not call the physical variables outside the nervous system "perceptions." I would call the resulting signals, the outputs of the sensory functions, perceptions.

This continues to make a difference as we move up the levels. A configuration-level input function does not RECEIVE configuration signals; it GENERATES signals that indicate presence of a specific configuration. Thus a perception of configuration must be the signal output by the configuration-function, not the inputs to that function. The inputs are perceptions of sensations, not configurations.
This is not really a chicken-and-egg quibble. In my view, configuration does not exist in a set of sensations; it is created by a function that operates on sensation signals to derive the appropriate invariants, and a perception results only when an invariant is represented by an output signal. The configuration that is perceived is determined by the inner form of the level-3 (or is it 4?) input function. A different input function at that level will be deriving a different configuration; there can be many different configuration signals derived simultaneously from the same set of sensation signals.

By "invariant" I do not mean "constant". An invariant is always defined with respect to some transformation. That transformation determines the identity of the invariant: it does not change the state of the invariant. Changes orthogonal to that transformation measure the state of that invariant. So the output signals of a perceptual function indicate the state of an invariant.

>In this context, the model of perception has to explain why, in specific
>conditions, these values show the dynamic variations they do.

My, this is tantalizing. My first concern is not why the variables show the dynamic variations they do, but how the values of the variables depend on the values of lower-level variables. If we can find the general transformation that can take a pattern of sensations and from it generate the perception of squareness, we will know what the squareness-signal means in terms of sensations. If the sensation-signals then vary dynamically (or remain at constant values), we will be able to predict how the squareness signal will vary (or not vary). The dynamic changes ultimately, I presume, are driven by changes of some sort in physical variables outside the nervous system. But even if those external variables are not varying, we still have to account for the fact that a fixed set of sensations is transformed into a signal that indicates some constant amount of "squareness." The model of the perceptual function would consist of the computations that transform input signals of fixed or varying magnitude into an output signal that is a fixed or varying measure of squareness.

>I think that JG Taylor's approach, that perception is a consequence of
>the behaviour that controls the perception (so compatible and
>complementary to your own theory) comes close to suggesting (not
>explaining) why the world presents itself in these ways. They are all
>ways of signalling requirements for behaviour ...

But doesn't that amount to putting reference signals in the environment? Please correct me if I am completely misinterpreting where you're coming from. I had said

>>It must also explain why this world presents itself to me in three
>>dimensions, stereo sound, and living color, chock full from edge to
>>edge of continuously-present smoothly changing noise-free colors,
>>shades,objects, motions, relationships, and operations in progress.

I didn't mean that the world is really that way. I meant that this is the picture that is present in perception, inside the brain, at all the levels. The "three dimensions, stereo sound" and so on are how perceptual signals appear to consciousness. I don't believe that these perceptions indicate to us what is required of us: they indicate only how the world appears. If you mean that it is within the constraints of a world structured in this way that we must find behaviors sufficient for controlling experiences, then we are in agreement. If you mean that it APPEARS to us that we are controlling an independent external world, and that we customarily project our inner goals into that external-seeming world, again we are in agreement. But is that what you meant?

You cite J. G. Taylor's position as being that "perception is a consequence of the behaviour that controls the perception." I assume that perception is a consequence of external variables affecting sensory inputs, so that output signals are generated for processing by higher-level input functions. Perceptions occur whether or not there is behavior, don't they? I can see saying that CHANGES in perceptions result partly from behavior that affects them, but how can perception ITSELF depend on behavior? I guess I don't understand what you mean.

>But that paragraph doesn't answer the base question, and the more I
>think of it, the more it sounds like questions about causality. It
>presupposes some kind of inflow-outflow model in which there is a
>"thing" called "perception" that must be explained. Is PCT a theory
>within which such a "thing" could be identified? As I understand it,
>the answer is both yes and no. Yes, in that the multi-dimensional set
>of variables that serve as inputs to the control systems could be
>identified as that "thing" if they could ever be measured; No, in that
>the whole activity is a matter of process, not of state.

The "thing called perception" is the world that is presented to us. When we measure ANYTHING, we are measuring perceptions. This is what I want to account for. The main constraint on this accounting is the neural/physical model that says all information about the external world appears first in the nervous system -- yours and mine -- as a set of one-dimensional neural signals that can vary only in magnitude. Everything else the brain knows or experiences must be derived from the behavior of these basically identical signals -- including physical and evolutionary theories. This leads me to identify "the world" as described above *as a set of neural signals in the brain*: as a set of perceptions. That, physically, is what it is -- I can see no other way to account for it without denying the simplest facts of neurology and physics or introducing magic. I want my model to be consistent with the best physical models we have. So I am forced to conclude that the world I experience is not the world that physics studies by inference, but activities in my own brain.

I don't know where you stand on this -- it's not clear to me from your words. I know that many people understand "control of perception" up to a point, but they have not really let go of the idea that "the world" is out there, and "perceptions of it" are in here. I am saying that it's ALL perception -- i.e., in here. Even when we observe our own behavior, it's a perception of behavior that we mean. By using the term "perception" I imply a process with an input and an output, so I'm not denying that there is an external world. I'm just saying that it isn't the external world that we experience.

I'm uncertain about the degree to which you subscribe to this view. When, in the above quote, you say about aspects of the apparent world, "They are all ways of signalling requirements for behaviour that have led some of our ancestors to survive and produce offspring," I get the feeling that you are exempting concepts like "our ancestors" and the accompanying evolutionary theories from being about the world of perception. In other contexts that's perfectly OK with me. When I write programs, I'm a naive realist, too. When I drive a car on a highway, I'm a naive realist and I hope that all the other drivers out there are, too. But in this context we have to be clear about why we're theorizing. What I'm trying to do is to achieve consistency among the basic models, the most tried and true, and only the simplest and least controvertible aspects of those. So I would not invoke evolution here. If the synthesis of models I'm trying to achieve is correct, then theories of evolution are derivative; they're ideas that brains have had about the behavior of some of their perceptions and imagined perceptions. The same goes for chaos:

chaos is about perceptions in a derivative kind of way, but it doesn't get at the level of discourse I'm pursuing here. I'm talking about the very nature of our experience of a real world, which includes not only the world of the senses but the world of ideas, too.

Well, enough of that very difficult stuff. On to chaos, in your next missive.

Me:
>>Even a model based on chaos must at some point produce a smooth
>>continuous analog world.

You:
>Well, I think there must be some misconception here. The use of that
>word "Even" in the last sentence is most peculiar. Almost any model
>based on chaos will produce a smooth continuous analog world (unless it
>is a discrete digital simulation, of course, but that wouldn't apply to
>the real thing).

Hmm. Maybe we need an example to focus on. Suppose I'm tracking a moving target with my finger. What I'm controlling is the perception of the relationship between finger and target in (say) x and y coordinates. So I perceive the x and y distances between target and finger. I compare these distances (two control systems) with the desired distances, and produce an action directed to reduce the discrepancy. Result: the finger perception remains in the desired relationship to the target perception (which could be one inch to the right of the target and three inches higher -- the reference-distance doesn't have to be zero).

Clearly, the perceived distance of finger from target in x or y is an analogue variable; it can change smoothly from plus to minus. Now how could the perceptual function that produces the distance-signal be chaotically organized, yet generate a smoothly-variable (or constant) neural signal? In all the chaos models I have seen, there is a variable that oscillates in a complex pattern, the pattern remaining near some attractor in phase space, but the variable itself is continuously changing its value in a pattern that seems randomly perturbed. I agree that the changes in the variable are smooth and continuous in most cases -- but how would those smooth oscillatory changes relate to a perception that does NOT oscillate?

I'm not trying to rule out chaotic phenomena as part of the internal workings of a perceptual function. But out of that perceptual function must come a signal that varies as the target-finger distance is perceived to vary -- including the case in which target and finger are stationary. How would such a signal be produced from the oscillatory wanderings of a chaotic variable? Why does the relation of finger to target vary systematically and repeatably if the underlying process is neither (grossly) systematic nor repeatable?

The world does not look chaotic. If "the world" is really the set of all our perceptual signals, then the perceptual signals do not look chaotic. But you say that chaos is (can be) involved in their production. Do you see my problem?

I'll look up your book review.

I'll send you the Little Man (version 1) and the Demo programs. This version of the LM doesn't have dynamics but illustrates many basic principles. Surely you can find an AT with a mouse to borrow or steal.

I have finished translating Version 1 from Pascal into C, and also the dynamics program, and they both compile without errors. That doesn't mean they will run yet. I will send you the C source code when these programs are merged and running. Porting to other hardware, however, isn't trivial because library programs differ so much. I'll try to make the code ANSI compatible, but there will still be work to do. Want to send me a Sun workstation?

Rick Marken (911028) --

Your way is nicer than mine. I guess They will continue as they go no matter what we do. We might as well just keep on keepin' on.
Best to all

Bill

Date: Wed Oct 30, 1991 8:57 am PST
From: POWERS DENISON C
Subject: Language

[From Bill Powers (911030.0800)]

Bruce Nevin (911029) --

Bruce, you're getting very close to a solution of something. PCT does not, as you say, contain a coherent account of language, but you're on the track of one. My reason for introducing the category level (mentioned, I think, in BCP only as a tentative possibility and not really introduced for several more years) was to provide something to act like a symbol for higher levels to work with. I didn't realize, however, until reading your post, that a symbol IS a perception. I mean I knew that category signals were perceptions, but I didn't make the connection. You make me feel a little stupid.

Martin Taylor (911029) --

>I could never imagine that the higher level perceptual processes could
>be conceived as if they were linguistic, as Bruce does, or that the
>notion of a grammar applies very well to systems in which there is
>perpetual reorganization, as must be the case for high-level control
>processes in a world of far more degrees of freedom than we can control
>for (Powers 911029).

I agree with you, too, so I suggest that there is a transformation connecting your views and Bruce's. I think that you're right in saying that higher level processes are not linguistic, in the terms in which you and I (you more than I the amateur) have known linguistics. But I think that Bruce is trying to change the meaning of "linguistic" so we think of language in a new way. After all, to say that the higher levels of perception ARE language is also to say that language IS the higher levels of perception and control. What gets changed is the concept that language is something divorced from the rest of experience, that can be studied without coming to understand the whole human being.

Surely, to go from a world of analogue signals consisting of relationships down to intensities to a world of signals that stand for categories requires a transformation of kind. The symbols as perceptions are functions of variables in the lower-level world. You could say that they "encode" that lower-level world, but that's just another way to say "transform," which is another way to say "perceive at a higher level."

On the other hand, these symbols are perceived in meaningful order or sequence, and the meaningful orders/sequences are elements handled in logical, rational, rule-guided networks of contingencies, the program level. The control processes employed at these levels certainly "must specify semantic and syntactic conditions for well-formedness of mental representations," mustn't they? They are the rules of grammar and syntax, and perhaps (at the principle level) something that has been rather vaguely recognized as "deep structure." If the neural signals being passed around at these levels began as symbols for categories, then this symbolic character will pervade our experiences of the higher levels of perception. This will be true even if the symbols aren't words. You could say that "dressing for success" employs a grammar. Algebra certainly has its grammar and syntax. So does C.

The encoding and decoding of meanings are, as you say (and as discussions on this net amply illustrate) fraught with difficulties, not the least being the fact that the medium has more degrees of freedom than our perceptual systems have. But there is one connection that is quite reliable, and that is in the speaker's perception of the meanings to the speaker of the speaker's words. It is that perception (or its imagined counterpart) that the speaker is controlling for. This introduces feedback into the encoding process, and in fact turns it into a decoding process. Just as in my algebraic example for Oded, there is then no longer a need for an encoding process that is the inverse of the decoding process: the form of the decoding process determines how the output will be structured. The output processes only have to be able to alter the production to make its decoded result -- its transformed or perceived result -- match the intended result. This will assure that given a noise-free and distortion-free channel (if you can find one) and a listener who decodes/transforms/perceives using exactly the same functions, the same result will occur in the listener. This never actually happens, but it's as close as we can get to communicating. It seems to me that you have said almost the same thing.

The key to getting together on these ideas is, I think, to recognize that language itself is in process of being redefined.

Gary has sent me a message saying the J. Marvin Brown is or will soon be on the net. He is a linguist and a friend, who teaches in Bangkok. I welcome him with affection and hope he will join the fray.

Thank you, Rick. WOW is a very nice reinforcement.   Best to all   Bill P.

Date: Wed Oct 30, 1991 8:58 am PST
From: Bruce E. Nevin
Subject: no more code metaphors

[From: Bruce Nevin (911030 0723)]

(Martin Taylor 911029 17:00)--

Well, that clears up a stumbling block in my understanding of what you are saying, Martin. I hereby exempt you from the tar brush which, nonetheless, many writers on language apply liberally to themselves. Provided you stop using words like "code" and "encode". They don't apply to the little man demo or the crowd demo and they don't apply to language. Transformations effected in "interpretation" of higher-level error signals as a reference signal, or in comparison of lower-level perceptual signals with that reference signal, are of the same kind for all behavior, including language. The requirement that a code be a 1-1 mapping from language to cipher makes it clearly inappropriate for hierarchical relations in a complex living control system. And use of the word "code" while denying that you intend its customary heavy metaphoric freight seems to me at best perniciously obtuse.

>I don't know what other word than "code" I
>can use to describe the kind of transformation that occurs between levels.

How about "transformation" or "mapping" between levels? I agree you intend something more complex than is often intended by these terms, but their definitions embrace more complex cases so the disparity is I submit less than for "code," and these terms, lacking the familar association with the "conduit" or "portage" metaphors of communication, are certainly less misleading. Even better, I suggest again Wm. Pierce's notion of constitutive rules (references posted previously, I can dig them up again, if you like), which I think correspond very nicely to what you have in mind, though his direction of approach is sociology.

>I could never
>imagine that the higher level perceptual processes could be conceived as
>if they were linguistic, as Bruce does, or that the notion of a grammar
>applies very well to systems in which there is perpetual reorganization,
>as must be the case for high-level control processes in a world of far
>more degrees of freedom than we can control for (Powers 911029).

I am not being reductionist. I am not just saying that higher levels of perceptual control are merely linguistic. First, I am saying that they include what we are accustomed to talking about as symbolic and semiotic, of which language is a part. Second, I am saying the converse, that "linguistic" is a richer term than had been thought (and similarly for "symbolic," etc.).

The constraints in language that we call grammar are subject to reorganization, and this is a clear and explicit property of Harris's operator grammar (e.g. analogic extension, discussions of language change and variation, etc.).
-=+=-=+=-=+=-=+=-=+=-
I must say I am not entirely convinced by your disavowals, however. Your discussion

>we must use physical media to influence a communicative
>partner, because we can[not] use telepathy [for feedback]
>. . . to predict the effect of a transmitted message.

suggests that there might be a choice, whether or not to use physical media, as though people had messages or communicative intentions prior (phylogenetically, ontogenetically, or logically prior) to language, and what I wrote directly undercuts any such presupposition. For example, people do not have the message that they utter distinct from and prior to its utterance. I submit that people do not formulate a message whole and then utter it, but rather formulate it in the very process of uttering it. (Rehearsals and other apparent exceptions depend upon imagined utterance.) The message is immanent in its utterance, not transcendent, and is shaped as much by the processes of utterance as by any higher-level communicative intention. Put another way, intentions (error signals) that result in formulating/uttering a message should not be termed the "message" that the utterance "encodes." The experience of thirst might eventuate in my getting up, filling a glass with water, and drinking, or any of a great many other things, including (particularly if I were a woman) asking you if you would like something to drink. The error signal of thirst is no more the message "encoded" by the latter than by the former.

You say there is a difference:

>There is a difference, however, in that it is
>usually true that one of the intentions encoded is that the intention be
>recognized, and to that degree the code is AT THAT MOMENT invertible by
>the receiving partner (or at least is intended to be). In dealing with
>inanimate objects, the environment affected by an action is not expected
>to interpret the intention that was encoded in the action. Only the actor
>knows whether the affected perception approaches the reference.

This distinction applies to all social action, not just to language. Here again, I suggest you look at Pierce's notion of constitutive rules. The code metaphor is simply too impoverished to express the richness of interactive mutual calibration of intent that appears to go on in much of social action, and is completely off the mark for the colligative effect of individual intentions as in the crowd demo--not to mention misleading any potential audience in, as I have said, a most pernicious way.

Be well,

Bruce Nevin
bn@bbn.com

Date: Wed Oct 30, 1991 8:59 am PST
From: CHARLES W. TUCKER
Subject: CAUSE, CHILDHOOD AND WORDS

CSG-EM16 FROM CHUCK TUCKER 911030.0800

Dear CSG'ers:

I think we will have to call a "timeout" on the NET for about a week before I will be able to catch up; I am still reading posts from a week ago and have not begun to outline any comments on them. I will keep working since I realize that my comments are so valuable to all of you. But, in the meanwhile, I stumbled across a comment by my friend Morse Peckham on the topic of "causality" that ought to thrill all of you who have discussed it on the NET. As you might imagine, his statement does not quite match perfectly with anyone elses but I think that it is similar enough to several that it might be useful. Here goes:

"Before 'cause' is anything else, before it is a notion, or an idea, or a concept, 'cause' is a word. That word appears in verbal behavior after an observation of the nonverbal has been made or even while it is being made; when it appears makes no difference. Furthermore, as the various successive steps in the subsequent verbal process are made, 'cause' can take on different functions, though that function always exports from the verbal into the nonverbal a factor which is not observable.[p. 8]"

. . . .

"In short, 'cause' belongs to verbal behavior, not to nonverbal events, even nonverbal behavioral events; and an instruction to determine a cause for a nonverbal event is an instruction to engage in verbal behavior, not nonverbal behavior.

Comparing two similar but not identical sentences will make this clearer. "My finger hit the typwriter key and causes a letter to appear on the paper," and "My finger hits the typewriter key and a letter appears on the paper." What does 'cause' in the first sentence add to the second sentence? Certainly the addition of this word together with 'to' gives me the feeling that I have said something of significance, but have I? Surely I know nothing more about the operation of the typewriter that I did before, but on the other hand - and this is the explanation for the sense of significance - an opportunity has been opened to me to learn something more. Suppose that I now say to myself, "If I hit the key with my finger a letter will appear on the paper." And suppose that I do that and a letter does not appear, the introduction of the word 'cause' enables me to make further tests. I discover then that I did not hit the key hard enough, as a few tests show me. . . . Thus, the previous statement that the word 'cause' is an instruction [p. 9] to engage in verbal behavior, not nonverbal behavior, needs to be qualified. If the causal statement is recast in a "If ... then..." form which consists of instructions for nonverbal behavior, then the behavior initiated by the word 'cause' can eventuate in nonverbal search behavior. At this point, however, it is advisable to introduce a caution. If I "search" for both verbal and nonverbal behavior, I tend to subsume both kinds in such a way that a similarity between the two appears to be asserted. Clearly, when I talk about verbal search behavior, I am using 'search' metaphorically. On the other hand, when I talk about nonverbal search behavior as initiated by the term 'cause,' I am initiating not only search behavior but also 'test behavior'." . . . Its appearance (the word 'cause') can eventuate in both verbal and nonverbal behavior, though, it is to be remembered, it need not eventuate in any behavior." [p. 10]

What is assumed by Peckham's statements is that words are used by the person to set a reference condition but none of what he writes would disallow this possibility and, in my rendering of Peckham's writings, his semiotic formulation calls for it. How words work neurophysiologically is a topic for another post but a hint is that words are gestures and gestures are physical nonverbal conduct.

A note on a word: My daughter has taped 3X5 cards on objects with the words she uses for them on the cards so my grandson Mac will match words with objects. They have an aquarium with fish in it and she put a card with the word 'fish' on it and taped it to the aquarium. She then asked Mac, while pointing to the aquarium, "What are those?" and he said "That's aquarium". Now she has to put another card on it. The next time she asks he will probably say, "That's water".

On "Childhood" [PBS 10/28/90]

I was rather disappointed with this segment of the series. I found the part with Kagan to be too S-R and the part on the day care to be like an advertisement for day care centers. On the other hand, I found the family of hearing impaired who taught ASL to their child to be very interesting and the part with Brontbrenner (sp?), which emphasized the activities of the child in forming his own conduct, to be excellent. The part with Kagan that I found interesting was where he claimed to be "showing" the moral judgments of the child. It seemed to be something that Piaget would do and it did use the idea of introducing "disturbances" for the child.

Date: Wed Oct 30, 1991 12:43 pm PST
From: Martin Taylor
Subject: Re: no more code metaphors

[Martin Taylor 911030 14:10]
(Bruce Nevin 911030 0723)

Bruce, you must live in a different linguistic environment than I, and in one perhaps like David Hill. I have been working with the idea of coding as transformation for 35 years or so, and this is the first time anyone has complained that its normal meaning is not as I have been using it. But having just gone to the library and looked at technical and general dictionaries, I can certainly see where you are coming from: Code as a representation of letters and other symbols in a numeric sequence, code as a set of rules and regulations, code as shoemaker's wax (!)... One-to-one mapping does seem to be a component of a lot of the meanings.

To me, coding usually conveys the idea of a one-to-many transformation that ensures redundancy, structure, and separation in the transformed message space. But it is clear from your and David's reaction that there exists a community of people with whom I wish to communicate that has an entirely different concept of what "code" implies. So I guess I will have to try to find another word. "Mapping" certainly doesn't do it. It has all the same connotations to me that "code" does to you--pre-specification, a probability that the mapping is 1-1 and invertible, and so forth. Transformation is too general. Also, I want to use the concept of a process that performs the action, and for this I have always used "Coder" and "Decoder" for the process in the sender of the message and in the recipient respectively. "Mapper" and "Demapper" hardly fill the bill!

I'm not sure where the idea of a conduit links with the idea of code, but I do use (heavily) the idea of a virtual channel, through which a virtual message is passed. If this is a conduit, so be it, but remember it does not objectively pass the message, but instead it passes whatever information is required at that level of abstraction to get the partner to appear to understand the message.

Now we have another terminological misunderstanding, as evidenced by

>I must say I am not entirely convinced by your disavowals, however.
>Your discussion
>
>>we must use physical media to influence a communicative
>>partner, because we can[not] use telepathy [for feedback]
>>. . . to predict the effect of a transmitted message.
>
>suggests that there might be a choice, whether or not to use physical
>media, as though people had messages or communicative intentions prior
>(phylogenetically, ontogenetically, or logically prior) to language, and
>what I wrote directly undercuts any such presupposition. For example,
>people do not have the message that they utter distinct from and prior
>to its utterance. I submit that people do not formulate a message whole
>and then utter it, but rather formulate it in the very process of uttering it.

I think you are identifying "message" with a word string, at least as far as I can interpret what you write. And in conjunction with your other posting (and later in this same one), I am not sure what you mean by "language." So, rather than comment on what I don't understand of your criticism, I'll try to clarify.

The way I am working it, A finds that the world is not as he wants it to be (perception does not match reference). There is something A can do to improve the situation (a goal and a plan to achieve it are initiated). This something might better be done with the participation of B, and A's model of B does not predict that B will do the necessary things as matters stand, so A must do something to affect B's future behaviour (a communicative goal emerges as a subgoal of the main goal). To achieve the communicative goal, A determines what information must be received by B -- hold on, don't complain yet! -- and that information is what I call "the top-level primal message".

The reason I said not to complain yet is that I know (regardless of PCT) that A's determination of the information B needs to get is predicated on a model of B that is incomplete and very probably incorrect, and that A will be modifying his actions as a consequence of B's behaviour, in a classic PCT way. The "information" A wants B to get is an initial multidimensional representation of what A thinks will get B to act as A wishes, and it will be continuously modified as A observes B's reactions.

Now this message HAS to be prior to language, as I understand language. A may necessarily give B the information without language, because it may be important to achieving the goal that B not know that A provided the information (Iago dropping Desdemona's handkerchief is the classic example of how such a message may be encoded). But often, the communication goal is achieved through dialogue, by which I mean that A intends, as part of the communication goal, that B know that A intends to communicate. In this case, A has as part of his intention for B's behaviour that B should intend to communicate with A about the message A is trying to send (e.g. that B should pick up the bucket and pour its contents into the tub). Even here, it's not clear whether we should use the term "language" for the encoding of the message; if A cocks his head at the tub, recognizing that B is aware of the proability that pouring the bucket will be needed, is that "language?" Perhaps, but that's not the way I have understood the word. If B fails to pick up the bucket after the cock of the head, then A might recode the message, either gesturally or by using words or both. In my terms, this would be using different support protocols to convey the same message (naturally in different encodings, too). The protocols run over different channels, and here we do use the conduit metaphor. I see nothing wrong with it, provided you don't expect the same thing to come out of the end of the conduit as went in the beginning. It's more like pushing an object on a wavy surface, where the result is the movement of the object and its relation to the intended movement, rather than the result being whether the push was the same push you applied.

(Bill Powers 911028.0900)

I'm afraid that many people have trouble understanding what I write. I don't know why, because what I am trying to get across seems very clear to me. I envy you your clarity of exposition. Even when I don't fully agree with you, I usually have the feeling I know what you are trying to say.

I'll try to answer your posting about the nature of perception separately (later; this one has taken an hour!).

Martin Taylor

Date: Thu Oct 31, 1991 7:03 am PST
From: POWERS DENISON C
Subject: Catching fly balls

[From Bill Powers (911031.0730)] Gary Cziko(911029) --

You ask if there is some trick to catching a fly ball. Yes, there is. See Chapman, Seville (1968); Catching a baseball, American Journal of Physics Vol. 36, No. 10, p. 868-870. Chapman never came right out and said that the outfielder had to control a perception, but he came near enough.
The basic finding is

" Thus, for an outfielder in the right place, the _tangent of the elevation angle increases uniformly with time_ until the ball is caught."

He explains helpfully that an external elevator rising up the side of a building at a constant speed would have this appearance (to a stationary observer).

"In the case of a pop fly to the infield having the _same_ initial vertical component of velocity and hence the same initial rate of change of elevation angle as seen by the outfielder, tan [theta] increases at a _decreasing_ rate. In the case of a home run, tan[theta] increases at an _increasing_ rate." (p. 869). Theta is elevation angle.

If the player is in the wrong position initially, but after a short delay starts running so as to keep the rate of increase of tan[theta] constant, again he will end up at the right spot at the right time. "Presumably he is adjusting his speed to maintain constancy of the rate of change of tan[theta]." (p. 870). (Because this is what is required; Chapman doesn't explain how he might accomplish this purpose).

As to azimuth, the principle there is even simpler: keep the ball at a constant bearing relative to the direction of running (i.e., adjust the direction and speed of running so that the bearing of the ball remains constant -- if you can). Chapman cites this principle of "proportional navigation," and concludes that "an astonishingly simple amount of information on the constancy of the rate of change of tan[theta] and on the bearing of the ball tell him that he is running at the right speed in the right direction for the catch. Baseball is still a great game, even if the physics in it is unknown to the players." (p. 870).

Had he known (or applied) control theory, he might have said that baseball is a great game, and the players don't have to know physics in order to control the right perceptions.

I should add that they don't have to know trigonometry, either. For ordinary fly balls to the outfield, the maximum angle will be only about 50 or 60 degrees at the time of the catch. So theta and tan(theta) differ only by a small amount -- and at the higher angles, where the inaccuracy is greater, the ball is nearing the player, so estimation errors make less and less difference. If the player has a somewhat inaccurate reference signal for the required pattern of rising speeds (event level?), the running speed will have to be corrected continuously, but not by much.

Now that you know how Willy Mays did it, you can sign up for Senior League.

Best

Bill P.

Date: Thu Oct 31, 1991 9:29 am PST
From: Gary A. Cziko
Subject: Re: Catching fly balls

[from Gary Cziko 911031.1110]

Bill Powers (911031.0730) said:

>" Thus, for an outfielder in the right place, the _tangent of the
>elevation angle increases uniformly with time_ until the ball is caught."
>
>He explains helpfully that an external elevator rising up the side of a
>building at a constant speed would have this appearance (to a stationary
>observer).
>
>Now that you know how Willy Mays did it, you can sign up for Senior League.

Not so fast. It still seems pretty tough to me, even understanding the physics.

How do I know if the angle of the ball is growing at a steady rate? You've provided an "objective" variable to control, but PCT has to do better than than this. What is the "subjective" variable that is controlled. I understand that the eye muscles do not provide position feedback so this could not be used. I can see how playing in a domed stadium or outdoors with a partly cloudy sky could provide a background field against which to judge movement of the ball. But this may not be possible outdoors under a perfectly clear or completely overcast sky. Is catching fly balls more difficult on a clear or overcast day outside? Or perhaps Willie Mays kept his eyeballs fixed and caught his balls by running to keep the angle of his head to his body increasing at a constant rate!?

--Gary