CSG_9101

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Date: Wed, 2 Jan 91 19:06:10 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mar@CS.ABER.AC.UK
Subject: Positive feedback

Bill--

Positive feedback has always been intriguing to me. When I read your book I immediately thought: it can't be just like that, positive feedback may also have a role on behaviour control. I'm still confused about it. I'll give you an example: in a workshop on behaviour-based robotics last June in Germany, there was a talk by Simon Goss (Universite' Libre de Bruxelles, Belgium).

His approach to intelligent behaviour is based on a team of simple, random and identical units that need only be locally informed, without being hierarchically organised. Distributing the team within the environment and introducing positive feedback interactions between individuals allows the amplification of localised information. The coordinated collective reaction to these local signals is the solution of the problem.
In this model, no individual is aware of all the alternatives possible, and no individual contains an explicitly programmed solution, together they reach an "unconscious" decision. He termed this process functional self-organisation.

Rather than inventing such systems from scratch, he's chosen to examine existing ones, the best possible material being provided by group living animals. He considered a very simple one, the "formica simplex" (ant) with no memory, no flight ability. The trail back to the nest is reinforced by recruits, with less individuals missing. An ant always chooses a trail with more pheromone, a substance they produce. When it chooses, it deposits more pheromone on that trail, performing positive feedback.

When only a few possible paths are offered to these ants, they always choose the shortest. There is nothing extraordinary in it, only positive feedback. In a random move, the ants that have chosen the shortest path arrive to their objective first, and the chosen return path is the same because is the only one with pheromone. When ants from longer paths arrive, the return path is inevitably chosen to be the shortest because it has already at least two layers of pheromone deposited. There is no goal, no formal communication, and yet they achieve the task through a very simple mechanism.

Now I wander if isn't there positive feedback in some of the higher levels of control systems... the control of principles, or the control of system concepts, for instance. I confess that I'm confused, this is conflicting.

Regards,

Marcos.

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Date: Wed, 2 Jan 91 15:53:58 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Faculty Position

I would be very pleased to have someone who knows about control theory get this job in my department. A letter circulated with the announcement says "basic research with potential for contributing to an understanding of educational issues is completely acceptable" and so prior research in educational settings doesn't seem to be a prerequisite. Bill Powers will be talking to the College of Education here on March 1, so at least some people around here will know about control theory after then. He will also talk at the Beckman Institute.

Urbana-Champaign is an attractive place from a number or perspectives. While weather and scenery are not wonderful, there are great arts, athletic, and academic facilities. While control theory has yet to make a big impact on this campus, sociologist Clark McPhaiI is here and I'm starting to get some students interested.--Gary

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Dear Colleague:

A Cognitive Science faculty position is open at the University of Illinois' Department of Educational Psychology (see announcement below). There is the possibility for association with the Center for the Study of Reading or the Beckman Institute for Advanced Science and Technology. We are looking for someone addressing Cognitive Science issues of importance to education, who has an established line of work. Level is open. Please let me know of appropriate candidates. We are particularly interested in identifying candidates from minority groups.

George W. McConkie (george@huey.vp.uiuc.edu)
Chair, Cognitive Science Search Committee

FACULTY POSITION IN COGNITIVE SCIENCE AND EDUCATION
Department of Educational Psychology
College of Education
University of Illinois at Urbana-Champaign

POSITION: Tenure track faculty position (open rank, with preference for an advanced assistant or beginning associate professor) in Cognitive Science to join College of Education faculty as part of the Department of Educational Psychology. Available August 1991.

QUALIFICATIONS: We seek an individual who possesses an earned doctorate

with a specialization in an area of cognitive science. Applicants should have demonstrated a record of scholarly productivity in some aspect of cognitive science that is important to educational issues or in applying cognitive science perspectives to education.

RESPONSIBILITIES: The successful candidate will be expected to fulfill the traditional professorial roles of teaching and advising, conducting research, and providing service. We seek someone who can provide leadership in the department in the area of graduate instruction in cognitive science and education. The individual will also be expected to provide leadership in developing programs of research in which cognitive science approaches are taken to address issues of importance to education. Some allied areas of expertise within the department include learning, knowledge structures, cognitive strategies in science and other content domains, reading and reading comprehension, and metacognition. With appropriate interests and qualifications, there is a possibility of affiliation with the Center for the Study of Reading or with the Beckman Institute of Advanced Science and Technology.

SALARY: Competitive, depending on experience and qualifications.

APPLICATION PROCEDURES: To ensure full consideration, nominations and applications must be submitted by January 31, 1991. Applications (including a letter of application, a curriculum vita, three letters of reference, sample publications, and other supporting materials) should be sent to: Professor George McConkie, Cognitive Science Search Committee Chair, Department of Educational Psychology, 1310 S. Sixth St., Champaign, Illinois 61820. Phone: (217) 333-7634.

The University of Illinois is an affirmative action/equal opportunity employer.

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Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: language and hierarchy

Bill and anyone with thoughts on the subject,

Trying to glean from "Behavior..." aspects of language related to levels of the hierarchy, I've made the following tentative outline:

LEVEL ASPECT OF LANGUAGE

Intensity	printed image on retina (movement in the case of ASL); soundwaves in auditory canal; braille letters on skin.
Sensation	identification of 1st order input functions as "linguistic": letter-like shapes/language-like sounds.
Configuration	"letters", phonemes, (syllables)
Transition	dipthongs, syllables, segmentation, intonation, tone
Events	words, phrases

I stop here since I haven't read the 1988 extension of the hierarchy. A couple of questions from the 1973 book: At that time, the level denoted "Program" contained a brief discussion on the "use of words", one of which would require a different level of hierarchy. Preference was expressed for the idea that words are another kind of perception (therefore not requiring another level). Did the addition of levels resolve this? The same discussion mentioned a couple of different ways of dealing with verbal communication - a perceptual transformation as the message is being received, or use of a "word-handling hierarchy", postponing perceptual transformation until the word processing is finished. The latter would lead to an overly literal rendition of non-literal messages. These two ways are mentioned as another reason to treat words as perceptions. I don't think I follow the segue into the concluding remarks on symbols on page 166. Are you simply pointing out how we can use or react to "programmed manipulations of words" without some higher order control of them? Or am I missing something?

At this (seventh) level it seems to be implied that grammar rules are dealt with, since in talking about Control of Principles it says, "Principles are perceived and controlled at a higher level than the level at which logical or grammatical sentences can be constructed (p.169)." But chomskian notions of "deep structure" or some kind of implicit 'native-speaker' knowledge of grammar would fall under eighth-order or higher levels, is that correct? And then a recognition that our [individual] grammar is somehow deviant from a social norm would be higher still?

Finally, regarding L2 and the same Chapter 13 discussion, could something like rote memorization of another language be characterized as word perceptions which evoke no non-word perceptions? Higher order systems would then have no use for these "words" until they could be associated with our experience. Such a characterization of course gets into a reply of Bill (901130) where L2 learning comes to mean representing one's experiences in a different way, not learning translation equivalents (if there are such things) for L1 words. And this leads into a characterization of an "early" bilingual's languages as "unitary" inasmuch as the child grows up in a single environment, two languages simply imply two ways of talking about experiences. However, "late" bilinguals also only have their experiences to

go on. I think there is a problem in perceptions of language at a societal (system) level and an individual (system) level. Not only do language teachers hold up learners to the standard of "Native Speaker", but I'm afraid this standard really entails an implied, archetypal (or stereotypical?) "Native Person" which deosn't really exist, but has become some sort of shared perceptual reference. Any current CT implications for these ideas?

Joel

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Date: Wed, 2 Jan 91 17:33:16 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko, Ed Psy, U Illinois, Urbana" <CZIKO@UIUCVMD.BITNET>
Subject: Noticing the obvious

NOTE: THIS IS REALLY FROM BILL POWERS -- I'M TEMPORARILY USING AN OLD LOGON OF GARY CZIKO'S. I WILL RECEIVE CSG-L MAIL AS USUAL. IF YOU WANT TO SEND DIRECT TO ME ONLY, SEND TO CZIKO@UIUCVMD (BITNET), OR G-CZIKO@VMD.CSO.UIUC.EDU (INTERNET) AND INCLUDE A NOTE THAT IT'S FOR ME.

GARY: I SEEM TO HAVE ACCESS TO YOUR READER FILES...THIS IS GOING TO TAKE SOME WORKING OUT. HOPE I GET A REAL LOGON SOMEWHERE SOON.

Gary said he is sending me a paper in which "limit cycles" are supposed to play some part in behavior. I haven't seen it yet, but I'm reminded of a lot of the "new physics" stuff that's been going around -- The Emperor's New Mind, The Quantum Self, chaos in the brain, and so on. I'd like to say this about that:

An Essay on the Obvious

I think that all attempts to apply abstract physical principles and advanced mathematical trickery to human behavior are aimed at solving a nonexistent problem. They all seem to be founded on the old idea that behavior is unpredictable, disorderly, mysterious, statistical, and mostly random. That idea has been sold by behavioral scientists to the rest of the scientific community as an excuse for their failure to find an adequate model that explains even the simplest of behaviors. As a result of buying this excuse, other scientists have spent a lot of time looking for generalizations that don't depend on orderliness in behavior; hence information theory, various other stochastic approaches, applications of thermodynamic principles, and the recent search for chaos and quantum phenomena in the workings of the brain. The general idea is that it is very hard to find any regularity or order in the behavior of organisms, so we must look beyond the obvious and search for hidden patterns and subtle principles.

But behavior IS orderly and it is orderly in obvious ways. It is orderly, however, in a way that conventional behavioral scientists have barely noticed. It is not orderly in the sense that the output forces generated by an organism follow regularly from sensory inputs or past experience. It is orderly in the sense that the CONSEQUENCES of those output forces are shaped by the organism into highly regular and reliably repeatable states and patterns. The Skinnerians came the closest to seeing this kind of order in their concept of the "operant" but they failed to see how operant

behavior works; they used the wrong model.

Because of a legacy of belief in the variability of behavior, scientists have ignored the obvious and tried to look beneath the surface irregularities for hidden regularities. But we can't develop a science of life by ignoring the obvious. The regular phenomena of behavior aren't to be found in subtleties that can be uncovered only by statistical analysis or encompassed only by grand generalizations. The paydirt is right on the surface.

The simplest regularities are visible only if you know something about elementary physics -- and apply it. Think of a person standing erect. This looks like "no behavior." But the erect position is an unstable equilibrium, because the whole skeleton is balancing on ball-and-socket joints piled up one above the other. There is a highly regular relationship between deviations from the vertical and the amount of muscle force being applied to the skeleton across each joint. There is nothing statistical, chaotic, or cyclical about the operation of the control systems that keep the body vertical. They simply keep it vertical.

The same is true of every other aspect of posture control and movement control, and all the controlled consequences of those kinds of control. Just watch an ice-skater going through the school figures in competition. Watch and listen to any instrumentalist or vocalist. Watch a ballet dancer. Watch a stock-car racer. Watch a diver coming off the 30-meter platform. Watch a programmer keying in a program.

It's true that when you see certain kinds of human activity, they seem disorganized. But that is only a matter of how much you know about the outcomes that are under control. The floor of a commodities exchange looks like complete disorder to a casual bystander, but each trader is sending and receiving signals according to well-understood patterns and has a clear objective in mind -- buy low, sell high. The confusion is all in the eye of the beholder. The beholder is bewitched by the interactions and fails to see the order in the individual actions. When you understand what the apparently chaotic gestures and shouts ACCOMPLISH for each participant, it all makes sense.

Of course we don't understand everything we see every person doing. It's easy to understand that a person is standing erect, but WHY is the person standing erect? What does that accomplish other than the result itself? We have to understand higher levels of organization to make sense of when the person stands erect and when not. We have to understand this particular person as operating under rules of military etiquette, for example, to know why this person is standing erect and another is sitting in a chair. But once we see that the erectness is being controlled as a means of preserving a higher-level form, also under control, we find order where we had seen something inexplicable. We see that an understanding of social ranking, as perceived by each person present, results in one person standing at attention while another sits at ease. Each person controls one contribution to the pattern that all perceive, in such a way as to preserve the higher-level pattern as each person desires to see it.

It seems reasonable that once we have understood the orderliness of simple acts and their immediate consequences, we should be able to go on and understand more general patterns that are preserved by the variations that remain unexplained. As we are exploring a very large and complex system, we

can't expect to arrive at complete understanding just through grasping a few basic principles. We must make and test hypotheses. But if we are convinced that the right hypothesis will reveal a highly-ordered system, we will not stop until we have found it. If, on the other hand, we are convinced that such a search is futile, that chaos reigns, we will give up the moment there is the slightest difficulty and turn to statistics.

I claim that human behavior is understandable as the operation of a highly systematic and orderly system -- at least up to a point. I say that it is the duty of any life scientist to find that orderliness at all discoverable levels of organization, and to keep looking for it despite all difficulties. We must explore all levels, not just the highest and not just the lowest; what we find at each level makes sense only in the context of the others.

We have a very long way to go in understanding the obvious before it will be appropriate to look for subtleties. I have no doubt that we will come across mysteries eventually, but I'm convinced that unless we first exhaust the possibilities of finding order and predictability in ordinary human behavior, we won't even recognize those mysteries when they stare us in the face. I don't think that anyone is prepared, now, to assimilate the astonishments that are in store for us once we have understood how all the levels of orderly control work in the human system.

We won't get anywhere by looking for shortcuts to the ultimate illuminations that await. Most of the esoteric phenomena of physics that are taught in school today were occurring in the 19th Century. But who, in that century, would have recognized tunneling, or coherent radiation, or time dilatation, or shot noise? If we want to see a Second Foundation of the sciences of life, we have to begin where we are and build carefully for those who will follow us. If we succeed in trying to understand the obvious, the result will be to change what is obvious. As the nature of the obvious changes, so does science progress.

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Date: Thu, 3 Jan 91 07:30:26 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko, Ed Psy, U Illinois, Urbana" <CZIKO@UIUCVMD.BITNET>
Subject: Positive feedback

Originated by Bill Powers via Cziko: 910103
Marcos Rodrigues (910102) --

I'm glad you were tempted to join the conversation. Welcome.

Your example of the ants laying trails of pheremones is interesting. It seems somewhat idealized, however, in that it requires the later-arriving

ants to carefully avoid the paths taken by all ants who arrive at the objective before them on the first trip. If, on the initial trip, the third-arriving ant picks up and follows the trail of the second-arriving ant, then there will be another trail with a double dose of pheremone to follow away from the destination, towards home. I should think that as more and more ants converge on the destination on the first foray, the number of fortuitously-doubled, -tripled, -quadrupled, and so on trails would increase rapidly, so that the original doubled trail would soon be lost among the others, long before the first ant made the trip again. Also, why don't the ants follow the first trail AWAY from home? Has anybody simulated ants with this pheremone-following property to see whether it is sufficient to account for their trail-following behavior?

It is also difficult to explain why more than one ant would ever arrive at a little speck of food if trail-following were the only way of finding it. How did the first ant find it, if not by tracking up the gradient of a scent? And if the first ant did that, why not the second and all the rest? The length of the path taken, it seems to me, would depend on which way the surface wind is blowing. Perhaps we can derive a theorem of ant geodesics from this.

"Information-amplifying" is much too general a term to apply to positive feedback effects. Simply "amplifying information" generally would be nonsensical. What does it mean to increase the information that my telephone number is 272-2731, or that North is to my left? Positive feedback means specifically an effect of a system's action that increases its own tendency to produce MORE OF THAT SAME ACTION (without limit). If one independent system's action increases the action of another one, that alone is not either positive or negative feedback. It is simply an effect. The action of the other system(s) must in turn reflect on the first system's action if there is to be any feedback of either sign. In the case of the ants, it would seem to me that the pheremone-doubling effect would simply increase the loop gain of the trail-following control system by making the lateral gradient of pheremone more pronounced. The trail-following remains a negative feedback phenomenon.

I can see, however, that my earlier blanket rejection of positive feedback inside a single organism was premature. Positive feedback can occur between any two independent systems if the actions of each disturb the other in the right direction. If resistance of one system to side-effects of the other system's action result in increasing the other system's error, the other system will produce more of the same action, and that is a positive feedback loop (involving both systems). At a given level in a hierarchy of control, any two systems are independent of each other in terms of actions on the variables peculiar to that level. Therefore each can disturb the other in this positive-feedback way by acting on the shared lower-order environment, just as independent organisms can do. This can happen if they control input variables that do not represent independent variations in the lower-order world.

In posts a month or so ago, Wayne Hershberger spoke of reorganization as a process of randomly altering the signs of error-sensitivity until all feedbacks became negative. That can explain how a single developing control system adjusts the signs of all its output effects until all feedback effects on its inputs have signs correct for negative feedback. If two independent systems, each capable of negative feedback by itself, interact so as to create positive feedback, then either one system or both must

reorganize so as to control a different input variable more independent of the other, or a higher level of control must come into existence to prevent using the two potentially-conflicting systems at the same time, or to prevent giving them incompatible reference signals. Perhaps this is the mechanism through which higher-level systems come into being.

With regard to the ants, there is nothing in principle that prevents positive feedback from occuring in the relations between independent individuals. So I don't object to your raising this possibility with respect to choice of path. To show that positive feedback does exist, however, you would have to show that there is a closed loop of effects that continually increases the magnitude of the effects. I don't think that trail-following alone is sufficient to produce this positive feedback loop. But maybe it is -- the way to settle the question is to set up a stimulation and see if it behaves as expected, given the assumed rules of behavior.

Best regards -- Bill

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Date: Thu, 3 Jan 91 09:58:40 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Powers' address

To correct Bill Power's 910102, until further notification, personal mail to BIll should be sent to him at either CZIKO@UIUCVMD (Bitnet) or CZIKO@VMD.CSO.UIUC.EDU (Internet) (and not G-CZIKO@VMD.CSO.UIUC.EDU). These correct addresses will now show up at the end of his messages along with his correct identity.--Gary

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Date: Thu, 3 Jan 91 10:09:20 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Behavior of perception

Well, I'm back. There have been quite a few interesting posts. Rather than respond to any in particular I'd just like to muse about some ideas I am planning to start working on. I already described some of this work in the "Perception" chapter of Robertson and Powers "Introduction to Psychology: A Control Theory Approach" (CSG Press, 1990). The idea is this--information about perception should tell us something about behavioral capabilities -- and information about behavioral capabilities should tell us something about perception.

In conventional psychology, studies of the structure of perception and behavior

are treated separately. In control theory, they are two sides of the same coin.
I recently noticed an interesting phenomenon in conventional psychology -- there are hierarchical models of perception and hierarchical models of behavior. Is this just a coincidence? From the conventional perspective it must be; there is no reason to expect perception and behavior to both be a result of hierarchical processes. But, according to control theory, this is no coincidence. Behavior and perception are the same -- if one is hierarchical so is the other. Moreover, it is the same hirearchy. Levels of behavior are levels of controllable perceptual variables.

One important implication of this point of view is that a person cannot "do it" if they cannot perceive it. Limits on our ability to perform certain behaviors should coincide with limits to our ability to perceive these behaviors. A nice, tangible example of this is found in studies of people's ability to produce and perceive sequences. A fellow named Rosenbaum has done a lot of research on people's ability to generate sequential behavior -- like a sequence of keypresses. Based on the pattern of inter-response times involved in generating the sequence, Rosenbaum concludes that the "motor program" used to generate the sequence is hierarchical. Ignoring for the moment the question of how a high level "command" (to produce the sequence) is elaborated into the appropriate subroutines that produce the "responses", there are still some interesting observations that Rosenbaum has made about the limitations of sequence production. For example, errorless sequences of keypresses cannot be made at rates faster than about 4/sec. The same is true for sequences of spoken letters -- the fastest rate is about 4/sec. It turns out that the fastest rate at which you can perceive a visual or auditory sequence of events AS A SEQUENCE is 4/sec. I bet that the same applies
to a sequence of pressures at the finger tips (I have not done this experiment but if anyone knows of a perceptual study of this sort I'd love to get the reference. The study would test the rate at which a sequence of taps to the tips of dufferent fingers can be recognized as a sequence -- ie - pinky, thumb,
index, thumb, pinky, thumb, index, thumb, etc.).

So a perceptual limit seems to correspond to a behavioral limit. I believe that people could not produce an errorless sequence faster than 4/sec because they could not perceive the sequence at a faster rate; and what they are doing when they produce a sequence of keypresses is produce a sequence of perceptions (of finger configurations and sensations). The fact that this behavioral limitation is really a perceptual limitation is also suggested by the fact that people can move their fingers alot faster than 4 presses/sec. Think about trills and "drumming" your fingers. I would like the data but I think the fingers are moving at rates over 10/sec (any data on piano trill rates?). The reason that people can move their fingers faster when trilling than when moving them in a sequence, I believe, is because they are controlling
a different perception -- a transition rather than a sequence. Transitions between configurations can be perceived at very high rates: the visual transition from a dot at one position of the screen to a new dot at a different position on the screen can be seen at rates over 30/sec (as in motion pictures). I'm sure that you can perceive the transition in "taps" to the tips of your fingers as a transition between fingers at rates around 30/sec or higher.

Powers' model of a control hierarchy implies that each level of perception

has a different "integration period" -- the window during which the lower order perceptions are integrated into the higher order perceptual signal. Higher order systems should have longer integration periods than lower order systems. The integration period for sequence perception seems to be about
500 msec (the time needed for two events to be perceived as one after the other). The integration period for transitions seems to be about 60 msec. The integration period for programs and principles should be longer than 500 msec. These are estimates of minimum integration periods. So they also indicate something about the fastest rate at which a perception at that level can be controlled (where a controlled perception is seen as "errorless" behavior). Thus, the fastest sequence that can be controlled is on where no two component events occur in less than 1/2 sec.

If anyone knows of any other perceptual data that is related to performance data then I would love to hear about it. It is not always possible to relate the two classes of data easily -- especially because there are often multiple levels involved in both the perceptual and behavioral data. I would particularly like examples of perceptual/behavioral limits on control of examples of each class of behavioral variable -- intensity, sensation, configuration, transition, event, relationship, category, sequence, program, principle and system concept. I think it shoul dbe possible to find examples of limitations from intensity to program; higher levels may be tough.

Best regards

Rick M.

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Date: Thu, 3 Jan 91 12:41:41 -0600
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From: g-cziko@UIUC.EDU
Subject: Not so obvious?

Powers (910102)

Bill, I agree with what you have to say about how behavior makes sense when we try to understand its purpose, but that doesn't rule of for me the usefulness of some the new physics stuff, such as chaos.

Perhaps the difference is that you are primarily interested in already formed control systems while I am primarily interested in reorganization (my field is education). It is in reorganization that something like chaos seems to be a useful concept. If reorganization is basically blind variation and selective retention, what is the source of the variations? When you push the reorganize button because of chronic error, what system then kicks in to reorganize? Since chaotic attractors can provide such a rich source a variation using a very simple interative function, I would be

surprised if chaos wasn't involved in reorganization.

And if chaos is involved in reorganization, it follows that even two identical control systems which then reorganize will reorganize differently (you may change a reference level, I may develop a new perception). All kids in high school may control for being accepted by a peer group. If there is error (peer rejection), one might turn to the math club, another to a street gang with perhaps no way to know which way each one will go. In chaos theory this is called sensitive dependence on initial conditions (or the butterfly effect) and I can't see how you can rule this out when considering the big picture of a person's lifespan. Sure, once you know that someone is in a street gang, his behavior may make more sense, but why the street gang and not the math club? Some reorganizations may be almost inevitable given the human genome and the usual human environment (we all seem to develop Piagetian object permanence, for example) but others seem much more variable, perhaps even chaotic, as the variation component of biological evolution appears to be.

Because of the unpredictability of reorganization, it seems to me that control theory cannot offer the type of prediction that the other perspectives offer (but still can't deliver). And if this is the case, I think there will always be great reluctance to embrace control theory by most social scientists. Control theory may do better at modeling tracking tasks, but what happens when reorganization gets in the way? Control theory can't model what happens, and neither can any other approach, but at least the other approaches hold on the (vain) hope that someday will know enough to make the long-term predictions.

Why does all this remind me of the uncertainty principle in quantum physics? Is it because control theory is a major paradigm advance which clearly shows us the limits to our knowledge? Are the "traditional" social scientists like Einstein in their belief in hidden variables that will remove all uncertainty?--Gary

P.S. Here's another thought about reorganization. Control theory says that behavior is not important, but perceptions are. But for higher-level systems, couldn't you also say that all lower order perceptions are also unimportant, as long as the higher systems are satisfied? So then, what perceptions are really important? Only the highest-level ones? Or are there so lower-order perceptions that will always remain important, such as body temperature, that cannot be reorganized?

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Date: Thu, 3 Jan 91 13:25:01 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>

Subject: Re: Behavior of perception

>Rick,

>If anyone knows of any other perceptual data that is related to performance
>data then I would love to hear about it. It is not always possible to relate
>the two classes of data easily -- especially because there are often multiple
>levels involved in both the perceptual and behavioral data. I would
>particularly like examples of perceptual/behavioral limits on control of
>examples of each class of behavioral variable -- intensity, sensation,
>configuration, transition, event, relationship, category, sequence, program,
>principle and system concept. I think it shoul dbe possible to find examples
>of limitations from intensity to program; higher levels may be tough.
>
If I understand what you're asking for correctly, I think there might be some ERP work in neurophysiology which could shed some light on temporal limits of lower levels of the hierarchy. I was getting ready to dig up some studies using language and ERPs because ERPs are time-bound, and I want to see what evidence there is for the first 4-5 levels and their corresponding perceptions of "language". The main problem I guess is that while ERPs give fairly discrete temporal records, WHERE the electrical activity comes from and WHAT it reflects is a subject of debate. Anyway yesterday I gave a cognitive neurophysiologist a copy of Chapter 12 of the 1973 book and asked him for comments.

Joel

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Date: Thu, 3 Jan 91 20:53:23 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <CZIKO@VMD.CSO.UIUC.EDU>
Subject: Language

Joel Judd (910102) --

Re linguistic levels: "shape" shouldn't appear at second order, where only quality of sensation is (supposedly) sensed. Sensation is the lowest level that is easy to grasp subjectively -- it takes considerable concentration (or artificial situations) to make intensity obvious (disregarding what kind of sensation it is). In the realm of sound, sensations are things like tones and hisses -- phenomena that have the quality of sounds and not of other senses, such as sights. In vision, you have shades of light and dark, colors, and perhaps edges and corners. Depends on what you can get from weighted sums of intensities. I think edge-enhancement can occur at second order. You have to take the nature of the sensory apparatus into account. In the cochlea, for example, the quality of sound is pitch, because different intensity signals arise from different receptors in the frequency-discriminative spiral organ. The auditory sensations would arise from all the ways the individual frequency-signals could be combined by addition and subtraction. There are edge-enhancers right in the retina, so some sensation signals must be generated even prior to conduction up the optic nerve.

Only at the third level is there perception of shape (visual) or phoneme (auditory). You have handled the 3rd, 4th, and 5th levels just as I would. You might find it interesting to include an analysis of singing at these levels.

The next level is supposed to be "relationships." Since we're in a purely linguistic hierarchy here, the relationships of interest aren't among the meanings of the words, but among the words themselves as linguistic events and objects. This might be the level where words act on each other: a subject requires a predicate, an adjective modifies a noun, an adverb modifies a verb. A relationship is a constraint on the behavior of entities that are otherwise independent.

Next comes category: words are classified into nouns, verbs, and so on -- at least by people who study language. "Man" is a noun. Etc.

Then I suppose we have word- or sentence-element-order, the sequence level where ordering is the variable-type of interest. In English, VP usually follows NP. Then programs, which are networks of choice-points (between which there are only sequences or lists from the next level down). These networks would correspond to the rules of syntax and probably spelling ("if C, then the sequence is ie, else the sequence is ei"). Anything driven by rules that contain branching possibilities. If there are no branches, it's just a sequence, one level down from programs.

When you get above the program level, you introduce things like elements of style and I suppose the stuff Chomsky talks about as "deep structure." Although I think that deep structure is probably not a verbal phenomenon. By the time you get to this level, you're not dealing in words any more. Such things would be principles. And of course the system concept is language -- the French seem to have a very clear system concept of their own language, and refuse to recognize what you're saying if you don't speak it properly. Or so I've heard.

This hierarchy, as I've described it, is purely linguistic -- that is, meaning isn't considered. There is also a hierarchy of non-verbal perceptions, which I maintain is the very same hierarchy: the same operations are applied to words as to nonverbal experiences. Thus it should be possible to show how words, appreciated at a given level, connect to meanings that are experiences at the same level. The implication is that any perception can constitute "language." At that point, wisdom tells me to turn this over to the linguists. I think you understand my approach perfectly well. Anything more you do with it will be new. And interesting.

Best regards -- Bill

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Date: Fri, 4 Jan 91 12:20:31 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: CYBSYS-L Moderator <cybsys@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: CFP: ASC 1991

Just to let y'all know what's going on. . .

O-->
| Cliff Joslyn, Cybernetician at Large, cjoslyn@bingvaxu.cc.binghamton.edu
| Systems Science, SUNY Binghamton, Box 1070, Binghamton NY 13901, USA
V All the world is biscuit shaped. . .

CALL FOR PAPERS, PANELS, AND PRESENTATIONS

CYBERNETICS: ITS EVOLUTION AND PRAXIS

The 1991 Meeting of the American Society for Cybernetics
Amherst, Massachusetts
July 17-21, 1991

The 1991 annual meeting of the American Society for Cybernetics will focus on the balance between the evolution of languages and models within the field of cybernetics, and the development of applications for cybernetics (cybernetic praxis, or doing cybernetics) in other fields. We want to involve people working in many fields who use "cybernetic-like" approaches but who may not consider themselves to be primarily identified with cybernetics as an intellectual tradition. (This may include your colleagues). These fields may range from art and music to management, from family therapy to cognitive science, and from evoultionary systems to education. If we are to think about information / behavior / learning / cognition in complex systems, and to think about the process of thinking about information / behavior / learning / cognition in complex systems (gasp!) we need to spend some time looking at specific systems, through the eyes and hands of people connected with the study and praxis of those systems. In short, the meeting will embody the mutualness in which cybernetic theory can INFORM systemic thinking in different domains, and can, in turn, be INFORMED by everyday practice in those domains. We thus hope to take seriously Gregory Bateson and Margaret Mead's idea of "keeping data flowing through the system", while at the same time question what constitutes "data" and how we construct coherences that allow a body of practice to emerge as such.

We will have morning plenaries that focus on the work of people who work within various specific systems, whose work can be connected by some common cybernetic principles / approaches. These will be followed by parallel sessions in which work in specific areas of cybernetics is duscussed. We want to leave the afternoons free for exchanges and communions of whatever sort, that typically arise at our meetings. At night, we will focus on areas of theory and/or language, useful across systems. All of this will occur in bucolic Western Massachusetts at the University of Massachusetts in Amherst.

We encourage participation in many forms: paper presentations, workshops, panels, multilogues, etc.

DEADLINE: We invite your participation. Proposals must be received by March 1, 1991, and should include: 1) a title and abstract (250-500 words); (2) a description of format(s) (e.g., seminar, performance, paper presentation, panel, etc.), and 3) audio-visual requirements.

ASC STUDENT SCHOLARSHIPS: The ASC Scholarship Fund will provide a limited number of scholarships in order to enable students to pariticipate. For more information, contact the conference

organizers at the address below.

ADDRESS:
Proposals and requests for information should be mailed to:

Frederick Steier
Center for Cybernetic Studies in Complex Systems
Old Dominion University
Norfolk, VA 23529-0248, USA
Tel: (804) 683-4848
Fax: (804) 683-4898

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Date: Fri, 4 Jan 91 11:48:37 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: Language

>Bill (910103),

A VERY helpful post. I'm grateful you find the time to reply. A couple of things to bounce back as I try to avoid chicken/egg type dilemmas:

>The next level is supposed to be "relationships." This might be the level where
words act on each other: a subject requires a predicate, an adjective modifies
a noun, an adverb modifies a verb. A relationship is a constraint on the
behavior of entities that are otherwise independent.

After sending my questions yesterday I remembered a book by Brian MacWhinney concerning language acquisition. They make a case for "competition" in their model of language and language acquisition. One aspect of the model is the idea of "slots", and that idea seems to go along with the level of relationships. The slot is simply a place for a lexical entry (or morphological, or phonological) which is constrained by other slots already filled, or to be filled. In languages such as English these constraints can be fairly rigid; in Spanish or Italian, fairly flexible. There is an ERP component (N400), correlated with semantic relationships, that for English sentences begins with a large amplitude which gradually decreases over the course of the sentence as the lexico-semantic possibilities become more constrained (but which will markedly increase if semantic probabilities are violated). But...it seems that syntactic order falls under a description of sequence:

>Then I suppose we have word- or sentence-element-order, the sequence level
>where ordering is the variable-type of interest. In English, VP usually
>follows NP.

Where VP and NP contain seperate elements in a relationship, but the phrases and clauses themselves are also in a relationship with each other. As I bounce back and forth among levels it's hard to remember that language at a fairly high level is being used to talk about lower levels. It's also hard to try and work out relationship/sequence with experience/language. For example, say one visually perceives 'dog chasing cat'. That is the perceived relationship. Now:

>Next comes category: words are classified into nouns, verbs, and so on --
>at least by people who study language. "Man" is a noun. Etc.

So I have two things [CONFIGURATIONS]: which I label a 'dog' (noun) and a 'cat' (noun) [CATEGORY]. I want to linguistically express the action I see [TRANSITION], so I use a verb: 'chase' [EVENT]. One of the aspects of this verb is that its object is the thing being chased - in active sentence order this thing would be placed after the verb, and the thing doing the chasing is placed before [RELATIONSHIP]. At this point I've got something like 'dog chase cat', about 2-3 year-old language. However, I know (as a mature native speaker) to use an article with nouns such as dog and cat in the singular [CATEGORY -> PROGRAM?], and since in this particular case I am referring to a specific dog and cat I use 'the' (the English constraint is 'a','an','the') [PROGRAM: if NOUN and SINGULAR and SPECIFIC -> 'the']: 'the dog chase the cat.' Now I also know that presently occurring action is typically described with a form of the verb requiring an '-ing' ending and 'be' before the verb [PROGRAM]. And 'be' must be in a present form which "agrees" with subject element 'the dog', and so should be 'is' [PROGRAM]. Now I have an utterance 'the dog is chasing the cat', which corresponds to the visual perception I decided (?) to couch in linguistic terms (although after such an explanation I wonder why I bothered).

Does this make sense? I can see where the perception hierarchy of 'dog' and 'cat' category can easily be the same as the linguistic hierarchy of 'dog' and 'cat' category. But to even answer a simple question like "What's that?" (pointing to a dog) I would probably say "A dog", not "Dog a" or "Dog". So I have to move into PROGRAM to linguistically name a CATEGORY? And what experience is "a" (or 'of' or 'and'...)--linguistic convention? Does the RELATIONSHIP constrain the linguistic item used, and the PROGRAM its order?

I can see, though, how something like 'deep structure' would fit in at the level of PRINCIPLES which require quite a bit of experience (at least several years for L1) with the language to develop reference levels that can judge the programs used in communication. This experience MacWhinney refers to as necessary to distill "prototypical" language from our linguistic experience, which guides our actual language use. Many instantiations of language are necessary for us to derive "prototypical" examples. I think that for language education this is important for at least two reasons: 1) It is unlikely that most people will pick L2 language principles from only a few experiences with/examples of the L2. This bodes ill for the learn-a-language-overnight business; 2) Many language texts have traditionally presented "prototypical" language for students to learn. eg.

```
"THE GREETING": A: Hello!
                B: How are you?
                A: Fine, thank you. And you?
                B: Fine, thanks.
                THE END
```

This seems to be "relative frequency" education rather than something like "possibilities"; that is, what the average person might say at any given time versus what YOU would say at a particular time, which is more like communication.

Enough for now - Joel.

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Date: Fri, 4 Jan 91 13:30:00 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Warning -- RSCS tag indicates an origin of SMTPUSER@UBVMSC
From: Hugh Petrie <PROHUGH@UBVMS.BITNET>
Subject: Re: Behavior of perception

Marken (910103)

I think that Rick Marken's observation that in an important way behavior and perception are the same thing is right on target. Indeed, one might add that the reference signal is also of the same logical type. For a long time philosophers of action theory argued against the psychologists that separating the intention with which an action was performed (the reference signal) from the description of the action (the perception--or the "action" if described in appropriate terms) was illegitimate. They early on noted that behavior, considered simply as "output" was insufficient as a concept to account for what people do. The implicit model seemed to be "action = behavior plus intention." They argued that there is an "analytic" or meaning connection between intention and action--and, indeed, that does make good sense. Where the philosophers fell down was that under that way of looking at things they could not explain how, in some common sense sort of way, the intention does "cause" the action. The main problem, of course, is that the notion of cause they had was the linear s-r notion.

Once one puts control theory into the picture, a lot of things become clear. The analytic connection between intention and action is necessitated bys the fact that the control loop must compare perception and reference signal and thus they must be of the "same logical dimensions". What I think Marken is doing is pointing out to us that "behavior" that means anything is what the philosophers call "action" and that must be the same as perceptual variables. What we see ourselves as doing must be what we are controlling. What we are controlling must be what we intend. At the same time, the control loop provides just the right model for showing how all of this can have causal effects in the world.

For those of you who are interested, I tried to talk about the philosophical action theory part of this in The Dilemma of Enquiry and Learning, pp 89-109. At any rate, I think that this is an extremely important insight and can help us both understand where more traditional accounts have gone wrong and where some of their insights might be reformulated into more adequate accounts.

Cheers, Hugh Petrie

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Date: Fri, 4 Jan 91 13:32:00 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Warning -- RSCS tag indicates an origin of SMTPUSER@UBVMSC

From: Hugh Petrie <PROHUGH@UBVMS.BITNET>
Subject: Re: Not so obvious?

Powers (910102)--The obvious; Cziko (910103)--Not so obvious

Just a comment or two on the obvious or not so obvious. As an educator, I find myself attracted to Gary Cziko's emphasis on reorganization--which seems to be at least one of the main mechanisms of learning (if not the only one). The problem is that even if one believes that reorganization is the only mechanism for learning, that doesn't get us very far. As Gary has said, why the street gang over the math club? Why persist with something hard rather than simply give up and try something else? And so on. Of course, one answer could simply be that the reorganization is always blind, but evolutionary principles will always ensure that those reorganizations which remove error will more likely stay. That response doesn't give much guidance except perhaps to suggest that we educators ought to try to arrange the environment around learners in ways that will help the "right" (preferred by us) reorganizations to be the ones which most quickly remove error and to make the "wrong" reorganizations continue to be least likely to remove error, if by no other means than withholding adult praise, which we assume most children want somehow or other.

Indeed, as I think about it, why couldn't some s - r psychologist take what I have just said and suggest that I am simply talking about reinforcement?

I guess that what I am suggesting is that some time spent thinking about reorganization on more specific levels and how it might be guided by other, higher or lower, levels would be useful, even if we just come up with some possbily testable hypotheses.

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Date: Sat, 5 Jan 91 15:55:48 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <CZIKO@VMD.CSO.UIUC.EDU>
Subject: Linguistics, learning

910105:Bill Powers:linguistics,chaos,learning.

(A suggestion for a sortable one-line header:

date: originator: subject words separated by commas, spaces optional)

Joel Judd -- (910104)

Yes. You're going at this just the way I imagined it would be done. I think you're starting to develop a picture of all these levels operating at the

same time. We have to go back and forth in talking about them because communication in sentences is lineal and attention is limited in scope. WHILE the syntactic program level is stringing sentence-sequences together, the sequence level is ordering category-names, the category level is directing word-relationships, the relationship level is constraining word-events, and so on. And at the same time, the same things are going on at the same levels in a tree that deals in wordless experiences. At each level, the word perceptions are evoking non-word perceptions, which are being fit into a multi-level model (control in the imagination mode). If there's a grammatical error, it is corrected in the linguistic hierarchy. If there's a model error (i.e., the words evoke a moaning -- I mean a meaning -- that clashes with the model), the error leads to reselection of words or reordering of sentences, etc. There are many systems operating in parallel, monitoring and adjusting different aspects of communication, such as spelling, ambiguities, and unfortunate juxtapositions of words (I threw the horse over the fence some hey; the shooting of the hunters awakened me). This is really making me wonder if we need a separate "modeling" level, or if the modeling process is simply what goes on at all the levels when they're in the imagination mode, or partly in it.

Gary Cziko, Hugh Petrie (910104) --

When I talk about behavior, I mean what's created by reorganization. We have to start understanding behavior by analyzing its most obviously regular features -- do the easiest things first. The process of reorganization doesn't strike me as being behavior, because it's not organized on the output side. I don't think that reorganization itself can be taught because there's nothing organized to teach.

What we CAN do is to teach the already-organized system, the person, how to recognize reorganization when it's happening and understand what it means. Reorganization is a state we get into when we experience protracted error. It is not entirely pleasant. Learning is stressful. But those of us who have learned the value of learning have learned to accept the stress, not to remove ourselves from the situation or turn off the effort that is creating it but to endure, knowing that the ultimate result is usually an enhancement of the scope and power of the will. We as teachers can learn how to avoid creating the irrelevant stress of conflicts that have nothing to do with learning. We can teach students the difference between stress that is part of learning and stress that does need to be solved through changes in behavior that avoid it. We can teach them to avoid extremes of stress -- intrinsic error -- that turn reorganization into panic. We can teach them to evaluate the results of reorganization before depending too much on them.

I wouldn't worry about S-R psychologists, Hugh. Let them say it's just reinforcement. Then explain (if it's worth the trouble) that a reinforcer is simply a variable that a person is controlling. It doesn't do anything special to the person; what makes it reinforcing is that the person wants it to happen or exist. What gives it its illusory power is the fact that people will act so as to bring the environment into states that they like; the causal arrow runs the other way. Reinforcement is an effect, not a cause.

Or you could just ignore their suggestions and tell them how it really works.

Gary, I think that students of chaos would take issue with the idea that "chaotic" is just another word for "random." Chaos is apparent randomness, but with an underlying order that is expressed as an "attractor" in phase-space (a rather arbitrary choice of presentation). And the simple iterative processes for creating it aren't all that simple -- the Mandelbrot set calculations look simple until you realize that the variables are complex variables, not algebraic -- manipulating them requires using the rules of algebra that deal with imaginary numbers.

But I don't really object to supposing that the "random" changes involved in reorganization might really result from chaotic processes rather than just random noise-generators (which are the simplest to implement). What matters about the randomness of change, what gives it its power, is that it is not simply an algorithm that conforms to some existing ordering process in the learned hierarchy. This is the only way in which learning can be something other than a permutation of the known. Reorganization must operate randomly WITH RESPECT TO ANY KNOWN ORDERING PROCESS if it is to find a solution to a control problem that NO existing system can handle.

It's true that you can't predict the outcome of reorganization in general. But in specific situations you can: if you know what must be controlled in order to remove a specific kind of error, and if you know all the possible means that will result in control, you can say at least that the final organization will (a) control that variable, and (b) use one of the means that affect it. For each potential means that you can see would work, you can say exactly how the person will act when disturbances alter the controlled variable. The means will be varied so as to have an equal and opposite effect on the variable.

Even when you can predict the final outcome in terms of controlled variables and (a family of) means of control, you still can't say how reorganization will get to the final result from the starting point. It might lead in the wrong direction first, or go immediately to the required system. When Rick Marken and I model the e. coli type of behavior with human subjects, we can adjust the model so that the general form of approach to the goal is the same as the person's. But that means only that the total number of direction-changes is about the same, and the approach to the goal goes at about the same rate. We can't predict the person's behavior move for move, as we can in a tracking task. That is an inherent limitatation in predicting any behavior that has a random component. It's also a limitation when the behavior involves external disturbances or hypersensitivty to initial conditions. I can say (after suitable experimentation) that you will keep a car within two feet of the center of its lane on a ten-mile trip, but I can't predict the detailed deviations. Even if I know all the disturbances, the process of steering involves cumulative errors that are highly sensitive to small perturbations, and I can't integrate the corrective output in the model exactly enough to predict the cumulative errors. All I can say is that they will remain smaller than some calculable limit.

All this means is that models have limits of accuracy. Nothing surprising there.

As to the relative importance of perceptions at the different levels, I think you're right that higher perceptions are more important than lower. A lower-level goal will always be changed by a higher-level system if that will correct higher-level error. But while the particular states of lower-

level perceptions are unimportant from the higher point of view, the presence of SOME equivalent set of lower perceptions is essential if the higher are even to exist, and the lower-level perceptions must behave in a specific pattern if the higher-level perception is to be maintained in its reference state. In having lunch, it isn't important what you eat as long as it's palatable and nutritious, but you can't perceive having lunch if there's NOTHING to eat, or if, given the lunch, you can't manage to get it into your mouth. So we can't just use the dichotomy important-unimportant. We have to ask "important in what regard, and to what degree, and in what context?"

In order for the highest-level perceptions to exist, all the lower levels must also exist and behave properly. We don't reorganize in order to eliminate the need for lower-level perceptions. We reorganize until the whole hierarchy is coherent, with the minimum possible error at all levels.

One last remark on reorganization: I'm with Hugh in thinking that reorganization doesn't happen very often or very fast, although it may always be ticking over in the background at idling speed. The human hierarchy, in its adult state, is vast. Most of it, I think, continues in the same form for a long time, long enough to study it in some detail. While new control systems may be added at any time and at any level, I think that most of the reorganization (during maturation) is going on at the highest levels. The changes at the highest levels result in a lot of changing of reference signals at lower levels, but not in any changes of organization. A mere change of goal doesn't imply reorganization -- it means only that some higher system has altered its output.

The most profound reorganizations are those that bring new perceptions into being or alter old ones. These are the reorganizations that govern what any subsequently-formed control system is going to control. Reorganizations on the output side merely implement the control. The output reorganizations may be much more common than input reorganizations -- and I think they can be both frequent and rapid. Look how fast you can lose -- and regain -- the ability to carry out a skill, even when you know what you want to accomplish. We live in an environment with widely varying dynamic characteristics, so our output processes have to adapt rapidly. You walk to your car, drive it to the airport, and fly your own plane away, all using the same musculature but with extremely different dynamic stabilization parameters. The output adaptations are so rapid that they're probably not even classifiable as reorganization. Petar Kokotivic probably has models in which these changes are algorithmic, not even random. But reorganization of perception is a much slower process, and so, I suspect, is the creating of comparators and properly hooking up their error signals to the right lower-order systems.

Once we have learned a new kind of perception, do we EVER change it again? I don't know the answer.

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Date: Mon, 7 Jan 91 21:44:35 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Skinner's Swangsong

All seems remarkably quiet in the net today. Perhaps this is the result of New Year's resolutions to spend less time in front of computers and more with real people. Maybe not a bad idea.

I thought I'd advantage of the lull to ask if anybody knows anything about the paper Skinner was working on before he died. This was referred to as his swansong in the NPR interview I heard last year where he mentioned that the paper would argue that he did for the behavioral sciences what Darwin did for the biological sciences (then why do I like Darwin's theory but not Skinner's?).

I know that Bill Powers is preparing an article about SKinner for the mid-February CSG Newsletter (which will be mailed to CSG members with paid up dues: hint) so maybe he knows about the status of his last writings.--Gary

P.S. Annual dues to the Control System Group are $25 and should be sent to Mary Powers, 1138 Whitfield Rd., Northbrook, IL 60062. Dues for students are just $5. Make checks payable to the Control Systems Group, Inc.

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Date: Tue, 8 Jan 91 10:17:34 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: More hierarchy

Yes, Gary, it has been rather quiet on the net. Maybe its the general apprehension about the "war" deadline, or the apparently depressing state of the economy. Or, maybe its just winter or the drought.

I would like to say thanks to Hugh Petrie for the nice comments about by post about the equivalence of perception and behavior. I am relatively excited about trying to write the paper about this stuff. Perhaps I will experiment with posting segments of the paper on the net for pre-publication comments/suggestions. What do you (CSGnetters) think?

One of the points I plan to make about hierarchical models is that these models make sense for perception but they seem kind of forced for behavior (if one thinks of behavior as output). It is reasonable to think of a higher order perception (like a word) as a function of lower order perceptions (phonemes). But why generate output "hierarchically"? For example, Rosenbaum has a hierarchical model of sequence production that says that the commands for producing elements of the sequence are issued only when a signal transverses a hierarchical structure to reach that node. The length of the segment transversed accounts for the time between occurance of elements of the

sequence. But why go through all this trouble to generate the elements? Why would the nervous system work this way?

Well, of course, it doesn't work this way. But there is a hierarchical structure to behavior. But it results, not from some desire on the part of the nervous system to generate "structured programs" with lots of subroutine calls. It results (I argue) from the hierarchical structure of the process which generate the perceptions that are controlled. The hierarchical structure of behavior results from the hierarchical structure of perception.

More later -- now back to work.

Best regards

Rick M.

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Date: Tue, 8 Jan 91 12:44:18 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Language, learning, doubts

Powers (910105):

I've been following with interest your discussion with Joel Judd on applying control theory to language competence and behavior. But I am sort of stuck on one point.

I can certainly see why control theory is needed to account for the speaking behavior, that is, the actual production of vocal (or written, or signed) language. But I'm not sure what the case is for the higher levels you two have been discussing.

For example, If there is a level in the hierarchy of language responsible for getting the words in proper order (syntax) and if this is done and set up in the brain before the actual words are produced behaviorally, why is negative feedback needed at this level? If the eyes don't need negative feedback because there are no (usual) disturbances to their movement, then why would the higher components of language knowledge? Why does language need to be error driven? What is going to screw up the order of words of my planned sentence before I actually utter it? I can understand that I may be producing language in order to satisfy some nonlinguistic need (like telling my kids to clean up their rooms) and that there are many variable ways of expressing this desire to my kids and I will stop bugging them only if the rooms are cleaned, but I'm not sure why I need control theory for the planning (or computation) any INDIVIDUAL sentence.

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Concerning your comments on reorganization, learning and prediction, I just need more time to think about it all. But here is a reaction to your saying:

>As to the relative importance of perceptions at the different levels, I >think you're right that higher perceptions are more important than lower. A >lower-level goal will always be changed by a higher-level system if that >will correct higher-level error. But while the particular states of lower->level perceptions are unimportant from the higher point of view, the >presence of SOME equivalent set of lower perceptions is essential if the >higher are even to exist, and the lower-level perceptions must behave in a >specific pattern if the higher-level perception is to be maintained in its >reference state. In having lunch, it isn't important what you eat as long >as it's palatable and nutritious, but you can't perceive having lunch if >there's NOTHING to eat, or if, given the lunch, you can't manage to get it >into your mouth. So we can't just use the dichotomy important-unimportant. >We have to ask "important in what regard, and to what degree, and in what >context?"

But I suppose my point was that even having lunch can become unimportant if a higher level says so. Indeed, for someone participating in a hunger strike or going on a crash diet, any lunch at all would consitute an error signal at the higher level. Not having lunch then satifies the higher level, but lower level errors are sure to be there. I suppose that this is a sure-shot recipe for stress. (Although I now remember reading that victims of severe malnutrition stop feeling hungry after a while, they just become very weak. So I guess there a sort of reorganization is possible for even very basic reference levels.)

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Finally, I've been having some indecent thoughts lately about control theory, and so I'd like to confess. I'd like some reactions to help me drive these thoughts from my mind (and thereby complete the conversion process) or at least let me know what my penance should be. These thoughts go something like this:

Control theory says that people do what they do so that they get what they want. Why do they want what they want? Some of it is just part of being animal (e.g., need for food, shelter, sex, etc.). But then again even some of these basic animal needs may be disregarded, at least for a while (e.g., hunger strikes, diets, celibacy, sleeping out under the stars). Other needs are more complex, but control theory can't tell me what these will be. THE TEST may be able to tell me what someone is controlling for at a given time, but this can change at any time as a result of chronic error (which cannot be seen from the outside) which triggers reorganization. Since reorganization is randomly driven, control theory can't tell which new needs will emerge or what new perceptions will develop. Control theory consequently plays down the traditional roles of science, i.e., prediction and control. But it does offer understanding of human behavior. Yes, it says that people do what they do so that they get what they want . . . --Gary

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <CZIKO@VMD.CSO.UIUC.EDU>
Subject: Sequence perception, reorganization

Gary Cziko -- (910108)

>For example, If there is a level in the hierarchy of language responsible
>for getting the words in proper order (syntax) and if this is done and set
>up in the brain before the actual words are produced behaviorally, why is
>negative feedback needed at this level?

Sorry, the warranty on this theory has expired. It's too late to return it for a refund. You are just going to have to try to fix it yourself, or go out and shop for a better one. Read Consumer Reports next time.

I can't (in other words) come up with a slick answer. All I can do is ask "How do you know when the sentence you're producing or planning has the words in the right order?" It seems to me that you have to PERCEIVE this in order to know it. You have to see whether the perceived word-sequence evokes non-verbal meanings in experiential sequences that can plausibly occur. "I filled the glass with water and then turned on the faucet." Oops. This means that the level in charge of producing sequences must alter its outputs until the correct sequence is perceived -- whether you're planning, speaking, or writing. This can go on, of course, at very high speed in the brain's parallel processors.

When I think of higher-level perceptions, there's an image that sometimes seems to help. Think of a court reporter typing away as the proceedings go on. The keystrokes are leisurely, because each stroke produces a symbol that stands for some group of sounds, a prepackaged sequence that is represented by a single keystroke. When I think of a perceptual signal at the sequence level, I don't see the signal as itself being a sequence. The signal says that the sequence it represents has occurred or is in progress, just as the court reporter's keystroke does. The keystroke itself isn't a sequence: it's a signal THAT a given sequence is occurring.

Sequence-recognition is part of the machinery of the perceptual function; the machinery isn't represented as a perception. It simply generates a signal whose magnitude depends on the resemblance of the current sequence of inputs to the sequence that this system is designed to recognize. When we experience that signal, we get the sense that a familiar sequence is going on. We get the sense of "sequenceness."

This seems at first to be terribly restrictive -- it sounds like saying that we really don't experience sequences. But if you try very hard to see what the difference is between "man bites dog" and "dog bites man", you will see that the difference is due to the fact that one sequence is not the same as the other -- but just WHAT the difference is is almost

impossible to express. You can say "Well, in the first instance 'man" occurs BEFORE 'dog', but in the second 'man' occurs AFTER 'dog'." But what is the difference between "before" and "after," perceptually? That's the very thing we're trying to understand, isn't it? We don't seem to be able to avoid begging the question.

The same is true at any other level. You can see perfectly well that something colored blue looks different from something colored red. But when you try to pin down just what the difference is, you're stuck. They're simply two different impressions, as if they were occurring in different places. There isn't any qualitative difference between them except the very difference you're trying to explain.

This strange difficulty in describing how different perceptions are different is consistent with saying that all perceptual signals are alike, no matter what the kind or level of the perception. The only way in which a given perceptual signal can change is in magnitude (really frequency), where the magnitude says how much of the particular perception is present. This idea is consistent with what we find in the nervous system; no matter where you put the electrode, all you can measure is a signal made of blips occurring faster or slower. All neural signals are alike. They have meaning only because of the computing functions that make them dependent on other neural signals.

This way of modeling perception is the only way to avoid the Little Man In The Head or the Infinite Regress problem. If perception of sequence resulted in a signal that itself was a sequence of somethings, then we would have transformed the input sequence but we wouldn't have recognized it. We would still need a sequence-recognizer to tell us what sequence is present in the signal. In order to perceive anything, we must end up with a single signal that represents the judgment "Yes, the something that I am designed to recognize is in fact occurring." That signal can be passed on to other systems that can use the information THAT the perceptual something has occurred, but are not themselves organized to carry out that kind of perceiving function. If you try to explain perception in terms of signal PATTERNS, you end up with a mess, infinite regress.

Everyone who tries to model perception ends up with this picture, I think. In a perceptron that is trained to recognize an "A", you don't get an output that looks like an "A". You just get a signal when the "A" is present and none when it's not, or when something else is present. If something else is present, you get an identical signal, but coming out of a different part of the physical perceptron. I'm sure that Frank Rosenblatt understood this. He understood many things that were lost when he became the victim of others' academic competitiveness.

When we model perception for imitating tracking behavior, we posit a function that can produce a signal proportional to the spatial location of the cursor and the target. But those signals don't change their positions as the cursor or the target move. They have no "position" of their own. They represent judgments, indications of the state of a position variable. If you could see only the signal and didn't know where it came from, you wouldn't know that it represents position instead of temperature or the closing price of AT&T.

I often say that perceptual signals "represent" something outside the organism. That is probably miscontrued more often than it's understood as I

mean it. At the lower levels, I mean that the magnitude of the signal is an analogue of the magnitude of some attribute of the external world (as we would measure it with instruments). At the higher levels, I mean that the signal indicates THAT the variable is present (and to what degree). I never mean that the signal looks like the variable or is in any way isomorphic to it. A program perception is a signal representing the fact that a given program is in operation. It isn't a program.

There is obviously a lot that I don't know about how a sequence-recognizer would be built to produce signals of this sort. I made a feeble guess about how a neural network might be set up to recognize an event on the basis of sequentiality (before I had separated events from sequences), on pp. 143ff in BCOP. I didn't try to guess how the output function of a sequence-controller would have to be organized. Solving this problem (and most others having to do with human perception) is going to require the work of a lot of people cleverer than I am.

On Reorganization --

If people were constantly reorganizing so that no stable properties were ever visible, we wouldn't have much by way of organized behavior to study. I don't think that's the case. I think that by far the largest part of behavior is highly organized and stable. But don't forget that higher-level systems are very complex. If a person is simultaneously controlling for ten programs while trying to satisfy 20 principles in service of half a dozen system concepts (some of which may be in conflict), you are going to get extremely complex behaviors. You might easily mistake the interplay of error-corrections among these concurrent systems for reorganization, when actually each system is perfectly orderly. It's too easy to drop details of behavior that we don't yet understand into the "reorganization" bucket.

When an important high-level reorganization does occur, the result is likely to be a long period during which all control systems in the whole hierarchy go through a process of finding a new equilibrium. During this process, the details of behavior may go through long chains of changes. But if we understood the whole system, we would see that each adjustment is perfectly logical in terms of the perturbation that has occurred, and in terms of interactions among the many control systems at the many levels that have been disturbed. Just think of what happened in your own system the first time you understood the real meaning of "sexual intercourse." Such major insights naturally lead to a cascade of reinterpretations and adjustments of other meanings, but I don't think that much reorganization is involved. It's just that the whole system has to adjust its reference signals in the light of this new way of seeing things.

Of course I simplify. One reorganization may lead to problems that kick off others. But if we learn to see control systems for what they are, we can also learn to see the effects of reorganization more clearly, and separate them from changes that are not really reorganization, but simply higher levels of orderly control going on in many systems at once.

Now that you have confessed, I will assign you your penance. You are doomed to spend the rest of your life explaining control theory to people who will not understand you. Somewhat like Cassandra, who was given the gift of prophecy and the curse that no one would believe her.

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Potpourri

Bill Haley -- (910108)

Thanks for the expression of interest in my hierarchy paper which is,at the moment, little more than a figment of my imagination. I do hope that this imagined tome does not become my "tomb" -- but galleys ye shall receive as I dream them up. By the way, who are you? Are you still doing any work with the Comets? (In case you are much younger than I, that is a reference to Bill Haley and the Comets, the group that made "Rock Around the Clock" a hit).

Joel Judd (910103)

>If I understand what you're asking for correctly, I think there might be
>some ERP work in neurophysiology which could shed some light on temporal
>limits of lower levels of the hierarchy.

Joel -- Thanks for the suggestion. Is ERP something like an evoked potential? Any easy reference materials I could look at. Thanks. RM

Bill Powers (910108)

Of course, the Cassandra image is perfect. Control theorists are indeed the modern day Cassandras. But what did we do to deserve it. My wife, a mythology buff, will love this image of her lunatic husband.

More hierarchical thoughts:

I think one of the best evidences of hierarchical organization in behavior comes from experiments that show one (or more) control systems operating within the time frame of other control systems. This was the beauty of some of Bill Powers' "portable demonstrator" experiments. The simplest is when E's hand pushes down on S's hand to signal S to move his/her hand down from a fixed position. S's initial reaction is ALWAYS an upward push before downward
acceleration -- the position control system reacts to the disturbance to position before the higher order system can treat the disturbance as signal to change the reference for the position control system. The same kind of phenomenon occurs in the experiment Bill and I report in Hershberger's Volitional Action (the "Levels of Intention" chapter).

What I would like to have is a computer demo which makes it possible to monitor two levels of control continuously. It would be best if there were two higher level systems using two (or three) lower level systems to achieve their changing goals. The two levels of control could be revealed by continuously measuring the impulse response to a disturbance of each variable. The impulse response should be faster for lower than for higher order variables. The program could use Bill's method of continuously extracting the impulse response from the closed-loop behavior.

Any suggestions about how this might be done?

Best regards

Rick M.

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: dreaming

After rereading p. 225 in BCOP, I'd like to lay down on the CT psychoanalytic couch a second and ask about something I might have missed in the explanation.
Lately, I have been frustrated (again) by my inability to compose music to go along with things I've written or would write that convey my feelings and expriences. But on at least three occasions I have "vividly" dreamed composing an appealing melody, and even playing it on piano or whatever. I know that the melody was not just a figment of my perception because on one of those occasions I woke up with the melody, could hum it, but foolishly went back to bed without working it out and writing it down. Is this the reverse of the feasibility example given in the book? Why can't I come up with such things while I am awake?

Joel

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Date: Wed, 9 Jan 91 14:50:32 -0600
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: environment/reference level

Hello everyone! I've been dying to get back on this network for the last three weeks. All last semester I read the posts on this net and spent most of my time rearranging my thinking (which wasn't so difficult having only been a "behaviorist" for a year) and determining various applications and implications for psychology and life in general. For the most part, "everything fit right into place," but on the day after I left for vacation I thought of something which has been driving me crazy ever since.

I'll use the illustration that made me think of this problem--I've thought of many other examples since. OK, the idea that the environment acts as a disturbance and not a stimulus is clear and makes complete sense to me instance after instance. But I think that picture is too narrow (I hope I'm wrong). For instance, one day I'm in the Hallmark store buying Christmas cards. The only goal on my mind (that I'm aware of) is buying cards. I of course have other goals which allow me to stand there and breathe and whatever else. But the one thing I'm not thinking about is

women UNTIL a beautiful blonde woman of 19 walks by. Now I have a new reference signal--a few in fact, most of which I am all too aware of. This woman was not a disturbance (except in relation to the goal of quickly buying Christmas cards)!

Will someone please explain to me how control theory can explain that. It's just not the same as following a cursor or driving a car. Now maybe really high on the reference levels I had this goal in mind but this woman's non-presence was a sort of disturbance--I can't attain it if I'm not aware of it.
Or maybe the upper reference level that leads me to look for Christmas cards can be better satisfyed through this woman. For example, maybe I have the goal of making people happy and I know that I could find more success in this endeavor with this woman than all the Christmas cards combined will provide.

Neither of these explanations are satisfactory to me. I see this as a serious problem that I imagine you've noticed before. What's your solution? Reduce my error, please. If by chance you've been discussing this for the last three weeks, I'll find out when I go read my mail now.

--Mark Olson

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From: m-olson@UIUC.EDU
Subject: response to chen

(I sent this as a reply to you, Chen, but it came back at me so I'll send it through the network.

>Last month you mentioned a book "Cosmic Coincidences" by John Gribben.
>Can you tell me more details? I can't find it in my library.
>Is it an academical book in cosmology?
>The publisher, date, what it talks about etc.
>Thanks.
>
>Chen,
I'm responding three weeks later. Gribben is the same guy who wrote "In Search of Schroedinger's Cat" and "In Search of the Big Bang" and alot of others of the same slant. I wouldn't call it an academic book in cosmology since you can get it at Waldenbooks at any shopping mall, but I did read it for a class.

Gribben, John, and Rees, Martin (1989). Cosmic Coincidences. New York:

Bantam.

Do you still want to know what it talks about (since it's been 3 weeks)?

--Mark Olson

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From: marken@AEROSPACE.AERO.ORG
Subject: Good Questions

Joel Judd - (910109)

Just a shot at an answer-- my experience is that dreamed solutions (like your melody) are quite "real" in the sense that they are perceptions you can produce for yourself like other thoughts; and that's why you can write them down or work on them if you wake up; they are "real" imaginings. The prolem is that they ALMOST always seem like greater solutions to the dream problem than to the one your are really trying to solve in waking life. If you had writtten down you dream melody you might have been disappointed with it when you were awake. This often happens to people who try to preserve "profound" dream imaginings. I have had great ideas for experiments or demos in my sleep or as I was dropping off, wrote down the main concept and then found it was not so hot when I examined it in more detail in the morning. Your dream might have solved the idea that a melody is a feasible solution to your communication problem, it just didn't necessarily give you THE melody that would solve the problem (produce the desired perception). Of course, sometimes dream imaginings do solve or contribute to the solution to a problem. The instance I am familiar with is the chemist Kekule (sp?) who claimed to have dreamed of a snake biting its tail while he was struggling (during waking) to determine the structure of the benzine molecule. The snake image from the dream led to his waking realization that the structure was a ring.

Mark Olson (910109)

I asked Bill Powers the same question (but it was candy, not girls that were the releaser) many years ago. There are many possible ways to come up with an answer; for example, it may be that there is always some small level of intrinsic "sexual" error. When a pretty girl goes by, systems involved in controlling that intrinsic error (systems that are part of the learned perceptual control hierarchy) start to do their thing -- preparing you to talk, orienting your gaze, secreting preparatory hormones in case there is a bed nearby, keeping you where you are because you're already going steady, etc. But, I admit, it is an interesting problem -- why do certain perceptions (food, sexy pictures, whatever) seem to create error (and behavior to deal with it) when there was none before. It seems like

stimulus - reponse behavior. Well, maybe it is; maybe we will need to add something to the model that gives certain perceptions the ability to start control processes. We may be talking here about an attention phenomenon -- and attention is something that has not been delt with well in the model yet. I still don't know, for example, why controlling two independent perceptual variables should be any harder than controlling either one individually -- but sometimes it is.

Since that (and many other) talks with Bill about the model I have learned an answer to the "how does the model deal with that?" question that I really like and recommend to everyone -- "I don't know".

Thanks for the questions; they are as much or more fun than an answer.

Hasta Luego

Rick M.

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From: g-cziko@UIUC.EDU
Subject: Re: Sequence perception, reorganization

Bill Powers (901008)

I appreciate the comments on the perception of sequence of perception and the court recorder analogy. The more ways I can think about this the more it helps my understanding.

But I'm still not sure why I need perception and negative feedback for mental planning when there are no disturbances. You said:

>All I can do is ask
>"How do you know when the sentence you're producing or planning has the
>words in the right order?" It seems to me that you have to PERCEIVE this in
>order to know it. You have to see whether the perceived word-sequence
>evokes non-verbal meanings in experiential sequences that can plausibly
>occur.

I can know that I put the words in the right order because that's where I put them and nothing has changed them. Think of your paper with Tom Bourbon that he presented at the August meeting. Planning is just as good as control theory if there are no disturbances. Somehow I need to find a reason to worry about disturbances before I can convince myself that control theory is needed. As I said, if the brain doesn't need propioceptive feedback from the eyes because there are no disturbances, why should these higher mental processes? Perhaps the disturbances are just

caused by the background noise of neural processes or perhaps competing processes. Then we would need negative feedback.

Maybe I'm just being thick. But I think we need to convince people of disturbances before we can sell control theory. What good is the medicine if there is no sickness?--Gary

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <CZIKO@VMD.CSO.UIUC.EDU>
Subject: Didn't know I wanted it til I saw it

Mark Olson (910109) --

Rick's answer ("I don't know") is the right one, but I'll offer some other possibilities. The phenomenon to be explained could be characterized as "I didn't know I wanted it until I saw it."

One possible explanation derives from what I call the "Giving-Up Effect." I first came across it when trying to model operant conditioning. J. E. R. Staddon collected some data from many sources showing that animals on a wide range of schedules behave like control systems when the schedules are easy to moderately hard, but change smoothly to a different mode for the most difficult schedules (least food per press of the bar). For the easiest schedule (Referring to a figure of Staddon's, experiment by Motherall), the average animal presses about 200 times per session to obtain 200 rewards (1:1 ratio). By extrapolation, the free-feeding rate would be about 210 rewards per session, which we can take as the reference level for food input. As the schedule gets harder and harder, the rate of rewarding drops until the animal is pressing 3000 times per session to get 75 rewards per session (40:1 ratio). The error is then 125 rewards/session. Then as the ratio is increased further, the reward rate drops smoothly to about 8 or 10 rewards per session, the error rises to about 190 rewards/session, and the pressing rate DROPS to about 1500 presses per session. This is at a ratio of about 160 presses for 1 reward, a ratio of 160:1. These numbers were obtained by measuring a figure in Staddon's book with a millimeter scale, so they're very approximate.

When behavior is having the least amount of effect on rewards, so the error is nearly total, the animal seems to give up. If we start with the largest ratio (where the error is largest) and progressively make the ratio smaller, the animal responds to an increase in rewards (and thus a decrease in error) by INCREASING its behavior rate. I was worried about this kind of data, in other experiments, because it doesn't fit control-system behavior. Staddon's collected data showed that the "wrong" relationship only holds under an extreme schedule, where behavior is almost completely ineffective in producing rewards (animals would starve to death if they could obtain

food only under these extreme ratios).

So as the schedule gets easier, the reward rate AND the behavior rate rise, even though the error is getting smaller. This continues to be the case until the error has declined enough, and the reward rate is perhaps 40 per cent of the free-feeding rate of 200+ rewards per session. Then, as the schedule gets still easier and the error drops further, the behavior rate also begins to drop with a drop in error, as it should for a normal control system.

I modeled this by establishing a second-level control system that sensed the cost - benefit difference. Costs were weighted presses, benefits were weighted rewards -- both averaged over time. When costs rose above benefits, the error sensitivity of the behavioral (food-input controlling) control system was made to decrease. This ad-hoc model sufficed to match the model's curve to all the data that Staddon had collected, quite closely. The loop gain for the highest-error condition was about 5, and rose to about 20 when the normal control region was reached, at a ratio of 40:1. It remained in the 20s for the rest of the data points.

While there are many ways to model this effect, the net result is that the error sensitivity of this kind of control system depends on the effectiveness of the behavioral action in controlling the input variable. The less effective the behavior, the lower the error sensitivity. This makes sense, in that such a system doesn't waste its efforts in an environment where they have little effect on input. In that sort of environment, the basic underlying control system still receives its normal reference signal, and experiences a large error signal because there is no matching input, but it produces hardly any output because the error sensitivity, the output gain, is so low. The output isn't zero -- I'm sure you gave the 19-year-old a communicative glance, at least -- but it's not what you call a full-court press. The error remains large and the output remains small until something changes to reduce the excess of cost over benefit. As action becomes more effective, the gain of the control system rises.

I learned later, after putting this model together, that there's another reason for the drop in behavior rate despite an increase in the error. The animals in this kind of experiment, according to an expert, don't actually spend all their time in the vicinity of the bar. The higher the ratio of presses to rewards, the more time the animal spends on other activities -- nosing about the cage, running in a running-wheel. And of course, because bar-pressing is the experimental variable, the timer keeps running and the behavior is still counted as bar-pressing (!). In my original model, the higher-level system just reached in and turned down the output gain. In another model, it could instead simply switch more and more often to a different control system, as the error gets larger. Over the course of an experimental run that lasts for one or more hours, the result would be the same, a drop in APPARENT error sensitivity in one system being used at less than 100 per cent duty cycle.

There's another possibility that I haven't really pursued, but is worth thinking about. In the lowest-level spinal tendon-reflex control systems, reference signals enter comparators with a positive sign and perceptual signals with a negative sign. Farther up, in the brainstem systems, many of the inputs from higher systems that look as if they should be reference signals enter the loop with a NEGATIVE (inhibitory) sign, while the

perceptual signals are excitatory -- positive. A large reference signal still calls for a large perceptual signal, but the signs are inverted from the "normal" way I draw control systems. This means that there is an extra sign inversion somewhere else in the loop, possibly the external part.

The interesting property of control systems with these signs inverted is that they cannot be brought into action by an increase in the reference signal. An inhibitory reference signal, after all, can only inhibit. The frequencies of neural signals can't go negative. In the absence of a perceptual signal, there is no error signal, no matter what the setting of the reference signal. So systems formed in this way simply do not act unless there is something that causes a perceptual signal to reach the comparator with a magnitude greater than that of the reference signal. When the lower-order or external situation is such that there is nothing to provide a perceptual signal, the control system will simply do nothing. It will generate action only to the degree that the perceptual signal exceeds the setting of the reference signal -- when there is more excitation than inhibition. Then a perfectly normal control action will begin, keeping the perception at the same level as the reference signal for any further tendencies of the perceptual signal to increase. To an external observer, the effect will be that of a "threshold of response" to anything that provides the relevant perceptual signal.

When I was surveying the neuroanatomical literature, long ago, I thought I saw some evidence that there is an alternation in modes of control from one level to the next -- positive reference signals at one level, negative at the next, and so on. But I could be wrong about that. Someone who is more up-to-date in that field will have to check this before we can believe it. I didn't try the negative reference signal concept in my model of operant conditioning. I'm not even sure it would help explain the phenomenon. At least this sign-inversion concept does one thing -- it shows how higher systems can turn a lower control system OFF.

The next time you go to buy greeting cards, ask yourself if what you want isn't really that 19-year-old blonde, but as the effectiveness of the usual efforts wouldn't accomplish much with no blonde in sight, you aren't trying very hard to correct the error.

p.s. The figure is Fig. 7.18, p. 214, in Staddon, J. E. R; "Adaptive behavior and learning," Cambridge: Cambridge Univ. Press (1983). The model will fit either curve with just a change in one reference signal for food input.

Bill Powers 1138 Whitfield R., Northbrook, IL 60062
cziko@uiucvmd (Bitnet) cziko@vmd.cso.uiuc.edu (Internet)

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Date: Wed, 9 Jan 91 23:12:04 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <CZIKO@VMD.CSO.UIUC.EDU>
Subject: word order

Gary Cziko (910109) --

My basic answer to your questions about needing control when there's no disturbance is: should we use one model when disturbances don't happen to be present, and a different one when they do? If a disturbance is slowly

varying, do we switch models at the moment it passes through zero? And then back again?

>I can know that I put the words in the right order because that's where I >put them and nothing has changed them.

How do you know that "that's where I put them?" Answer: you perceived them where you put them. "Putting" is a feedback process whereby some unsensed output process (that would normally create speech or writing) is short-circuited into the perceptual channels so you perceive it as if it had really happened. You experience only the perceptual signals, not the output that does the putting. This is supposed to be a parsimonious model: there is only one perceptual system that underlies all experience, whether it comes from outside or is generated internally.

The sense of "willing" a certain order, which is pretty vague in me and possibly in others, is all we get of the reference signal -- except the feeling that the order we perceive is "right." Right = no error. It also implies the possibility of "wrong."

I often think up sentences, look at them, and realize that I could improve the order. For example, I often put "only" too early in a sentence and have to revise it before committing it to paper or screen. Do I mean that he is the type of person that only a mother could love, or that he is the type of person that a mother could only love?

My basic answer, in the first paragraph, is also based on parsimony.

Bill Powers 1138 Whitfield R., Northbrook, IL 60062
cziko@uiucvmd (Bitnet) cziko@vmd.cso.uiuc.edu (Internet)

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Date: Thu, 10 Jan 91 13:10:37 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: environment/reference levels

I sent the following message yesterday but as far as I can tell it didn't get sent--I didn't get the little message from wherever that I normally get which lets me know the message was sent. I apologize for sending it a second time if it did in fact get through:

Hello everyone! I've been dying to get back on this network for the last three weeks. All last semester I read the posts on this net and spent most of my time rearranging my thinking (which wasn't so difficult having only been a "behaviorist" for a year) and determining various applications and implications for psychology and life in general. For the most part, "everything fit right into place," but on the day after I left for vacation I thought of something which has been driving me crazy ever since.

I'll use the illustration that made me think of this problem--I've thought of many other examples since. OK, the idea that the environment acts as a disturbance and not a stimulus is clear and makes complete sense to me instance after instance. But I think that picture is too narrow (I hope I'm wrong). For instance, one day I'm in the Hallmark store buying Christmas cards. The only goal on my mind (that I'm aware of) is buying cards. I of course have other goals which allow me to stand there and

breathe and whatever else. But the one thing I'm not thinking about is women UNTIL a beautiful blonde woman of 19 walks by. Now I have a new reference signal--a few in fact, most of which I am all too aware of. This woman was not a disturbance (except in relation to the goal of quickly buying Christmas cards)!

Will someone please explain to me how control theory can explain that. It's just not the same as following a cursor or driving a car. Now maybe really high on the reference levels I had this goal in mind but this woman's non-presence was a sort of disturbance--I can't attain it if I'm not aware of it.
Or maybe the upper reference level that leads me to look for Christmas cards can be better satisfyed through this woman. For example, maybe I have the goal of making people happy and I know that I could find more success in this endeavor with this woman than all the Christmas cards combined will provide.

Neither of these explanations are satisfactory to me. I see this as a serious problem that I imagine you've noticed before. What's your solution? Reduce my error, please. If by chance you've been discussing this for the last three weeks, I'll find out when I go read my mail now.

--Mark Olson

Educational Psychology 210 USmail: 405 South 6th St. #4

College of Education Champaign, IL 61820
Univ of Illinois at Urbana-Champaign
phone: (home) 351-8257 e-mail: (Internet) m-olson@uiuc.edu
(office) 244-8080 (Bitnet) FREE0850@uiucvmd

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Date: Mon, 24 Dec 90 14:57:17 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti1@VUB.VUB.AC.BE>
Subject: greetings

I guess I am the only one who will read emails everyday, including holidays, as long as I am in Brussels.
So I wish everybody:

MERRY CHRISTMAS & HAPPY NEW YEAR.

Chen

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Date: Fri, 11 Jan 91 08:41:51 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Re: greetings

Chen:

I just received this message today, January 11. There have also been some CSGnet messages returned from your address which I will send again.

It seems as if your computer is having technical difficulties with email.--Gary

>I guess I am the only one who will read emails everyday, including holidays,
>as long as I am in Brussels.
>So I wish everybody:
>
>MERRY CHRISTMAS & HAPPY NEW YEAR.
>
>Chen
>

Gary A. Cziko Telephone: 217/333-4382
Associate Professor FAX: 217/333-5847
of Educational Psychology Internet: g-cziko@uiuc.edu
Bureau of Educational Research
1310 S. 6th Street-Room 230
Champaign, Illinois 61820-6990
USA

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Date: Fri, 11 Jan 91 12:25:38 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: hello?

To everyone,

Is anybody out there? Is everyone taking the week off from CT?

Bill Haley: I assume your message was all in fun and not a dismissal of the problem. If no one appreciates my attempt in bringing in "everyday" examples, I can type up some boring ones which illustrate the same problem. I realize that hormones and opportunity play a major role here--now tell me how that works. What's the connection between the environment and the reference signal.

Gary Cziko: You might be interested to know that I'm taking MAT 488, Chaotic Dynamical Systems this semester. Something which is of interest: chaos and randomness are not the same concept in mathmatics.

Anxiously awaiting a reply,

--Mark Olson

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Date: Fri, 11 Jan 91 15:42:29 +0100
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <arti1!chen@MCSUN.UUCP>
Subject: email, obsession

My email connection had been down for two weeks. At last I saw today the greetings I sent on 24 Dec. 1990. Isn't it fun?

Gary Cziko, Bill Powers:
The first line of the header in my email system (SUN Unix) is the time I receive it. So I always record the receival time. Of course I can just edit the emails myself and store the sending time of the list server. But this is more inconvenient. So I won't quote any date to avoid the confusion.

Bill Powers:
Thanks for the diagram of the distance-sensing system. I will think about it. Your "crowd" program looks fun. I remember someone wrote a book called "Vehicle(s)" which described how a society of simple vehicles can show seemingly intelligent behaviors. Any relationships?

Mark Nelson:
I have my own email problem too. Do you know Marvin Minsky's "The Society of Mind"? Any influence on your work?

Chuck Tucker, Bill Powers:
I am very interested in the relationship between positive feedback and obsessive-compulsive disorder (OCD). Sometimes I wonder if I had (or maybe still have) OCD. It seems like in an ivory tower. It's so difficult to get out.

Chung-Chih Chen
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Pleinlaan 2
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(email: chen@arti.vub.ac.be)

Date: Mon, 14 Jan 91 08:20:26 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Hello

Well its monday and no csg mail. I got the post from Mark Olson last friday. He asked where everybody was. I want to know to. Is the net still alive. How about just a hello from those who are still out there, listening. Mark also mentioned a post from Haley that I never saw. Am I missing some postings? Perhaps the world situation really is having an influence on the rate of posts. It certainly diverts one's attention. But one could look at all these crises as an excellent and depressing example of what happens when extremely skillful groups of control systems get

together -- CONFLICT. Perhaps the world could usew some deep breathing execises. I don't mean to seem abouve this crisis myself -- my fists start tightening up when I think of this jerk Sodamn Insane saying he won't get his hand out of the candy jar no matter what. But then my eyes roll in my head at the lack of imagination that the west (and US) shows in giving the jerk no face saving alternative. Why do people imagine that external force will
change people's behavior (in any sensible direction other than opposing the force)? Ah well, back to my paper on hierarchical perception/behavior.
PS. For those of you with relatives in the gulf forces I send my heartfelt sympathies and flag waving support.

Hasta Luego

Rick M.

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Date: Mon, 14 Jan 91 13:59:57 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Where am I

I have read all of the posts since last year but have not been able to get myself organized to comment on any of them; I hope to be able to do so later this week.

My time has been spent with working on my classes and viewing the debate about the possible war and hoping and praying that there is no war. It is so sad that our representatives and the presidents' people can not figure out how to get out of this situation. I don't think there as any justification for this war (probably any war today) that I would accept but this one is especially troublesome since it is a Holy War. I don't think that most citizens of this country know what that means for us; I hope that the Europeans can get it worked out to stop the war.

PRAY FOR PEACE AND HOPE WE STILL HAVE A NET DAY AFTER TOMORROW CHUCK

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Date: Mon, 14 Jan 91 14:08:46 EST
Reply-To: Tom Benson <T3B@PSUVM>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Resent-From: "CHARLES W. TUCKER" <N050024@UNIVSCVM>
Comments: Originally-From: Tom Benson <T3B@PSUVM.BITNET>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: CRTNET 292

THOUGHT Y'ALLMIGHT LIKE THIS

----------Original message----------

CRTNET

January 13, 1991

Number 292

| |
| COMMUNICATION RESEARCH AND THEORY NETWORK |
| |
| Edited by Tom Benson, Penn State University |

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CONTENTS --

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The following is forwarded from:

> From: dst@dst.boltz.cs.cmu.edu (Dave Touretzky)
> Newsgroups: comp.ai
> Keywords: metaphor, cognitive psychology
> Date: 31 Dec 90 08:01:30 GMT
> Organization: Carnegie-Mellon University, CS/RI

To Friends and Colleagues on the Net:

From George Lakoff,
Professor of Linguistics,
University of California at Berkeley
(lakoff@cogsci.berkeley.edu)

January 15 is getting very close. As things now stand, President Bush seems to have convinced most of the country that war in the gulf is morally justified, and that it makes sense to think of ``winning'' such a war.

I have just completed a study of the way the war has been justified. I have found that the justification is based very largely on a metaphorical system of thought in general use for understanding foreign policy. I have analyzed the system, checked it to see what the metaphors hide, and have checked to the best of my ability to see whether the metaphors fit the situation in the gulf, even if one accepts them. So far as I can see, the justification for war, point by point, is anything but clear.

The paper I have written is relatively short -- 7,000 words. Yet it is far too long for the op-ed pages, and January 15 is too close for journal or magazine publication. The only alternative I have for getting these ideas out is via the various computer networks.

While there is still time, it is vital that debate over

the justification for war be seriously revived.
I am therefore asking your help. Please look over the
enclosed paper. If you find it of value, please
send it on to members of your newsgroup, to friends,
and to other newsgroups.
Feel free to distribute it to anyone interested.

More importantly, if you feel strongly about this issue,
start talking and writing about it yourself.

Computer networks have never before played an important
role in a matter of vital public importance. The time has come.
The media have failed to question what should be questioned.
It is up to us to do so. There are a lot of us connected by
these networks, and together we have enormous influence.
Just imagine the media value of a major computerized debate over
the impending war!

We have a chance to participate in the greatest experiment
ever conducted in vital, widespread, instantaneous democratic
communication.
Tens of thousands of lives are at stake.
During the next two weeks
there is nothing more important that we can send over these
networks than a fully open and informed exchange of views
about the war.

Here is the first contribution. Pass it on!

Metaphor and War

The Metaphor System Used to Justify War in the Gulf

George Lakoff Linguistics Department University of California at Berkeley (lakoff@cogsci.berkeley.edu)

Metaphors can kill. The discourse over whether we should go to war in the gulf is a panorama of metaphor. Secretary of State Baker sees Saddam as ``sitting on our economic lifeline.'' President Bush sees him as having a ``stranglehold'' on our economy. General Schwartzkopf characterizes the occupation of Kuwait as a ``rape'' that is ongoing. The President says that the US is in the gulf to ``protect freedom, protect our future, and protect the innocent'', and that we must ``push Saddam Hussein back.'' Saddam is seen as Hitler. It is vital, literally vital, to understand just what role metaphorical thought is playing in bringing us to the brink of war. Metaphorical thought, in itself, is neither good nor bad; it is simply commonplace and inescapable. Abstractions and enormously complex situations are routinely understood via metaphor. Indeed, there is an extensive, and mostly unconscious, system of metaphor that we use automatically and unreflectively to understand complexities and abstractions. Part of this system is devoted to understanding international relations and war. We now know enough about this system to

have an idea of how it functions. The metaphorical understanding of a situation functions in two parts. First, there is a widespread, relatively fixed set of metaphors that structure how we think. For example, a decision to go to war might be seen as a form of cost-benefit analysis, where war is justified when the costs of going to war are less than the costs of not going to war. Second, there is a set of metaphorical definitions that that allow one to apply such a metaphor to a particular situation. In this case, there must be a definition of ``cost'', including a means of comparing relative ``costs''. The use of a metaphor with a set of definitions becomes pernicious when it hides realities in a harmful way. It is important to distinguish what is metaphorical from what is not. Pain, dismemberment, death, starvation, and the death and injury of loved ones are not metaphorical. They are real and in a war, they could afflict tens, perhaps hundreds of thousands, of real human beings, whether Iraqi, Kuwaiti, or American.

War as Politics; Politics as Business

Military and international relations strategists do use a cost-benefit analysis metaphor. It comes about through a metaphor that is taken as definitional by most strategic thinkers in the area of international politics. Clausewitz's Metaphor: WAR IS POLITICS PURSUED BY OTHER MEANS. Karl von Clausewitz was a Prussian general who perceived war in terms of political cost-benefit analysis. Each nation-state has political objectives, and war may best serve those objectives. The political ``gains'' are to to be weighed against acceptable ``costs.'' When the costs of war exceed the political gains, the war should cease. There is another metaphor implicit here: POLITICS IS BUSINESS where efficient political management is seen as akin to efficient business management. As in a well-run business, a well-run government should keep a careful tally of costs and gains. This metaphor for characterizing politics, together with Clausewitz's metaphor, makes war a matter of cost-benefit analysis: defining beneficial ``objectives'', tallying the ``costs'', and deciding whether achieving the objectives is ``worth'' the costs. The New York Times, on November 12, 1990, ran a front-page story announcing that ``a national debate has begun as to whether the United States should go to war in the Persian Gulf.'' The Times described the debate as defined by what I have called Clausewitz's metaphor (though it described the metaphor as literal), and then raised the question, ``What then is the nation's political object in the gulf and what level of sacrifice is it worth?'' The ``debate'' was not over whether Clausewitz's metaphor was appropriate, but only over how various analysts calculated the relative gains and losses. The same has been true of the hearings of the Senate Foreign Relations Committee, where Clausewitz's metaphor provides the framework within which most discussion has taken place. The broad acceptance of Clausewitz's metaphor raises vital questions: What, exactly, makes it a metaphor rather than a literal truth? Why does it seem so natural to foreign policy experts? How does it fit into the overall metaphor system for understanding foreign relations and war? And, most importantly, what realities does it hide? To answer these questions, let us turn to the system of metaphorical thought most

commonly used by the general public in comprehending international politics. What follows is a two-part discussion of the role of metaphorical reasoning about the gulf crisis. The first part lays out the central metaphor systems used in reasoning about the crisis: both the system used by foreign policy experts and the system used by the public at large. The second part discusses how the system has been applied to the crisis in the gulf.

Part 1: The Systems

The State-as-Person System

A state is conceptualized as a person, engaging in social relations within a world community. Its land-mass is its home. It lives in a neighborhood, and has neighbors, friends and enemies. States are seen as having inherent dispositions: they can be peaceful or aggressive, responsible or irresponsible, industrious or lazy.

Well-being is wealth. The general well-being of a state is understood in economic terms: its economic health. A serious threat to economic health can thus be seen as a death threat. To the extent that a nation's economy depends on foreign oil, that oil supply becomes a `lifeline' (reinforced by the image of an oil pipeline).

Strength for a state is military strength.

Maturity for the person-state is industrialization. Unindustrialized nations are `underdeveloped', with industrialization as a natural state to be reached. Third-world nations are thus immature children, to be taught how to develop properly or disciplined if they get out of line. Nations that fail to industrialize at a rate considered normal are seen as akin to retarded children and judged as ``backward'' nations.

Rationality is the maximization of self-interest.

There is an implicit logic to the use of these metaphors: Since it is in the interest of every person to be as strong and healthy as possible, a rational state seeks to maximize wealth and military might. Violence can further self-interest. It can be stopped in three ways: Either a balance of power, so that no one in a neighborhood is strong enough to threaten anyone else. Or the use of collective persuasion by the community to make violence counter to self-interest. Or a cop strong enough to deter violence or punish it. The cop should act morally, in the community's interest, and with the sanction of the community as a whole. Morality is a matter of accounting, of keeping the moral books balanced. A wrongdoer incurs a debt, and he must be made to pay. The moral books can be balanced by a return to the situation prior to the wrongdoing, by giving back what has been taken, by recompense, or by punishment. Justice is the balancing of the moral books. War in this metaphor is a fight between two people, a form of hand-to-hand combat. Thus, the US might seek to ``push Iraq back out of Kuwait'' or ``deal the enemy a heavy blow,'' or ``deliver a knockout punch.'' A just war is thus a form of combat

for the purpose of settling moral accounts. The most common discourse form in the West where there is combat to settle moral accounts is the classic fairy tale. When people are replaced by states in such a fairy tale, what results is a scenario for a just war.

The Fairy Tale of the Just War

Cast of characters: A villain, a victim, and a hero. The victim and the hero may be the same person. The scenario: A crime is committed by the villain against an innocent victim (typically an assault, theft, or kidnapping). The offense occurs due to an imbalance of power and creates a moral imbalance. The hero either gathers helpers or decides to go it alone. The hero makes sacrifices; he undergoes difficulties, typically making an arduous heroic journey, sometimes across the sea to a treacherous terrain. The villain is inherently evil, perhaps even a monster, and thus reasoning with him is out of the question. The hero is left with no choice but to engage the villain in battle. The hero defeats the villain and rescues the victim. The moral balance is restored. Victory is achieved. The hero, who always acts honorably, has proved his manhood and achieved glory. The sacrifice was worthwhile. The hero receives acclaim, along with the gratitude of the victim and the community.

The fairy tale has an asymmetry built into it. The hero is moral and courageous, while the villain is amoral and vicious. The hero is rational, but though the villain may be cunning and calculating, he cannot be reasoned with. Heroes thus cannot negotiate with villains; they must defeat them. The enemy-as-demon metaphor arises as a consequence of the fact that we understand what a just war is in terms of this fairy tale. The most natural way to justify a war on moral grounds is to fit this fairy tale structure to a given situation. This is done by metaphorical definition, that is, by answering the questions: Who is the victim? Who is the villain? Who is the hero? What is the crime? What counts as victory? Each set of answers provides a different filled-out scenario. As the gulf crisis developed, President Bush tried to justify going to war by the use of such a scenario. At first, he couldn't get his story straight. What happened was that he was using two different sets of metaphorical definitions, which resulted in two different scenarios: The Rescue Scenario: Iraq is villain, the US is hero, Kuwait is victim, the crime is kidnap and rape. The Self-Defense Scenario: Iraq is villain, the US is hero, the US and other industrialized nations are victims, the crime is a death threat, that is, a threat to economic health. The American people could not accept the second scenario, since it amounted to trading lives for oil. The administration has settled on the first, and that seems to have been accepted by the public, the media, and Congress as providing moral justification for going to war.

The Ruler-for-State Metonymy

There is a metonymy that goes hand-in-hand with the State-as-Person metaphor:

Thus, we can refer to Iraq by referring to Saddam Hussein, and so have a single person, not just an amorphous state, to play the villain in the just war scenario. It is this metonymy that is invoked when the President says ``We have to get Saddam out of Kuwait.'' Incidentally, the metonymy only applies to those leaders perceived as rulers. Thus, it would be strange for us, but not for the Iraqis, to describe an American invasion of Kuwait by saying, ``George Bush marched into Kuwait.''

The Experts' Metaphors

Experts in international relations have an additional system of metaphors that are taken as defining a ``rational'' approach. The principal ones are the Rational Actor metaphor and Clausewitz's metaphor, which are commonly taught as truths in courses on international relations. We are now in a position to show precisely what is metaphorical about Clausewitz's metaphor. To do so, we need to look at a system of metaphors that is presupposed by Clausewitz's metaphor. We will begin with an everyday system of metaphors for understanding causation:

The Causal Commerce System

The Causal Commerce system is a way to comprehend actions intended to achieve positive effects, but which may also have negative effects. The system is composed of three metaphors:

Causal Transfer: An effect is an object transferred from a cause to an affected party. For example, sanctions are seen as ``giving'' Iraq economic difficulties. Correspondingly, economic difficulties for Iraq are seen as ``coming from'' the sanctions. This metaphor turns purposeful actions into transfers of objects. The Exchange Metaphor for Value: The value of something is what you are willing to exchange for it. Whenever we ask whether it is ``worth'' going to war to get Iraq out of Kuwait, we are using the Exchange Metaphor for Value plus the Causal Transfer metaphor. Well-being is Wealth: Things of value constitute wealth. Increases in well-being are ``gains''; decreases in well-being are ``costs.'' The metaphor of Well-being-as-Wealth has the effect of making qualitiative effects quantitative. It not only makes qualitatively different things comparable, it even provides a kind of arithmetic calculus for adding up costs and gains. Taken together, these three metaphors portray actions as commercial transactions with costs and gains. Seeing actions as transactions is crucial to applying ideas from economics to actions in general.

Risks

A risk is an action taken to achieve a positive effect, where the outcome is uncertain and where there is also a significant probability of a negative effect. Since Causal Commerce allows one to see positive effects of actions as ``gains'' and negative effects as ``costs'', it becomes natural to see a risky action metaphorically as a financial risk of a certain type, namely, a gamble.

Risks are Gambles

In gambling to achieve certain ``gains'', there are ``stakes'' that one can ``lose''. When one asks what is ``at stake'' in going to war, one is using the metaphors of Causal Commerce and Risks-as-Gambles. These are also the metaphors that President Bush uses when he refers to strategic moves in the gulf as a ``poker game'' where it would be foolish for him to ``show his cards'', that is, to make strategic knowledge public.

The Mathematicization of Metaphor

The Causal Commerce and Risks-as-Gambles metaphors lie behind our everyday way of understanding risky actions as gambles. At this point, mathematics enters the picture, since there is mathematics of gambling, namely, probability theory, decision theory, and game theory. Since the metaphors of Causal Commerce and Risks-as-Gambles are so common in our everyday thought, their metaphorical nature often goes unnoticed. As a result, it is not uncommon for social scientists to think that the mathematics of gambling literally applies to all forms of risky action, and that it can provide a general basis for the scientific study of risky action, so that risk can be minimized.

Rational Action

Within the social sciences, especially in economics, it is common to see a rational person as someone who acts in his own self-interest, that is, to maximize his own well-being. Hard-core advocates of this view may even see altruistic action as being ones self-interest if there is a value in feeling righteous about altruism and in deriving gratitude from others. In the Causal Commerce system, where well-being is wealth, this view of Rational Action translates metaphorically into maximizing gains and minimizing losses. In other words:

Rationality is Profit Maximization

This metaphor presupposes Causal Commerce plus Risks-as-Gambles, and brings with it the mathematics of gambling as applied to risky action. It has the effect of turning specialists in mathematical economics into ``scientific'' specialists in acting rationally so as to minimize risk and cost while maximizing gains. Suppose we now add the State-as-Person metaphor to the Rationality-as-Profit-Maximization metaphor. The result is:

International Politics is Business

Here the state is a Rational Actor, whose actions are transactions and who is engaged in maximizing gains and minimizing costs. This metaphor brings with it the mathematics of cost-benefit calculation and game theory, which is commonly taught in graduate programs in international relations. Clausewitz's metaphor, the major metaphor preferred by international relations strategists, presupposes this system. Clausewitz's Metaphor: War is Politics, pursued by other means. Since politics is business,

war becomes a matter of maximizing political gains and minimizing losses. In Clausewitzian terms, war is justified when there is more to be gained by going to war than by not going to war. Morality is absent from the Clausewitzian equation, except when there a political cost to acting immorally or a political gain from acting morally. Clausewitz's metaphor only allows war to be justified on pragmatic, not moral, grounds. To justify war on both moral and pragmatic grounds, the Fairy Tale of the Just War and Clausewitz's metaphor must mesh: The ``worthwhile sacrifices'' of the fairy tale must equal the Clausewitzian ``costs'' and the ``victory'' in the fairy tale must equal the Clausewitzian ``gains.'' Clausewitz's metaphor is the perfect expert's metaphor, since it requires specialists in political cost-benefit calculation. It sanctions the use of the mathematics of economics, probability theory, decision theory, and game theory in the name of making foreign policy rational and scientific. Clausewitz's metaphor is commonly seen as literally true. We are now in a position to see exactly what makes it metaphorical. First, it uses the State-as-Person metaphor. Second, it turns qualitative effects on human beings into quantifiable costs and gains, thus seeing political action as economics. Third, it sees rationality as profit-making. Fourth, it sees war in terms of only one dimension of war, that of political expediency, which is in turn conceptualized as business.

War as Violent Crime

To bear in mind what is hidden by Clausewitz's metaphor, we should consider an alternative metaphor that is _.n_.o_.t used by professional strategists nor by the general public to understand war as we engage in it. WAR IS VIOLENT CRIME: MURDER, ASSAULT, KIDNAPPING, ARSON, RAPE, AND THEFT. Here, war is understood only in terms of its moral dimension, and not, say, its political or economic dimension. The metaphor highlights those aspects of war that would otherwise be seen as major crimes. There is an Us-Them asymmetry between the public use of Clausewitz's metaphor and the War-as-Crime metaphor. The Iraqi invasion of Kuwait is reported on in terms of murder, theft and rape. The planned American invasion is never discussed in terms of murder, assault, and arson. Moreover, the US plans for war are seen, in Clausewitzian terms, as rational calculation. But the Iraqi invasion is discussed not as a rational move by Saddam, but as the work of a madman. We see US as rational, moral, and courageous and Them as criminal and insane.

War as a Competitive Game

It has long been noted that we understand war as a competitive game like chess, or as a sport, like football or boxing. It is a metaphor in which there is a clear winner and loser, and a clear end to the game. The metaphor highlights strategic thinking, team work, preparedness, the spectators in the world arena, the glory of winning and the shame of defeat. This metaphor is taken very seriously. There is a long tradition in the West of training military officers in team sports and chess. The military is trained to win. This can lead to a metaphor conflict, as it did in Vietnam, since Clausewitz's metaphor seeks to maximize geopol-

itical gains, which may or may not be consistent with absolute military victory. The situation at present is that the public has accepted the rescue scenario of the just war fairy tale as providing moral justification. The president, for internal political reasons, has accepted the competitive game metaphor as taking precedence over Clausewitz's metaphor: If he must choose, he will go for the military win over maximizing geopolitical gains. The testimony of the experts before Congress falls largely within Clausewitz's metaphor. Much of it is testimony about what will maximize gains and minimize losses. For all that been questioned in the Congressional hearings, these metaphors have not. It important to see what they hide.

Is Saddam Irrational?

The villain in the Fairy Tale of the Just War may be cunning, but he cannot be rational. You just do not reason with a demon, nor do you enter into negotiations with him. The logic of the metaphor demands that Saddam be irrational. But is he? Administration policy is confused on the issue. Clausewitz's metaphor, as used by strategists, assumes that the enemy is rational: He too is maximizing gains and minimizing costs. Our strategy from the outset has been to ``increase the cost'' to Saddam. That assumes he is rational and is maximizing his self-interest. At the same time, he is being called irrational. The nuclear weapons argument depends on it. If he is rational, he should follow the logic of deterrence. We have thousands of hydrogen bombs in warheads. Israel is estimated to have between 100 and 200 deliverable atomic bombs. It would take Saddam at least eight months and possibly five years before he had a crude, untested atomic bomb on a truck. The most popular estimate for even a few deliverable nuclear warheads is ten years. The argument that he would not be deterred by our nuclear arsenal and by Israel's assumes irrationality. The Hitler analogy also assumes that Saddam is a villainous madman. The analogy presupposes a Hitler myth, in which Hitler too was an irrational demon, rather than a rational self-serving brutal politician. In the myth, Munich was a mistake and Hitler could have been stopped early on had England entered the war then. Military historians disagree as to whether the myth is true. Be that as it may, the analogy does not hold. Whether or not Saddam is Hitler, Iraq isn't Germany. It has 17 million people, not 70 million. It is economically weak, not strong. It simply is not a threat to the world. Saddam is certainly immoral, ruthless, and brutal, but there is no evidence that he is anything but rational. Everything he has done, from assassinating political opponents, to using poison gas against his political enemies, the Kurds, to invading Kuwait can be see as furthering his own self-interest.

Kuwait as Victim

The classical victim is innocent. To the Iraquis, Kuwait was anything but an innocent ingenue. The war with Iran virtually bankrupted Iraq. Iraq saw itself as having fought that war partly for the benefit of Kuwait and Saudi Arabia, where Shiite citizens supported Khomeini's Islamic Revolution. Kuwait had agreed to help finance the war, but after the war, the Kuwaitis insisted on

repayment of the ``loan.'' Kuwaitis had invested hundreds of billions in Europe, America and Japan, but would not invest in Iraq after the war to help it rebuild. On the contrary, it began what amounted to economic warfare against Iraq by overproducing its oil quota to hold oil prices down. In addition, Kuwait had drilled laterally into Iraqi territory in the Rumailah oil field and had extracted oil from Iraqi territory. Kuwait further took advantage of Iraq by buying its currency, but only at extremely low exchange rates. Subsequently, wealthy Kuwaitis used that Iraqi currency on trips to Iraq, where they bought Iraqi goods at bargain rates. Among the things they bought most flamboyantly were liquor and prostitutes-widows and orphans of men killed in the war, who, because of the state of the economy, had no other means of support. All this did not endear Kuwaitis to Iraqis, who were suffering from over 70% inflation. Moreover, Kuwaitis had long been resented for good reason by Iraqis and moslems from other nations. Capital rich, but labor poor, Kuwait imported cheap labor from other moslem countries to do its least pleasant work. At the time of the invasion, there were 400,000 Kuwaiti citizens and 2.2 millions foreign laborers who were denied rights of citizenry and treated by the Kuwaitis as lesser beings. In short, to the Iraqis and to labor-exporting Arab countries, Kuwait is badly miscast as a purely innocent victim. This does not in any way justify the horrors perpetrated on the Kuwaitis by the Iraqi army. But it is part of what is hidden when Kuwait is cast as an innocent victim. The ``legitimate government'' that we seek to reinstall is an oppressive monarchy.

What is Victory?

In a fairy tale or a game, victory is well-defined. Once it is achieved, the story or game is over. Neither is the case in the gulf crisis. History continues, and ``victory'' makes sense only in terms of continuing history. The president's stated objectives are total Iraqi withdrawal and restoration of the Kuwaiti monarchy. But no one believes the matter will end there, since Saddam would still be in power with all of his forces intact. General Powell said in his Senate testimony that if Saddam withdrew, the US would have to ``strengthen the indigenous countries of the region'' to achieve a balance of power. Presumably that means arming Assad, who is every bit as dangerous as Saddam. Would arming another villain count as victory? If we go to war, what will constitute ``victory''? Suppose we conquer Iraq, wiping out its military capability. How would Iraq be governed? No puppet government that we set up could govern effectively since it would be hated by the entire populace. Since Saddam has wiped out all opposition, the only remaining effective government for the country would be his Ba'ath party. Would it count as a victory if Saddam's friends wound up in power? If not, what other choice is there? And if Iraq has no remaining military force, how could it defend itself against Syria and Iran? It would certainly not be a ``victory'' for us if either of them took over Iraq. If Syria did, then Assad's Arab nationalism would become a threat. If Iran did, then Islamic fundamentalism would become even more powerful and threatening. It would seem that the closest thing to a ``victory'' for the US in case of war would be to drive the Iraqis out of Kuwait; destroy just enough of Iraq's military to

leave it capable of defending itself against Syria and Iran; somehow get Saddam out of power, but let his Ba'ath party remain in control of a country just strong enough to defend itself, but not strong enough to be a threat; and keep the price of oil at a reasonably low level. The problems: It is not obvious that we could get Saddam out of power without wiping out most of Iraq's military capability. We would have invaded an Arab country, which would create vast hatred for us throughout the Arab world, and would no doubt result in decades of increased terrorism and lack of cooperation by Arab states. We would, by defeating an Arab nationalist state, strengthen Islamic fundamentalism. Iraq would remain a cruel dictatorship run by cronies of Saddam. By reinstating the government of Kuwait, we would inflame the hatred of the poor toward the rich throughout the Arab world, and thus increase instability. And the price of oil would go through the roof. Even the closest thing to a victory doesn't look very victorious. In the debate over whether to go to war, very little time has been spent clarifying what a victory would be. And if ``victory'' cannot be defined, neither can ``worthwhile sacrifice.''

The Arab Viewpoint

The metaphors used to conceptualize the gulf crisis hide the most powerful political ideas in the Arab world: Arab nationalism and Islamic fundamentalism. The first seeks to form a racially-based all-Arab nation, the second, a theocratic all-Islamic state. Though bitterly opposed to one another, they share a great deal. Both are conceptualized in family terms, an Arab brotherhood and an Islamic brotherhood. Both see brotherhoods as more legitimate than existing states. Both are at odds with the state-as-person metaphor, which sees currently existing states as distinct entities with a right to exist in perpetuity. Also hidden by our metaphors is perhaps the most important daily concern throughout the Arab world: Arab dignity. Both political movements are seen as ways to achieve dignity through unity. The current national boundaries are widely perceived as working against Arab dignity in two ways: one internal and one external. The internal issue is the division between rich and poor in the Arab world. Poor Arabs see rich Arabs as rich by accident, by where the British happened to draw the lines that created the contemporary nations of the Middle East. To see Arabs metaphorically as one big family is to suggest that oil wealth should belong to all Arabs. To many Arabs, the national boundaries drawn by colonial powers are illegitimate, violating the conception of Arabs as a single ``brotherhood'' and impoverishing millions. To those impoverished millions, the positive side of Saddam's invasion of Kuwait was that it challenged national borders and brought to the fore the divisions between rich and poor that result from those lines in the sand. If there is to be peace in the region, these divisions must be addressed, say, by having rich Arab countries make extensive investments in development that will help poor Arabs. As long as the huge gulf between rich and poor exists in the Arab world, a large number of poor Arabs will continue to see one of the superstate solutions, either Arab nationalism or Islamic fundamentalism, as being in their self-interest, and the region will continue to be unstable. The external issue is the weakness.

The current national boundaries keep Arab nations squabbling among themselves and therefore weak relative to Western nations. To unity advocates, what we call ``stability'' means continued weakness. Weakness is a major theme in the Arab world, and is often conceptualized in sexual terms, even more than in the West. American officials, in speaking of the ``rape'' of Kuwait, are conceptualizing a weak, defenseless country as female and a strong militarily powerful country as male. Similarly, it is common for Arabs to conceptualize the colonization and subsequent domination of the Arab world by the West, especially the US, as emasculation. An Arab proverb that is reported to be popular in Iraq these days is that ``It is better to be a cock for a day than a chicken for a year.'' The message is clear: It is better to be male, that is, strong and dominant for a short period of time than to be female, that is, weak and defenseless for a long time. Much of the support for Saddam among Arabs is due to the fact that he is seen as standing up to the US, even if only for a while, and that there is a dignity in this. If upholding dignity is an essential part of what defines Saddam's ``rational self-interest'', it is vitally important for our government to know this, since he may be willing to go to war to ``be a cock for a day.'' The US does not have anything like a proper understanding of the issue of Arab dignity. Take the question of whether Iraq will come out of this with part of the Rumailah oil fields and two islands giving it a port on the gulf. From Iraq's point of view these are seen as economic necessities if Iraq is to re-build. President Bush has spoken of this as ``rewarding aggression'', using the Third-World-Countries-As-Children metaphor, where the great powers are grown-ups who have the obligation to reward or punish children so as to make them behave properly. This is exactly the attitude that grates on Arabs who want to be treated with dignity. Instead of seeing Iraq as a sovereign nation that has taken military action for economic purposes, the president treats Iraq as if it were a child gone bad, who has become the neighborhood bully and should be properly disciplined by the grown-ups. The issue of the Rumailah oil fields and the two islands has alternatively been discussed in the media in terms of ``saving face.'' Saving face is a very different concept than upholding Arab dignity and insisting on being treated as an equal, not an inferior.

What is Hidden By Seeing the State as a Person?

The State-as-Person metaphor highlights the ways in which states act as units, and hides the internal structure of the state. Class structure is hidden by this metaphor, as is ethnic composition, religious rivalry, political parties, the ecology, the influence of the military and of corporations (especially multinational corporations). Consider ``national interest.'' It is in a person's interest to be healthy and strong. The State-as-Person metaphor translates this into a ``national interest'' of economic health and military strength. But what is in the ``national interest'' may or may not be in the interest of many ordinary citizens, groups, or institutions, who may become poorer as the GNP rises and weaker as the military gets stronger. The ``national interest'' is a metaphorical concept, and it is defined in America by politicians and policy makers. For the most part, they are

influenced more by the rich than by the poor, more by large corporations than by small business, and more by developers than ecological activists. When President Bush argues that going to war would ``serve our vital national interests'', he is using a metaphor that hides exactly whose interests would be served and whose would not. For example, poor people, especially blacks and Hispanics, are represented in the military in disproportionately large numbers, and in a war the lower classes and those ethnic groups will suffer proportionally more casualties. Thus war is less in the interest of ethnic minorities and the lower classes than the white upper classes. Also hidden are the interests of the military itself, which are served when war is justified. Hopes that, after the cold war, the military might play a smaller role have been dashed by the president's decision to prepare for war. He was advised, as he should be, by the national security council, which consists primarily of military men. War is so awful a prospect that one would not like to think that military self-interest itself could help tilt the balance to a decision for war. But in a democratic society, the question must be asked, since the justifications for war also justify continued military funding and an undiminished national political role for the military.

Energy Policy

The State-as-Person metaphor defines health for the state in economic terms, with our current understanding of economic health taken as a given, including our dependence on foreign oil. Many commentators have argued that a change in energy policy to make us less dependent on foreign oil would be more rational than going to war to preserve our supply of cheap oil from the gulf. This argument may have a real force, but it has no metaphorical force when the definition of economic health is taken as fixed. After all, you don't deal with an attack on your health by changing the definition of health. Metaphorical logic pushes a change in energy policy out of the spotlight in the current crisis. I do not want to give the impression that all that is involved here is metaphor. Obviously there are powerful corporate interests lined up against a fundamental restructuring of our national energy policy. What is sad is that they have a very compelling system of metaphorical thought on their side. If the debate is framed in terms of an attack on our economic health, one cannot argue for redefining what economic health is without changing the grounds for the debate. And if the debate is framed in terms of rescuing a victim, then changes in energy policy seem utterly beside the point.

The ``Costs'' of War

Clausewitz's metaphor requires a calculation of the ``costs'' and the ``gains'' of going to war. What, exactly, goes into that calculation and what does not? Certainly American casualties, loss of equipment, and dollars spent on the operation count as costs. But Vietnam taught us that there are social costs: trauma to families and communities, disruption of lives, psychological effects on veterans, long-term health problems, in addition to the cost of spending our money on war instead of on vital social needs at

home. Also hidden are political costs: the enmity of Arabs for many years, and the cost of increased terrorism. And barely discussed is the moral cost that comes from killing and maiming as a way to settle disputes. And there is the moral cost of using a ``cost'' metaphor at all. When we do so, we quantify the effects of war and thus hide from ourselves the qualitative reality of pain and death. But those are costs to us. What is most ghoulish about the cost-benefit calculation is that ``costs'' to the other side count as ``gains'' for us. In Vietnam, the body counts of killed Viet Cong were taken as evidence of what was being ``gained'' in the war. Dead human beings went on the profit side of our ledger. There is a lot of talk of American deaths as ``costs'', but Iraqi deaths aren't mentioned. The metaphors of cost-benefit accounting and the fairy tale villain lead us to devalue of the lives of Iraqis, even when most of those actually killed will not be villains at all, but simply innocent draftees or reservists or civilians.

America as Hero

The classic fairy tale defines what constitutes a hero: it is a person who rescues an innocent victim and who defeats and punishes a guilty and inherently evil villain, and who does so for moral rather than venal reasons. If America starts a war, will it be functioning as a hero? It will certainly not fit the profile very well. First, one of its main goals will be to reinstate ``the legitimate government of Kuwait.'' That means reinstating an absolute monarchy, where women are not accorded anything resembling reasonable rights, and where 80% of the people living in the country are foreign workers who do the dirtiest jobs and are not accorded the opportunity to become citizens. This is not an innocent victim whose rescue makes us heroic. Second, the actual human beings who will suffer from an all-out attack will, for the most part, be innocent people who did not take part in the atrocities in Kuwait. Killing and maiming a lot of innocent bystanders in the process of nabbing a much smaller number of villains does not make one much of a hero. Third, in the self-defense scenario, where oil is at issue, America is acting in its self-interest. But, in order to qualify as a legitimate hero in the rescue scenario, it must be acting selflessly. Thus, there is a contradiction between the self-interested hero of the self-defense scenario and the purely selfless hero of the rescue scenario. Fourth, America may be a hero to the royal families of Kuwait and Saudi Arabia, but it will not be a hero to most Arabs. Most Arabs do not think in terms of our metaphors. A great many Arabs will see us as a kind of colonial power using illegitimate force against an Arab brother. To them, we will be villains, not heroes. America appears as classic hero only if you don't look carefully at how the metaphor is applied to the situation. It is here that the State-as-Person metaphor functions in a way that hides vital truths. The State-as-Person metaphor hides the internal structure of states and allows us to think of Kuwait as a unitary entity, the defenseless maiden to be rescued in the fairy tale. The metaphor hides the monarchical character of Kuwait, and the way Kuwaitis treat women and the vast majority of the people who live in their country. The State-as-Person metaphor also hides the internal structures of Iraq, and thus hides the

actual people who will mostly be killed, maimed, or otherwise harmed in a war. The same metaphor also hides the internal structure of the US, and therefore hides the fact that is the poor and minorities who will make the most sacrifices while not getting any significant benefit. And it hides the main ideas that drive Middle Eastern politics.

Things to Do

War would create much more suffering than it would alleviate, and should be renounced in this case on humanitarian grounds. There is no shortage of alternatives to war. Troops can be rotated out and brought to the minimum level to deter an invasion of Saudi Arabia. Economic sanctions can be continued. A serious system of international inspections can be instituted to prevent the development of Iraq's nuclear capacity. A certain amount of ``face-saving'' for Saddam is better than war: As part of a compromise, the Kuwaiti monarchy can be sacrificed and elections held in Kuwait. The problems of rich and poor Arabs must be addressed, with pressures placed on the Kuwaitis and others to invest significantly in development to help poor Arabs. Balance of power solutions within the region should always be seen as moves toward reducing, not increasing armaments; positive economic incentives can used, together with the threat of refusal by us and the Soviets to supply spare parts needed to keep hi-tech military weaponry functional. If there is a moral to come out of the Congressional hearings, it is that there are a lot of very knowledgeable people in this country who have thought about alternatives to war. They should be taken seriously.

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** END OF CRTNET **
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Date: Mon, 14 Jan 91 18:57:50 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: negative waves

What's all this talk about the end of the world and disappearance of the net? Just when I thought my dissertation was down the tubes along comes CT and I see a light at the end of the tunnel (how's that for a metaphor?) Now when I have something to live for (academically speaking) I hear people talk like the end is near (maybe we could make up some posters to carry on the street saying "Repent! The final Disturbance is here!). Seriously, I read somewhere in scripture that when Joel Judd graduates with a Ph.D., "then ye may know that the end is near." So relax--you've got another twenty years or so.

Hugh, Bill, Rick, ?? ,

I have been dallying over a way to pose a couple of questions regarding reorganization and I just can't find the perfect way to ask so maybe it's time to negotiate some meanings.

I'm trying to outline the thesis chapters which briefly characterize learning in genera, then language learning, then implications for teaching. I'd like criticisms of the following:

1) Gary got Hugh to back off from his use of "accomodation" and "assimilation" (901213), but regardless of the terms I found the distinctions between working within a conceptual scheme and changing conceptual schemes themselves intuitively satisfying. Both of these were considered to be "learning", although only the latter was considered "reorganization" in DILEMMA. More recently, however, Robertson and Powers (1990) suggest turning from a focus on learning to one on "how organisms develop new control systems (p.7)." The development of new control systems is organization, and the modification of existing ones reorganization.

1a)Learning as a term/concept can be used as long as one remembers it refers to the development/modification of control systems. What one has learned is shown by demonstrating controlled variables, not their attendant behaviors.

1b)"New" knowledge, that which disturbs existing conceptual schemes, will approached by the student with current conceptual schemes and he will try to reduce the error with those schemes (Petrie 1981:184). Anamolies will result in the changing of conceptual schemes. Activities which might help a student to deal with knowledge in existing schemes are homework, in-class practice, etc. Activities which might facilitate changing conceptual schemes are discussion, having students "discover" the concept, etc.

It is at this point where Gary Cziko and I have talked about learning and can't agree on terms. He feels that changing conceptual schemes is learning, but "fixing" those schemes is not. It is at this point of reducing error AFTER reorganization that I guess I'm in search of a term or something. A concrete example we used was the concept of 'past' verb endings in English. As with almost any linguistic "rule", there are helpful generalities, and some annoying exceptions. I feel that reorganization involves getting the concept of an English past tense, and getting the irregularities down involves working within such a concept, yet both are "learning" (perhaps pointing to some of the problems of the term? This transition period has received much attention in the field of Second Language Acquisition under the label "Interlanguage". What say ye?

Joel Judd

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Date: Tue, 15 Jan 91 10:25:46 CST
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Subject: Learning and reorganization

General --

Mary is out of her casts as of the 11th, and can walk with a cane. The kneecap seems to have healed completely. Now it's only a matter of getting the control systems back in shape. Mary says she knows what it means to say that standing up is balancing one bone on top of another. Pretty wobbly at first, after 8 weeks of immobility. The underlying machinery of a knee joint with weight on it is inherently unstable. There is positive feedback -- any little buckle makes the tendency to buckle larger. The control systems have to put negative feedback around the positive feedback so that the net feedback comes out negative. Now you know how to design a leg.

Joel Judd, Gary Cziko, Hugh Petrie (910114 and previous) --

Perhaps assimilation is the use and refinement of existing systems while accomodation is the acquisition of new systems. I claim that one must not be hasty in seeing aspects of learning as reorganization. I think particularly that we must not get hung up on matters of classification -- this is a that. It's more important to keep looking at the processes involved and trying to pick them apart into understandable functions that are acquired. When we understand clearly what is involved in particular cases and stages of learning, I don't think it will be too hard to see what is reorganization and what is not. Something is not (primarily) reorganization if it can be accomplished by using an existing system under new conditions. To me, reorganization means literally changing the KIND of function being carried out, or adding a completely new function. Changing

organization, not simply changing how a given organization functions in detail.

A few random topics:

Practice effects: testing and modifying a new control system under varying conditions of the environment and in the presence of novel disturbances. You get the idea of applying general rules in language -- that's a reorganization -- but then you go through a long process of learning new rules and applying them, learning exceptions in specific cases, and developing swiftness and ease in the production of grammatical sentences that describe complex experiences. You know how to apply rules to language but you don't know all the rules yet: that is a matter of applying the new skill over and over. During practice, organization is certainly changing as the skills continue to improve, but it's not changing its nature. As Mary relearns walking, the control systems that maintain given angles at the knee-joint will start out using weakened muscles and neural computations that have drifted away from optimal values. The control systems are not yet stable. With practice, the muscles will grow in bulk, the damping coeffients will rise, and the loop gain will increase until control has returned to normal. But nothing NEW will be added to the ORGANIZATION of the systems that control this leg. The same circuit diagram will continue to apply. Only the detailed parameters will change their values, which they do continually anyway. Maybe we should call that sort of continual adjustment of parameters adaptation, to distinguish it from the building of a new system from a pile of parts. Our "adaptive control" experts might have a comment there.

Memory: really two topics, perceptual remembering and forming of memory associations. I haven't had much to say about associations except to characterize them as a form of memory addressing (any part of a memory recording can serve as the address of the remainder of the recording, or perhaps more realistically, as the address of some subset of recorded information). The forming of new memory associations is, I would guess, an inherited ability and not something acquired by reorganization. A lot of learning amounts only to extending the repertoire of memory associations. Building a naming-vocabulary, for example. Learning arithmetic tables. I think that the teaching of materials that require nothing but memorization should be treated explicitly that way, and students should be taught all the old rhetoricians' tricks that make it easy. Why dress up memorization problems to make them look as if they require fancy logic or other conceptual schemes? You might as well associate "11 x 12" and "12 x 11" (separately) with the number "123" and have done with it. A third-grader can learn those tables forever in a week, using funny association images and enjoying the process. Ten minutes a day does the trick. Instead of a semester or a year of pain that generates reorganization that only gets in the way. Understanding of why these associations are appropriate can come later, when (if) the student becomes curious about why they work so well. That's a different level of learning. There's no point in trying to teach generalizations before there's some experience to generalize FROM. Keep the damned mathematicians (and linguists) out of it when the problem is just straight memorization. All real generalization is post hoc. We can't teach generalization -- we can only wait for the capacity to generalize to appear, and provide material on which to practice this new kind of organization.

Well, I said a few "random" ideas.

The new skills that arise through reorganization can't be captured in words. A "new conceptual scheme" can be described, more or less, in words, after it has appeared. But to teach it one can't just describe it in words. The new scheme has to be demonstrated by setting up the lower-order aspects of the environment so that the new perception is perceivable, and so that actions of the right kind can control it. When you have to learn a new program, you can read the instructions over and over without really getting the idea. But if you can sit at the keyboard and try out each step as it's described, you repeatedly have the experience of "Oh, THAT's what it means." When I learn to operate a new computer or use a new programming language, the first thing I do is look for some cookbook instructions and follow them through slavishly, not even trying to "understand" them. I just want to see what happens. THEN I look for the explanation of what they mean, and since I've already experienced what happens at a lower level, I have little trouble grasping what the explanation says.

One of the big problems I experienced while young and that all my children experienced was the reaction "Why do I have to learn all this stuff?" I think that attempts to teach the big picture first have been a futile attempt to answer this sort of question, an attempt that doesn't really work very well. You say, "So you'll be able to get a good job and understand the world around you." Big deal, to a 14-year-old who already understands everything that matters and hates the idea of getting a job.

I think the answer is not to forecast the future usefulness of knowledge but to demonstrate its present usefulness in enhancing the student's capacities for control. Set up a puzzle. Show how to solve it: do this, then that, then this again, then that. Magic! How did you do that? Here, you try it. Do this, then that, then this again, then that. See? It works. Try it on a different problem. This, that, this, that. Worked again. Why does it work? Oh, we'll get to that tomorrow. First, everybody has to learn to do it. You're going to have to plead with me before I tell you why it works.

I didn't get where I am today by failing to step on the toes of people who know more than I do.

Different subject.

The deadline is in 14 hours. We will soon see how stimulus-response theory works yet once again.

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Date: Tue, 15 Jan 91 20:39:00 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Warning -- RSCS tag indicates an origin of SMTPUSER@UBVMSC
From: Hugh Petrie <PROHUGH@UBVMS.BITNET>
Subject: Re: Learning and reorganization

(Powers 910115), (Judd 910114)

Powers said,"Perhaps assimilation is the use and refinement of existing systems

while
accomodation is the acquisition of new systems. I claim that one must not be hasty in seeing aspects of learning as reorganization. I think particularly that we must not get hung up on matters of classification -- this is a that. It's more important to keep looking at the processes involved and trying to pick them apart into understandable functions that are acquired. When we understand clearly what is involved in particular cases and stages of learning, I don't think it will be too hard to see what is reorganization and what is not."

I think that is the right approach to looking at the relationship of learning and reorganization. In specific cases, WHAT is being reorganized and what is not are the questions. If I keep practicing shooting free throws, I see no particular reason not to say that I am learning better and better to shoot free throws, yet I suspect that the main "reorganization" occurs only in the "fine-tuning" of the basic control system--much as Powers describes it in terms of Mary's "relearning" to walk. On the other hand learning to shoot a basketball in the first case probably did require building a control system from parts laying around. I had to see it, see how it related to what I could already do, etc.

My guess is that some of the language learning tasks can likewise be sorted into more or less "fine-tuning" versus learning something fairly new. It will depend on the learning task and the experimental ananlysis of it. My problem is that I don't know enough about language learning even to hazard a guess on what various aspects might look like. I do think that Judd's first cut doesn't cause me any disturbance.

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Date: Wed, 16 Jan 91 10:21:28 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Learning and reorganization

(Powers 910115), (Judd 910114) Petrie (910115)

While I would agree that there may be a difference in amount of change involved in assimilation (or Powers' recent use of adaptation) and reorganization, the basic process (I feel has to be the same), that is variation and selection.

If my practicing free throws makes me more skilled, it can only be because my control systems have been changed in some way and this change is retained the next time I come up to the foul line. I can't see how this fine tuning can be fundamentally different from never having touched a basketball before and trying to get it through the hoop.

There have been a number of ways of explaining puzzles of fit, or the adaptation of one system to another. The first and probably still most

common is providence (divine or otherwise). Here we have Aristotle and Bishop Paley with their arguments from design (if living organisms are so fit and well designed, there must be a designer). The psychological equivalent is the homunculus.

The second is instructivism. The environment somehow instructs or transmits information to the organisms which results in adaptive changes. Here we have Lamarck and the inheritance of acquired characteristics. However, since ontogeny is epigenetic (i.e., recipe-based) and not preformationist (i.e., blueprint-based), this cannot work.

Next we have selection (blind variation and selective retention as Don Campbell would put it) COMBINED with providentialism and/or instructivism. This is Darwin (yes Darwin believed in Lamarckian inheritance of acquired characters and also wrote of a Creator who breathed life into the first organisms). But at least Darwin had the great insight of how powerful selection can be. He made selectionism legitimate.

Finally, we have pure selectivism, or universal selection theory (UST) as I call it. This is the ONLY way to avoid providentialism and instructivism (which for many reasons simply cannot work, at not least in biological evolution and I would argue nowhere else either). In psychology this seems to be the only way to avoid the homunculus that Powers keeps warning us about.

So, while there may be differences in the degrees of change involved in "fine-tuning" vs. reorganization, I would argue the basic process has to be the same. By making a strong distinction between fine-tuning and reorganization, it starts to sound to me as if one is saying that fine-tuning is somehow based on instructivism.

I would also like to throw out the idea that blind variation and selective retention must also be an inherent process of what appears to be already well-established control systems. A control system that is locked in so that it will not change seems of doubtful utility to me. It must continually vary its parameters (or whatever) to see if improvements can be made. Take the simple case of rat who learns to run on a wheel after hearing a tone to avoid a shock. If this control system was locked in the animal would never learn to extinguish when the shock was no longer delivered since it would just "think" that it was avoiding the shock successfully 100% of the time.

Perhaps we might introduce terms like major reorganization, minor reorganization (for fine-tuning, assimilation, adaptation, etc.) and minimal reorganization (for the functioning of well established control systems).

What I initially found so appealing about control theory was not the insight that behavior controls perception (which I now appreciate) but its potential to offer a psychological theory of behavior which avoids providentialism (homunculusism?) and instructivism. I'd like to keep it that way!

--Gary

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Date: Wed, 16 Jan 91 16:28:22 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: learning/reorganization/memory

Cziko (910116) and Petrie, Judd, Powers (previous),

Starting with a comment from the stick-your-neck-out department, I am feeling a bit nervous about trying to "fit" CT into an -ism, at least just yet. It seems that an -ism requires some sort of objectivity that we can't be sure of, especially regarding things like the origin(s) of the species, since we are dealing with our perceptions of things.

But leaving philosophy aside and turning to the following comment from Gary:

"If my practicing free throws makes me more skilled, it can only be because my control systems have been changed in some way and this change is retained the next time I come up to the foul line. I can't see how this fine tuning can be fundamentally different from never having touched a basketball before and trying to get it through the hoop."

Perhaps there are some aspects of learning and memory that are getting blended together in the basketball example. The first time someone showed you a foul line, a basketball, and a distant hoop, and told you to put the ball through the hoop, you probably had an idea of what to do--you had a "passive observation" memory of seeing the action before. But you had no perception of what it meant for YOU to put the ball through the hoop. Now, standing on the line, you've got to figure out how to get the ball to go through the hoop. If you were like me, fairly young, you probably couldn't do as the big guys do. You might have tried, but chances are your muscles simply didn't have the strength. So you probably tried something with both hands--a "granny" throw. This allowed you to approximate a perception of 'up and forward' which is what you wanted. Given enough effort, you probably missed a few times, then hit the backboard, the rim, and then started making a few baskets now and then. Now you have reorganized several levels of the hierarchy WHICH WERE ALREADY THERE into a 'free throw' program which did not previously exist. Later on, or if you were introduced to b-ball after growing up, your program was an over-the-head shot instead.

What you have, though, is a perception of 'shooting a free throw' which you did not have before. This perception is stored in memory. It can be imagined without you doing a thing (physically) ***QUESTION FOR BILL: In this sense, we cannot "imagine" what we have not perceived; that is, science fiction writers imagine all the time, but smelling a xart on the planet Ipsicum is not a perception?*** but it is also addressed when we return another time to the free throw line, because the reference levels are remembered. There is no reorganization for 'shooting a free throw' the next time. If there were, and people seem to agree that blind variation

seems to work in reorganization, the whole process would begin again: wild throws slowly changing to fairly consistent baskets. What good is memory if we have to reorganize every time?

People can and do improve free throwing. But this does not entail reorganization, any more than Marken's tea drinking example entails reorganization after every cup, or every sip, of tea. It DOES no doubt entail modification of lower levels: sensation, relationships, others. Different basketballs look, feel, and weigh different. I can even shoot free throws with volleyballs, four-square balls, and baseballs, as well as paper wads, and do so pretty well even the first time, because I have a higher level 'shooting free throw' perception. We need fine tuning (ie. a normally functioning control system) for the same reason we need control systems, because each situation is different and constantly changing.

Now if for some reason tomorrow I go to the court and the program I've used for fifteen years succeeds only in producing air balls or balls that go behind me, then I've got some reorganizing to do. If I've never shot a basketball before, reorganization is needed. When I can get the ball through the hoop once in a while, some fine tuning is needed. As long as my perceptual signals are within the error limits (60% of free throws good) I leave well enough alone.

Joel

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Date: Wed, 16 Jan 91 18:39:47 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Reorganization

From Bill Powers. I'm sending this from my new logon at Northeastern Illinois University, courtesy of Dick Robertson. The system gurus are still trying to figure out why I can't receive, though, so I will continue to receive all mail through my Urbana-Champaign logon, free0536@uiucvmd. If you send to CSG-L it will make no difference to you. When we get this new link working, Dick Robertson will also join us. If this doesn't get through, I'll send it the old way. Gary, if you don't receive this, please let me know.

Gary Cziko, Joel Judd (910116) --

The distinction I would like to make is between qualitative (structural) change, and quantitative (functional) change. I'm not sure that the parentheses add anything, however, because I often hear "structural" used when I would say "functional." So an example is needed.

Suppose I have a thermostat in place and functioning. To make a quantitative change in it, I can do a number of things.

I can, first off, change its set point: change the measure of temperature at which the furnace will switch from on to off and vice versa. This leads to a change in the behavior of the thermostat without altering any of its properties. I could also change the "anticipation" adjustment. This alters the effect of a rise or fall in temperature so that the furnace turns on before the temperature has fallen or risen all the way to the switching point, thus altering the magnitude of fluctuations in temperature without

altering the setpoint or the average temperature that is maintained. I have still done nothing to the organization of the thermostat, even though its behavior has again changed. Another thing I could so is to switch to a different fuel supplier, buying oil of a higher energy content. Now without altering anything structurally, I have made the thermostat spend less of its time with the furnace on. Another change in behavior with no change in organization.

Basically, to make quantitative changes, I alter PARAMETERS of the system without changing its ORGANIZATION. Now, how could I alter the thermostat qualitatively?

One thing I could do is to rip out the oil burner and substitute a gas burner. This requires changing the KIND of furnace I use. I have to alter the physical structure, the connectivity, of the system. Probably, the signal from the thermostat contacts will have to be routed to a different sort of igniter -- a gas valve instead of a liquid valve, with a pilot light instead of a spark to get the flame going. Because gas has a different thermal content and the efficiency of the furnace will be different, the behavior of the system is changed; it will spend more or less time "on" in order to maintain a given temperature.

I could get more drastic. I could disconnect the thermostat wires from the furnace and connect them to a motor that opens and closes windows. Or I could connect them to a heat pump. Or to a shutter that controls the amount of sunlight reaching a solar storage device, or to a motor that pumps liquid through a solar heat exchanger. I could clip out the bimetallic strip and substitute a semiconductor temperature sensor and its amplying circuit, and make the heat control continuously variable instead of on-off. I could move the thermostat sensor to a different place in the house, or put sensors in all the rooms and electronically average their readings. These changes would be qualitative, because they can't be achieved simply by altering parameters of the existing system. They are achieved by changing the system physically, structurally. Old parts are discarded and new ones are substituted. They might or might not change the way temperature is controlled.

When you first learn to shoot a free-throw, you don't have any control systems that can use the arms, hands, and legs to create the typical free-throw trajectory (I agree with Joel Judd here). You might start out throwing underhanded, two-handed, one handed, or backward over your shoulder. Those are qualitative considerations. You simply have to build a new control system to start shooting free-throws in any one of these ways. Every basketball player finds his own structural setup, his "style." After the basic control systems is set up, however, the player still has to play with the parameters. Where do you aim, at the backboard or the hoop? How high do you want the trajectory to be? How much spin do you put on the ball? How do you adjust for being pumped up, tired, discouraged? And you have to keep trimming up the control systems to keep them stable, to adjust the weights you give to arm effort, body effort, leg effort, and how you hold your mouth. Also, as you practice, all the muscles involved will grow in bulk and exert more force for the same driving signal. All these fine-tunings happen without altering the basic organization of the free-throw system at all. The connections stay the same.

My point is that you can't make the distinction between reorganization (qualitative change) and fine-tuning (quantitative change) just on the

basis of observing a change in behavior. You have to look at HOW the system is doing its controlling. You have to look at the block diagram of the system that tells you what is connected to what. A qualitative change involves changing the connections so they connect different blocks. A quantitative change involves altering the amount of effect that is being exerted by the existing signals in the existing blocks.

Selection can be at work whether we are talking about structure or function. While all these distinctions can have fuzzy edges in carefully-chosen cases, I think we can say that the structural changes are selected more by the environment than by the organism. Some things work and some just don't, and the organism has no say about that. The functional changes, however, emphasize internal choices more: when there are many things that will work, it's up to the organism to pick one. High trajectories can get the ball through the hoop just as well as low ones. You just have to make suitable adjustments in the parameters involved -- spin, speed, aim.

The ultimate selector is always the organism, of course, not the environment. Even if the environment says that you can't make a free-throw in one basket by shooting at the other one, the player has to be the one who insists that the free-throw should be made. That's the ultimate selection criterion. The aiming-point can be anywhere, but if the organism decides that it wants to make free-throws, it will keep changing its aiming point until a few of them start going in.

I'm with Joel Judd in feeling lukewarm about these "isms." Even if we like one better than another, it's going to carry a lot of baggage with it that a control theorist would like to discard. We're not in anybody's camp.

Smelling a xart on the Planet Ipsicum, Joel, is an image that is easy to understand. Ipsicum was clearly named by someone who knew Latin, and a xart is either a flower, an untidy animal, or something impolite. Science-fiction writers who are smart don't come up with words that are unpronounceable, and they no longer resort to saying that "a xart is something that human beings can't experience," because there's nothing to imagine to give meaning to the arbitrary symbol (made of familiar letters). The readers will imagine something anyway, and it won't be what you want. Only Henry Kuttner could get away with that.

Well, anyway, how does this quantitative-qualitative or structural-functional distinction grab you?

I'll write more later on the selection process itself. Maybe Gary can go into it in more detail: what does the selecting, and how?

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Date: Thu, 17 Jan 91 09:14:55 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: Reorganization

Bill (910116),

>The distinction makes sense as far as my understanding goes. One tangent I was
trying to provoke comment on by bringing up memory was also part of the
discussion Gary and I had a couple of days ago. He mentioned "learning" the

names of the state capitals. When he said that I had to think "What does it mean to say 'today I learned the capital of Illinois is Springfield'"? Either I have acquired some fact without relation to much else, or I have some concept of 'state' and 'capital' and 'government' and knowing the capital of Illinois has some meaning. In any case, it doesn't seem that knowing that fact, in and of itself, contributes to a concept of 'democratic government' or whatever. I guess it contributes to CATEGORIES such as 'state capital'? But as you and Petrie and others have pointed out, schooling is replete with such knowledge and our insistence that children learn it. Why not then call a spade a spade and use proven techniques to make it enjoyable.

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Date: Thu, 17 Jan 91 10:48:42 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Reorganization

Judd (910117); Powers (910117):

Joel Judd (910117) said:

> There is no reorganization for 'shooting a free throw' the
>next time. If there were, and people seem to agree that blind variation
>seems to work in reorganization, the whole process would begin again: wild
>throws slowly changing to fairly consistent baskets. What good is memory
>if we have to reorganize every time?

It seems that you have neglected the second part of the "magic" formula: blind variation and SELECTIVE RETENTION (I shoud probably also add the word "cumulative" to this last part). There is retention and memory of the control system modifications that have been. But conditions are always changing so that more changes (if even minor) have to be continually made and this must be done blindly from within the organism. Without this cumulative selective retention no increase in adaptive complexity can be explained. But retention without further modifications is also a near guarantee to eventually fail, unless you're luck enough to find a niche that simply doesn't change significantly over the eons, like the horseshoe crab.

Also:

>Now if for some reason tomorrow I go to the court and the program I've used
>for fifteen years succeeds only in producing air balls or balls that go
>behind me, then I've got some reorganizing to do. If I've never shot a
>basketball before, reorganization is needed. When I can get the ball
>through the hoop once in a while, some fine tuning is needed. As long as my
>perceptual signals are within the error limits (60% of free throws good) I leave well enough alone.

But if your shots are at first all consistently much too high, why do you call this reorganization while if you get them through once in a while you call it fine-tuning? And why be happy with just 60%? Don't we try to get them ALL in?

==========
Powers (910116)

I do feel that Bill Powers makes a potentially useful distinction between structural (qualitative) and functional (quantitative) changes which has some intuitive appeal. But then I start to wondering how the difference makes sense with respect to the nervous system. Bill, are you proposing that there are different neural changes underlying the two processes? If all learning is due to changes in synaptic connections, then I think you may be in trouble since by your definition this would all be structural. But if concentrations of neurotransmitters change as a result of learning perhaps this could be considered your quantitative change. Or perhaps you see the actual neural mechanisms as irrelevant to the issue.

I'd also like to hear more about your thoughts on selection since I don't feel quite ready to offer mine on how control system selection is accomplished other than to say that higher order perceptions would seem to have to be involved in selecting among the variations proposed by the lower levels.--Gary

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Reorganization

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Date: Thu, 17 Jan 91 11:44:00 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Continuing with reorganization

From Bill Powers --

The only way to control a control system is through the application of overwhelming physical force.

It doesn't do much good to urge people to be wiser or cleverer than they are. We need to understand how system concepts come to be invented and accepted, and why they can become so compelling as to enlist the support of millions of people and throw them at each others' throats. Given that system concepts are perceived at a level higher than that of logic and rationality, how are we to discuss them, compare them, or teach them? From what standpoint can we even knowingly examine them?

I was thinking this morning that the thought of going against an enemy by using invincible force is comforting. War is exhilarating when you are sure you can win. You feel safe knowing that nobody else is stronger. If you are stronger than everyone else, there is no need to be wise or clever. You can even afford to be compassionate, within cost-benefit limits.

War is caused by fear, not bravery.

More on reorganization and learning --

I currently think of reorganization as depending specifically on an output process that makes small changes at random, with the effects being judged by some built-in system by comparison with built-in specifications. Error governs the rate at which random changes are instituted, but has no relationship to the direction of change.

The changes I spoke of yesterday (910116) in connection with quantitative change of parameters are probably NOT random. When changes are systematic or algorithmic, they probably result from the working of an organized control system, not from reorganization. For example, if you're shooting your baskets consistently low, a reorganization could just as well result in shooting them even lower. There would be no relationship between the direction of change and the direction of the error. In fact, if you're shooting too low, you would raise your aim. That is a systematic correction, a logical one, one that can be based on a fixed relationship between error and change of action. This systematic way of correcting error could certainly be the product of reorganization (if it didn't exist before), but once it's in place it is simply the way a higher level of control works.

In my model the only influence of a higher level on a lower one is through changes in reference signals. Such changes can alter only the target that a lower system seeks. It's becoming more and more apparent that we need also to allow higher systems to adjust the parameters of lower ones. Parameter adjustment would include adjusting error sensitivity (including its sign), the amount of damping and other stabilizing parameters, and even the scaling of perceptual signals (as in my post to Chen of 901224). The simulation possibilities multiply in a daunting way, of course. But we don't have to explore them at random: when the simple model fails to explain behavior, we can see whether adding parameter control in a specific case will improve matters. I consider control by parameter a complication, and therefore to be avoided until we're forced into it.

But in discussing learning in general, I guess we're forced into it.

So, Gary, I think we have to include in the big picture the idea that reorganization can produce features of the system which, once produced, can then systematically institute parameter changes that alter details of behavior without blind variation and selective retention. The distinction

now on the table is between blind variation and systematic variation.

Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

Date: Thu, 17 Jan 91 13:50:49 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: update

Everyone,
Just a few hours ago I learned that, in fact, I was not receiving any mail from the network. This explains why I perceived that maybe all of you were on an extended vacation and why I sent a message a few days ago saying "Where is everyone?!" So the last message I have from the net is DEC 17. Hopefully Gary can or has resolved this for me--if not this is a one-way communication.

I quickly glanced over a printout of all that I missed and I regret that I wasn't in on it--so bare with me as I respond to "old" topics.

I noticed that the topic of Chaos had reappeared again. Despite your distaste for the topic, Bill, I am convinced that it plays an important role. Gary and Hugh already expressed how it may provide insight into reorganization. I also see it at another level (it does seem that the "answer" to the chaos/stability issue depends on what level one is coming from). I recently saw an hour-and-a-half film whose title goes something like "Koyanaqatsi." It's an Hopi Indian word for "life out of balance." Anyway the film has no sound and most of it runs at high speed. As I watched the film, I noticed that the scenes of urban life (people getting on an off escalators, driving cars, walking the streets, etc) the overall patterns resembled the dynamic patterns seen in clouds, fluids, and smoke. Do I think people behave in accordance to chaotic/dynamical laws? No I don't. But I do think that as each person behaves as a separate control system, dynamic and/or chaotic patterns appear. This is so because as one person alters his envirionment, the second person must alter his envirionment some, and the third person must then do the same, and so on and so forth--its necessarily dynamic (at least in a congested area).

So yes I do think that behavior is stable. But seeing chaos at the big-picture level (as well as the neural level) seems to have some interesting implications to the freedom vs. determinism arguement.

Bill and Rick,
Thank you for your comments on the "beautiful woman" dilema. I need to read them a few more times. I wish I had known that you had responded--now that its a WEEK later! So frustating...

I'll be in the Chicago area this weekend so my comments to all that I've missed will have to wait MORE.

Date: Thu, 17 Jan 91 18:05:12 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: selective retention

Gary Cziko (910117) --

Retention. Are you thinking of retention as osme separate process that is carried out after a favorable blind variation? If so, I don't think we need that. In the e. coli simulation, there is variation controlled (in frequency but not direction) by the difference between a reference signal and a controlled variable. When the difference is small, variations are spaced further apart. That amounts to "retaining" the good direction. But there is no mechanism for retaining -- retaining is just not instituting another variation. After every variation the system is in SOME kind of organized state. The variation puts it in that state. It will stay in that state until there's another variation.

Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Thu, 17 Jan 91 21:29:48 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Selective Retention

Bill Powers (910117b)

>Retention. Are you thinking of retention as some separate process that is
>carried out after a favorable blind variation?

Yes!

>If so, I don't think we need
>that. In the e. coli simulation, there is variation controlled (in
>frequency but not direction) by the difference between a reference signal
>and a controlled variable. When the difference is small, variations are
>spaced further apart. That amounts to "retaining" the good direction. But
>there is no mechanism for retaining -- retaining is just not instituting
>another variation. After every variation the system is in SOME kind of
>organized state. The variation puts it in that state. It will stay in that
>state until there's another variation.

When I first read this I actually thought you were kidding. Then I realized that it may instead reflect a big difference in the type of things we are interested in.

I am interested in changes in adaptive complexity. The only way to explain this without recourse to providence or Lamarckian-type instruction is via blind variation and selective (cumulative) retention. This seems to be the course for all knowledge processes of which biological evolution, the progress of science, and control system reorganization are all examples.

How can there be any growth in adaptive complexity without retaining and building on what has been selected previously? E. coli may not need a memory for its day to day functioning (although I tend to doubt it), but what do you call its genome? How did it get the way it is other than by natural variation and selection? How can a control system (or network of control systems) become more complex and adaptive to new and more complex environments other than by blind variation and selective retention? When you imagine the complexity of the control systems making up an organism and how well these systems fit the environment, how can you explain this puzzle of fit? The probability of it falling together by chance is so tiny as to

be absurd to consider. How can this overwhelming improbability be "tamed" (as Richard Dawkins would put it) by any process other than trying out new solutions and keeping and building upon the best ones?

If the E. coli doesn't try new organizations of its nervous system and somehow retain those that are better, then there is no way that I can see how its behavior/perception can become more adaptive over time. Perhaps this is the case for E. coli, but not certainly not for you or me.

The difference here is that you seem to be primarily interested in the functioning of already well-adapted (can I say successfully retained?) control systems. I am interested in explaining puzzles of fit and growth in adaptive complexity. And control systems are a marvelous example of puzzles of fit that need explaining.

If this is not clear perhaps Hugh Petrie can lend me helping keyboard. He's the one other sole on this planet that I know about who has combined the thinking of Don Campbell and Bill Powers to come up with the best theory of everything (for the life sciences) around today.--Gary

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Date: Fri, 18 Jan 91 10:11:49 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Complexity

Gary Cziko (910117)

>I am interested in changes in adaptive complexity. The only way to explain
>this without recourse to providence or Lamarckian-type instruction is via
>blind variation and selective (cumulative) retention. This seems to be
>the course for all knowledge processes of which biological evolution, the
>progress of science, and control system reorganization are all examples.

>How can there be any growth in adaptive complexity without retaining and
>building on what has been selected previously?

I just thought I'd drop in briefly on this reorganization conversation and take a brief respite from the major reorganization going on in the Gulf. I think maybe the Gary-Bill P disconnect turns on the word "complexity". This is a very problematic concept. It looks like things get more "complex" in the process of developing new organizations that work in new situations. The complexity seems to be a result of storing and retaining and modifying organizations that had worked before. I think what Bill is saying (and I agree)

is that there is no active retention process -- reorganization always works on the available strucure and this looks, to an observer, like increases in complexity. There is no seperate process that retains somethings and not others; why retain anyway -- retention of a change should be as likely as removal of an existing organization.

Our e. coli model doesn't appear to increase complexity because it only reorganizes in one dimension (orientation). There are reorganizations that would look more like changes in complexity -- such as adding levels to the hierarchy. I think there is an interesting question regarding why reorganizations which can, in a sense, add or subtract dimensions to the new result do seem to always add (this is the apparent increase in complexity).
For example, why is it that multi-cellular organisms did not just as often adapt by becoming "simpler" (fewer cells, fewer systems) than more complex (more cells, more systems) -- or is this even true? The fossile record, as I understand it, gives an impression of change, over time, always leading to increased complexity. This, I believe, is what Gary has in mind and wants to understand. I don't know that we have looked at this aspect of reorganization. But I have a feeling that the apparent increase in complexity is a side-effect of how reorganization works. There still most be a selection criterion (a reference for the effects of the reorganization) and the direction of reorganization is random with respect to the difference between reference and perceived result of reorganization. The question is "why do successive, successful reorganizations seem to result in increased complexity?". Why is it apparently harder to dismantle existing organisations than it is to modify or add to them? Any ideas?

I would suggest the obvious possibility -- that what appears to be true is true; reorganization proceeds by modification or addition but not by deletion. E coli reorganizes by modifying itself (randomly) in terms of an existing organization -- its swimming/tumbling system. I suggest that if things change in the environment such that this kind or reorganization did not work then the e. coli is more likely to mutate its swimming/tumbling system than to dismantle it - so that it always swims or always tumbles. The mutation is more likely to be a change that looks like no change in structural complexity (say, e. coli emerge with different error sensitivities, same behavior possibilities) or something that looks like an increase in structural complexity (like a change in its structure that lets it vary its direction in a less random fashion).

OK, this isn't really satisfying but I think its getting at what Gary is wondering about -- why does a sequence of evolutionary/reorganizational adaptations seem to proceed in the direction of increased complexity -- usually (I think there are instances where it has seemed to go on the other way but I can't recall)? This seems to me like a very interesting and, possibly, important question. Or an I just shell shocked?

Regards

Rick M.

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213 336-6214 (day)
213 474-0313 (evening)

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Date: Fri, 18 Jan 91 15:01:00 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Warning -- RSCS tag indicates an origin of SMTPUSER@UBVMSC
From: Hugh Petrie <PROHUGH@UBVMS.BITNET>
Subject: Learning, Reorganization, Selection, and Complexity

Marken (910118); Powers (910117); Cziko (910117); Judd (910117)

You folks really know how to keep a dean from writing memos and talking on the phone. Oh well, this is more fun, anyway. I have a couple of comments on the latest round of discussion of learning, reorganization, selection, and complex-
ity. From my perspective, everybody seems to be concerned with just slightly different problems or issues and I think there may be some hope for getting together.

I think Gary's concern comes from the psychological and philosophical litera-ture. The concern is for the homunculus or the "ghost in the machine." This tradition is one which explains human action usually in some terms or other which require us to assume that for each intelligent action which I perform, there is some higher order "me" which selects from among the alternatives, the one which is best. The question is, how does that happen? In brief, the history of this literature is that no really satisfactory answers have ever been given, and, indeed, the answers which are given always seem to have to place the real "intelligence" or "rationality" or "intention" in some higher order homunculus. There is usually an infinite regress possibility of the highere and higher order homunculi which always seem to have to be postulated in order to explain how action on the next lower level can truly be intelligent
or what have you. This is, I think, the problematic from which Gary is working
Like so many of us, Gary wants to try to solve the problem without the ghost in
the machine. Somehow, it's just "me" doing the things which I do.

Now, the most plausible alternative to the ghost in the machine has usually been some kind of mechanistic account. Then it's just bone and muscles and nerves behaving in a naturalistic world subject to the laws of physics and chemistry and so on. The problem here is that such accounts, primarily because
of the implicit assumption of some kind of straight-line causality in the natural world, don't really seems to be able to account for the intelligence and intentionality which we also seem to see in human action (at least some of the time, and even the "irrational" is often "rational" from a different set of
assumptions of what is being controlled.)

So how do we do this? Lots of us feel that control theory, although a clearly naturalistic explanation, may allow us to account for the intelligence and adaptability of action as well. We needn't fall into either the ghost in the

machine trap or the mechanistic trap. A problem occurs, however, in accounting for the "development" of control systems which, at a static level, seem to account pretty well for intelligent behavior within the problematic outlined above. What Gary is saying, is that a kind of Campbellian blind variation and selective retention mechanism could account for the development and reorganization of control systems--in short, for learning. Any other kind of mechanism to account for development will likely either fall prey to ghost in the machine objections or will require mechanisms which don't seem to work.

If the adaptive mechanism is always blind variation and selective retention, then why try to introduce distinctions like assimilation and accommodation, or functional and structural, or fine-tuning and reorganization into the discussion? All these distinctions, it seems, must rely on blind variation and selective retention. To this point, I think I agree with Gary and would urge Bill not to be too quick to adopt a model which would allow higher orders to alter system parameters without blind variation and selective retention. How would they know which parameters to alter (reorganize)? Is there a ghost in the machine intelligently deciding?

However, Gary, I think you may have misread Bill's short post on selective retention (910117). I think Bill can here be interpreted as offering a more specific hypothesis on how selective retention might actually be realized in a physical system in a very simple way. He is saying that the direction of variation is not controlled (ensuring blindness), but that the frequency of variation is (thereby accounting for selectivity, since frequency varies directly with the size of the operating system's error--that is, if the operating error is small, there will be little variation, ensuring that a reasonably well-adapted system sticks around.) Rather than worry about whether Bill was joking, Gary, I would urge you to adopt that working hypothesis. It would save you the problem of explaining how there was a separate retention system and how it works.

Also, Gary, I think that you are perhaps too insistent on avoiding the ghost in the machine and are dismissing structural--functional distinctions because you believe we must make those on the basis of whether there is blind variation and selective retention at work in the system or not. I think I can accept the blind variation and selective retention you espouse and still want to draw the distinction between fairly major changes in the structure of control systems and fairly minor ones in functioning. Indeed, I may want to do it primarily on educational grounds, i.e., on whether one can assume that undirected experience will probably be good enough to fine tune, or whether directed practice within an hypothesized existing, but relatively crude control system is necessary (the free-throw shooting case) or whether directed interventions to cause disturbances which we suspect will cause major control system reorganizations (teaching students how an electrical current can light a bulb and still be the same size current before and after it enters the bulb, or how to understand

control theory, for example). I think I can grant that all of these occur with
blind variation and selective retention of our neural systems of some sort or other, but that it is still important for thinking about planning educational disturbances to make distinctions like this. If the person doesn't yet even have the system, it makes no sense just telling them to practice.

Joel, I think, at the same time, that I do agree with Gary that we have to remember that WHAT is available to vary at any given point in the process will depend both on the evolution of the species and on the learning which has thus far taken place in the individual. Thus, we wouldn't have to completely start over in the basketball case, but we might have a lot of "gross-tuning" to do.

I think this is also relevant to Rick's comments on complexity. The reason that adaptation seems to get more complex rather than less, is that whatever we have at any given time necessarily works tolerably well, so the variations will be of elements of those systems rather than starting all over. Further, the elimination of systems does occur, more often of whole species if their evolutionary direction seems inappropriate--the dinosaurs, but sometimes of parts of organisms--the appendix (I think).

Cheers

Hugh G. Petrie

==========

Date: Fri, 18 Jan 91 15:01:00 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Warning -- RSCS tag indicates an origin of SMTPUSER@UBVMSC
From: Hugh Petrie <PROHUGH@UBVMS.BITNET>
Subject: Learning, Reorganization, Selection, and Complexity

Marken (910118); Powers (910117); Cziko (910117); Judd (910117)

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Cheers

Hugh G. Petrie

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Date: Fri, 18 Jan 91 15:26:07 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Reorganization and Retention

Rick Marken (910118)

How can it be that all of a sudden I seem to have so much trouble understanding Powers and Marken? 90% of what I know about control theory is the result of reading their books/articles as well as notes on this network and it has been quite revelation. But, boy, we really seem to be miscommunicating on this retention and reorganization stuff!

Let me pick on Marken since Powers is coming down to see me this semester and if I don't stay on good terms with him I'm afraid he'll find some excuse to stand me up like he did last semester.

>I think maybe the Gary-Bill P disconnect turns on the word "complexity". >
>This is a very problematic concept.

Complexity in itself doesn't interest me very much. Mt. Everest is very complex but who cares (except perhaps geologists or mountain climbers). Heap a bunch of rocks together and you have the same type of thing. I am interested in explaining ADAPTIVE complexity. Like the Empire State Building. Like E. coli. Like the organisms that fascinated Darwin, Wallace, and Paley. Adaptive complexity is complexity that does something useful for the system of which it is a part. Adaptive complexity shows itself in what Don Campbell calls "puzzles of fit."

>I think what Bill is saying (and I agree)
>is that there is no active retention process -- reorganization always
works
>on the available strucure and this looks, to an observer, like increases
>in complexity. There is no seperate process that retains somethings and
not
>others; why retain anyway -- retention of a change should be as likely as
>removal of an existing organization.

Why retain? I child needs to reorganize to learn his or her language. Why retain? What kind of question is this? Do you want to start from scratch all over again each time you are confronted with the same problem? I'm sorry, Rick, I just can't see where you're coming from here.

> Our e. coli model doesn't appear to increase complexity
>because it only reorganizes in one dimension (orientation).

Now I'm even more confused. Bill was arguing that reorganization is quite different from the operation of well-developed control system and now you say that the normal behavior of E. coli's gradient seeking is reorganization when even I would be willing to say that little if any major reorganization is involved here. Swim or tumble. Where's the reorganization?

>For example, why is it that multi-cellular organisms did not just as often
>adapt by becoming "simpler" (fewer cells, fewer systems) than more complex
>(more cells, more systems) -- or is this even true?

Organisms seem to become more adaptively complex because their environment also gets more complex because they (and evolving organisms) are also part of the environment (Lewontin's closed loop). Yes, this looks like positive feedback, but I don't want to start that topic again. But some people talk about two evolution strategies. One is to keep it simple and reproduce furiously (bacteria, viruses). The other is to get complex and invest a lot in relatively few offspring (Homo sapiens). The results are not yet in on which one will eventually dominate, but events in the Gulf are not encouraging for our side.

>The fossile record, as I
>understand it, gives an impression of change, over time, always leading to

>increased complexity.

No, I believe that there can also be decreases in adaptive complexity. I understand that parasites may be simpler than the non-parasitic ancestors from which they evolved (although the parasites are certainly better adapted to their new, "simpler" environment). Fish and lizards that crawl into and stay (for eons) in dark caves lose they eyes since in this environment they provide no advantages, only disadvantages (e.g., a route of infection). But they may develop other sensory systems to compensate.

> The question is "why do successive,
>successful reorganizations seem to result in increased complexity?". Why is it
>apparently harder to dismantle existing organisations than it is to modify or
>add to them? Any ideas?

It probably is more likely that a fine-tuning of a given organization will be more adaptive than dismantling it and starting again from scratch. This is why we see (selective) retention. There are also physical reasons. Evolution can't be turned backwards any easier than cream can be stirred out of coffee.

>I would suggest the obvious possibility -- that what appears to be true is >true; reorganization proceeds by modification or addition but not by deletion.

I agree, but how can you say this after arguing with Bill that there is no retention involved? How can modification and addition work if there is no retention from before to work on? And why bother if what you are going to wind up with is not going be retained?

Another reason for modification and addition instead of deletion is that memory is cheap. It doesn't seem to cost flowers much to have 2 or 3 times the amount of memory space in their genome than we do. We've got plenty of neurons to spare. So we opt for write once, read and modify many times. Why erase if there is lots of room and if you may need it again? People with big attics, basements and garages seem to accumulate more junk than apartment dwellers.

Neither evolution nor control system reorganization has to move in the direction of increased adaptive complexity. In fact, much of evolution may be a result of neutral, drifting processes. But such processes cannot explain increases in adaptive complexity, the remarkable fit between organism and environment (including other organisms) that is so characteristic of life.--Gary

P.S. This is a lot of fun, but I do need to spend more time on getting my book finished so that I can straighten out all you guys in one big shot. I would love to see some other topics back on the network. How about Bar-Kana? Now THAT was an interesting dialogue, but I'm afraid that with a name like his he may well be preoccupied with the Gulf events. Itzhak, are you still there?

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Date: Fri, 18 Jan 91 16:31:15 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Soleful Memory

Cziko (910117)

Now I know I going bonkers--sending messages to myself.

After writing the message which contained this last night at 9:30 I went home. Getting into bed around midnight, I received a strong error signal. Just a little thinking revealed that I used "sole" instead of "soul". When I got back to my office this afternoon and checked my outgoing mail copy, sure enough, it was "sole."

>He's [Hugh Petrie] the one other sole on this planet that I know about who >has combined the thinking of Don Campbell and Bill Powers to come up with >the best theory of everything (for the life sciences) around today.

Why couldn't I catch this error when I wrote it? Why did it pop up three hours later? I also do the same thing when I try to speak other languages. I remember 5 hours too late that I forgot to use the subjunctive, etc. This should give you memory people something to think about.--Gary

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Date: Fri, 18 Jan 91 15:57:10 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Selection, complexity, and retention

Gary (910118) Cziko:

Thanks for the response to my article; you've gotten things stirred up pretty nicely now. Unfortunately, I have to leave work now so I can't get back to you for a couple days. But I'll think about this stuff over the weeked. One thing I would suggest -- let's try to think of this stuff in terms

of phenomena and models. What is the phenomenon we are trying to explain? Evolution? Learning? These may have similar underlying processes but still, the processes may be different. Once we know the pehenomenon then what is the model; reorganization, complexity increase and decrease and retention might all be understood better if we can point to parts of our model and say "that's what I call retention (reorganization, complexity change)". I know that we retain changes -- and there must me a mechanism responsible for thing retention -- it just might not be something you would want to call a "retention mechanism" when you look at the mechanics of the model; that is what Bill, I think, was saying about the e. coli model (which may not be relevant to what you are interested in but then it would help if you would describe your model of reorganization, say). In the e. coli model the successful adaptations (direction) are not stored but change in them is variably delayed depending on their succeess. There is a process of "building on" to an adaptation because, in the gradient, e. coli's current positions is the result of previous adaptations. So its current position "retains" the results of previous moves but there is no retention mechanism per se.

Therrre may be a need to have a retention mechanism to explain the kinds of adaptive changes in morphology that you are talking about -- but I doubt it; But I think the question about why adaptations appear to become more complex is still interesting to me. I think, howevr, that we have to have a very clear definition of "complexity" and a clear model of reorganization before we can start asking interesting questions about it. My understanding of reorganization in a hierarchy of control systems, however, gives me no basis for understanding why a successful reorganization now should result in a more complex structure of control systems than earlier reorganizations (if that is what complex means).

Sorry to go on so; I like this topic because I don't know nearly enough about reorganization and I am very interested in biological evolution. So I hope the discussion continues; but, for my sake, try to formulate the ideas in terms of models (mechanisms, rube goldberg devices, whatever) that produce (or might produce) the phenomenon we observe.

Hasta Luego

Rick M.

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Date: Fri, 18 Jan 91 21:17:52 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@VMD.CSO.UIUC.EDU>
Subject: variation and retention

Gary, Hugh, Rick, et. al. (910118) --

I vote that we just go on contributing our pieces and let Hugh Petrie put them all together so they make sense. As Hugh intimated, our various approaches here are susceptible to integration, but we have to find the common demoninators first, meaning that we have to explore what each of us means in more detail.

Gary, I am not saying that there is no retention: I am saying that there is no SEPARATE MECHANISM of retention. I used E. coli as an example because its tropic behavior seems about as primitive as a control system can get,

and at the same time very close to the mechanism I envision for reorganization. The analogy I wanted to draw was between E. coli's way of moving up a gradient without any systematic means of controlling direction of movement, and (say) the brain's ability to build a new level of control without any systematic means of designing a new level that is appropriate to the environment. The analogy is between a tumble and a reorganization or a mutation.

As I say, I don't reject the concept of retention. I'm trying to do a Gestalt-switch here like the one with reinforcement. Behaviorists have assumed that when a reward is given, it does something to the organism to fix in place the behavior that produced it. It "reinforces the behavior." This makes the reward into a cause. But if you say that it's the LACK of reward (the error) that drives the behavior, then the reason the behavior ceases to change when the reward is given is that there's no more error to drive the behavior. Reinforcement is removing error, not fixing behavior. Reinforcement is an effect of behavior, not a cause. I'm saying that retention is simply the failure of another change to occur -- an effect, not a cause.

My proposition is that blind variations are driven by error. As long as there is error, the variations will continue. If a variation results in an increase in error, another one occurs immediately. If the result is a decrease in error, the next variation is postponed. And if the error disappears, the next variation can be put off a very long time. When variation ceases, change ceases, giving the appearance that some "retention mechanism" must have come into play. So the variations are not a cause, but an effect of error. I'm saying that all the processes of life are built around error-correction. At the most fundamental level, the error-correction process entails varying the spacing of blind changes -- mutations. Mutations are goal-directed not by being aimed at a goal but by being stopped when a goal is reached.

To round this off, what is a "variation?" By this I don't just mean a change in behavioral actions -- standing straight or standing slumped, holding your arm up here or down there. Most such changes can be accounted for by disturbances and by actions of higher-level systems. When I talk about variations in connection with reorganization, I mean really fundamental variations, such as changes in the length of the neck of a giraffe. If there's a mutation that changes the neck length recipe in the genome, and there is no further mutation, the neck length of all successive generations will be the new length, longer or shorter. No separate mechanism is needed to "retain" the new length in succeeding generations. We don't need a mechanism that looks at the change and says "Ah, that's a good one, I'll keep it." All we need is a mechanism that stops the changes. That might as well be the same mechanism that starts them.

A change in neck length doesn't occur because the giraffe-species wants to reach the leaves of trees that are becoming taller. The reason for change has nothing directly to do with the trees or the necks. It has to do with some CONSEQUENCE of not eating leaves. Traditional Darwinism says that this consequence is just starvation. The shorter giraffes starve before they can make more short giraffes. But I'm proposing a subtler mechanism. Giraffes, after all, can eat grass. But if there is a systematic change toward being tall enough to eat leaves, then by some route or other eating leaves must have led to a lower mutation rate than eating grass. In other words, buried somewhere in the genetic machinery is a control system that

is disturbed by not eating enough leaves. It acts when error occurs by raising the rate of mutation. This increases the variations in neck length (among other things, I presume). It does not increase neck length; it just makes a change in length, longer or shorter, sooner. If, after the mutation, the error is larger, fewer generations will pass before another mutation takes place. If the error is smaller, the next mutation will be postponed. Because it's the eating of leaves that is correcting the error, the mutations that lengthen necks will be "retained" simply because the next mutation will be put off.

Of course the protogiraffe might mutate in other ways. Its biochemistry might change so that eating grass can correct the same error that eating leaves used to correct. In that case, if it's built like an okapi, it might mutate back down again to where the grass is easier to reach and it can no longer reach the leaves.

The principle here is that "survival" is not the only possible selection criterion. I'm talking about selection factors INSIDE THE ORGANISM, where the selection factor is simply the minimization of the error that is driving the process of blind variation.

A species that works the way I'm proposing wouldn't have to leave a lot of dead bodies around in order to change its form. Some lines of descent would make the change quickly (lucky string of mutations) and others in the species might detour through paths that temporarily make the error worse. But if there's only one objective method of minimizing the error, all the lines will converge to the same final form, near enough.

Lamarck couldn't have discovered characteristics that can become inherited because, in addition to taking too naive an approach, he didn't wait long enough. The kind of genetics-level reorganization I'm thinking of might take a hundred generations to make an appreciable change, or even many more considering that the changes are random and go the wrong way half of the time. Not every variable, furthermore, relates to the basic kinds of controlled variables I am imagining. Cutting off tails may stress a mouse, and continued stress over many generations might lead to detectable changes in mutation rate, but what mutation of organization or form could affect the rate at which tails get cut off generation after generation? If the experimenter ALWAYS cuts off the tails, there's no control process possible. There's no way to tell if you're getting warmer or colder, and that's essential if random changes are to be converted to systematic long-term effects through effects of the related error signals on mutation rate.

There's a lot of tangential stuff in the above, so maybe I should repeat the main proposition: retention is not an active process, but the natural result of slowing or stopping the process of blind variation. Retention is not-varying.

Last word, to Hugh Petrie. I think that higher systems DO alter parameters of lower systems without any blind variations. In order to make an arm stable when you pick up a weight, the damping coefficient must immediately be raised, and it is raised. There is no series of experimental adjustments of the damping coefficient: it simply gets bigger when the load's mass increases, and smaller again when the extra mass is gone. When there's a reversal in a tracking task, one-half second later the control system gets its output sign reversed to compensate. Immediately, without trial and error. I think we have to accept control of parameters as an empirical

fact.

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Date: Sat, 19 Jan 91 13:30:30 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: onward?

Now that everyone is gleefully thinking about learning and change, can I throw some more wood on the fire? Unfortunately the nature of a thesis means that at some point I have to pound out some rudimentary understanding of something like learning and continue on. What I'm continuing on to is motivation and then attention. The latter has already been mentioned as being a weak area in CT thought.

But I'm more concerned about the former right now. In CH.1 in the Robertson/Powers Psychology text it is correctly pointed out that an understanding of control systems means that learning/motivation/perception are all aspects of environment-control. So how we talk about these is changed. From the perspective of the reorganizing system, would the motivation to "learn" be survival? In that way we can't talk about motivation apart from "reorganization"; the system is initiating changes because the intrinsic error "motivates" it to!(?). And then we must look at a particular level of the hierarchy when discussing "motivation" -- what does it mean to say I'm motivated to increase my free-throwing percentage, to get an "A" instead of a "B", to become like a native-speaker in a L2. Is motivation nothing more or less than an error signal? Am I "motivated" to reconcile differences with my wife because my system concept of marriage as a lifetime decision is experiencing error? An unmotivated system is one in which the error is small. Can E. Coli be as motivated as me (complexity aside)?

Joel

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Date: Sat, 19 Jan 91 17:28:04 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Motivation

Joel Judd (910118) --

With reference to "motivation" see Bateson's "dormitive principle." The dictionary I have gives as the meaning of motivate "to provide with a motive or motives." OK, motive: "1. Something that prompts a person to act in a certain way or that determines volition; incentive. 2. the goal or object of one's actions."

The word motivation is essentially empty of meaning. It refers to a mysterious something that causes us to move. We move, therefore there must be a motivator. The more energetically we move, the stronger must have been the motivation. In reference to a goal, it animistically endows the external world with the power to cause coordinated behavior.

Control theory gives us a detailed hypothesis about the sources and aims of behavior. We don't need the term motivation at all. The way this term is used in "scientific" (-sounding) discourse can cover any cause of noticeably energetic behavior: an opposing disturbance or obstacle, an underestimate of the actual effects of action, a high setting of a reference signal, a high error sensitivity, or a loss in effectiveness of behavior on the controlled variable. All of these conditions can lead to energetic behavior, which by custom we would then say is "highly motivated." But there is no single referent for that term.

Many people use the term motivation as if it's a substance or force inside a person that just sort of generally pushes behavior to higher levels. There is no such substance or force; the image represents an unworkable model. There is really very little difference between saying that energetic behavior is highly motivated and saying that it's inspired by God.

To say that "A is motivated to do B" tells us that A does B, but not why.

Psychology is totally clogged with terms like motivation, which appear to explain in causal terms but actually only describe effects. Some of these terms can be translated into some collection of control-system phenomena. But when so translated, they are found to be ambiguous: they leave room for many quite different explanations of the same effect.
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Date: Mon, 21 Jan 91 12:55:24 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Retention, Evolution

Powers (910117b); Marken (910118a, b); Petrie (910118); Powers (910118):

I'm both relieved and embarrassed. Relieved that we are in fact talking the same language and I think now that I do agree with what you were saying. Embarrassed in that I didn't think enough about what you were saying before responding. This is one of the dangers of e-mail!

Somehow I didn't think enough about the term "separate retention mechanism" with respect to reorganization. No, this need not be separate. Upon further reflection I can see why it shouldn't be separate and I now see some potential problems with the use of the terms "selection" and "selective retention."

In biological evolution variations appear in the genome and these are retained for the individual's life. They are retained for the species, however, only if the individual passes them on to offspring. This is the sense in which the term "natural selection" is used.

So Marken and Powers want to say, in effect, that the a new variation is in a sense "retained" the moment it appears and its retention continues until it is replaced by another variation since the first variation did not remove all error. This is fine with me.

I think Darwin got himself into the same sort of trouble as I just did. Many people read "natural selection" in the first edition of the _Origin_ as meaning that some supernatural entity was making the selections,

deciding which variations should be kept and which should go. Huxley and Wallace asked him to give up the term "natural selection" altogether to avoid this misinterpretation and substitute instead "survival of the fittest." Darwin then used both. A separate retention mechanisms might imply some separate intelligence watching how things are going and then deciding to keep the variation or trying another one. No, I don't like this idea at all. I need to think some more about all this, but I now appreciate your points and think that you were right all along, just misinterpreted. Sorry for getting so excited about this, but you caused a big error when I thought you were saying retention wasn't needed.
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Powers (910118) on a control theory view of evolution.

Let me say a bit about this. Bill, would you believe that in spite of talking to you at length about evolution, it is only now that I finally feel I really understand what you've been trying to get through to me. Yes, it seems to make sense to expect that mutations should somehow put off when things are going well and increased when they are not. But now that I better understand your perspective, I think I can more clearly see some potential problems.

>The principle here is that "survival" is not the only possible selection
>criterion. I'm talking about selection factors INSIDE THE ORGANISM, where
>the selection factor is simply the minimization of the error that is
>driving the process of blind variation.

Of course you realize it is not just survival that is important, but surviving long enough to find mates and having offspring who will also survive to . . . etc. Biologists call this "inclusive fitness." But that aside, I still don't see how you can dispense with Darwin's hammer. If things are going well, Darwin's hammer will not be needed since there will be no mutations. But if there is error (animals starving) mutations will increase. Keep in mind that the most successful individuals must be experiencing error for this to work.

But the only way the better mutation can be selected is through the elimination of those which are less fit. Yes, less dead bodies may be left around (although why should evolution care about this?), but it is still through the survival of the fittest (or elimination of the less fit) that that adaptation can work. I just don't see how any "internal selection" process can replace the "external" one. Genes only make a difference insofar as they cause phenotypic differences which fit or don't fit the environment.
But I like the general idea, but perhaps only for asexually reproducing organisms.

This is because I see problems with sex. With sexual reproduction there is always a good deal of variation, regardless of mutations. So I don't think you can say that all genetic variation is error driven. Even if my wife and I are experiencing no error, there is no way that we can reproduce clones of ourselves through natural reproductive methods. Perhaps you want to see this as the normal amount of healthy variation and use the mutations as the major driving force for "real" evolutionary change. But then you should consider that David Goldberg, who uses genetic algorithms on computers, uses mutations as not a source of new variation but rather as a way of returning to old variations which were prematurely discarded.

Goldberg uses the crossover (sexual) variation as the source of new ideas and mutations as a type of "check with tradition." If this is how biological evolution works, the control theory perspective seems problematic since the crossover (recombination) type of variation is built right into the system--genetic recombination appears independent of any possible source of error.

Sexual selection might also pose a problem. Peacocks have elaborate long tails since peahens originally preferred peacocks with slightly longer tails who were therefore more sucessful in attracting peahens and having offspring consisting of peacocks with longer tails and females with more preference for long-tailed peacocks who had offspring . . . Where is the error here? The error should be experienced by the peacocks who don't have neither long tails nor mates, but these are not the ones who are going to reproduce and so the greater genetic variability they would contribute (since they are in "error") never gets into the population.

Finally, Darwin's important principle of superfecundity must be considered. As Darwin realized when he read Malthus (as did Wallace), species produce MUCH more offspring than can possible be supported by the environment. So there will always be considerable error since normally only a very small proportion can survive to reproduce. The dead bodies are just unavoidable. I could caricaturize your position by saying that you would like to envision some periods of time when everything for a species is just going fine. All offspring survive, everybody finds a mate, nobody gets eaten, and no error and no (or little) genetic variability appears. Then when new disturbances arise, the species responds with increased variablity to find a new solution (and it is only now that the dead bodies start piling up) and then things are just hunky dory again for a while (just like a control system). Even though I am not a biologist and no expert in evolution, from what I do know this just doesn't seem to be a possibilty. There is always variation and there is always error and the individuals experiencing the most error should be the least successful in reproducing.

--Gary

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Date: Mon, 21 Jan 91 14:02:18 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Dennis Delprato <USERXEAK@UMICHUM.BITNET>
Subject: Motivation, Spooks, & Control System Epistemology

REALLY FROM Dennis <DELPRATO@UM.CC.UMICH.EDU>

In my view, Bill Powers's recent (910119) comment on motivation needs to be thoroughly digested. It so contacts the crux of my interest in control theory that I am led to underline every word of Bill's commentary and to briefly place his position into the framework that I

see for behavioral science of the future.

Complex socio-cultural-politico developments that go back to the decline of Hellenic culture have given us the hoaxes of mythical internal and external determiners of psychological events. First, completely naturalistic Aristotelian soul was transformed into a substantive nonspatiotemporal entity. This purely invented construction, albeit itself the product of naturalistic events and processes (human behavior), became the bedrock of institutions promising humans escape from intolerable conditions of living. Ever so gradually, as conditions of living began to become more like those of Hellenic culture, thinkers began the long, excruciating process of secularizing soul. Soul was verbally transformed into mind, consciousness, experience, and mentality. Eventually, some thinkers began to consider that the referents to soul could only be something like "behavior." We had the great struggles, beginning in the second half of the 19th century and continuing up to the present, over whether the subject matter of psychology was mentality or "behavior." Just as there was never unanimity over just what mental events were, those promoting "behavior" have never been able to decide on what the events of behavior are. Thus, today we continue to find behavior defined as muscle twitches, glandular activity, and movement in space.

It is now possible to identify a small, but growing, motley collection of thinkers (as yet quite unaware of each other) who are working on a relatively coherent alternative to the two major psychological approaches (mentalism and behaviorism) that have received the most notoriety to date. Bill's commentary on motivation nicely exemplifies how control system theory fits into this movement that is tantamount to re-naturalizing our conception of humans.

Both mentalism and behaviorism retained hypothetical mystical causes of behavior. Motivation is but one of the numerous such constructions that authorities have advocated over the years. The tendency to remain tied to nonspatiotemporal soul as the foundation for human psychological activity has been so powerful that we have never had a psychology; instead, we've had spookology. I suggest that one task of the many that control system workers could fruitfully consider is examination of the controlled variables of specialists in psychological research and theory. That is, individuals making statements about human behavior are control systems. What are they controlling for, and what is the ontogeny of their controlled variables, e.g.?

I suppose this takes us to control system epistemology, adequate development of which may be a sine qua non for large-scale movement away from the hoary mentalisms and behaviorisms.

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Date: Mon, 21 Jan 91 11:06:49 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Selection

Powers (910118) -- explains his vision of a control approach to evolutionary change driven by error rather than survival. I always knew that the idea of mutation rate varying in proportion to error was a significant change from the Darwininan, constant mutaton rate model. But Bill's post made it clear to me that there is another suggestion that control theory has for the evolutionary biologists -- instead of "survival of the fittest" it might be "least average intrinsic error of the fittest". Not quite the same ring but, still, it may be right!!

I still would like to hear more detailed comment from Bill or Gary or Hugh or any other evolution freaks about why changes appear to move toward increased
complexity. Is there really an evolutionary increase in complexity (obviously there is to the extent that single celled systems had to preceed multicelled one's). But is the fossil record really one which shows more complex organizations following less -- or are there just as many cases of simple (in some sense) systems having descended from more complex systems? I seem to be unable to get my brains around this too well. It seems that most of you seem to accept that complex inexorably follows simple; but given the model of evolution that Bill described, I see complex organizations as likely as simple ones; whatever variation works should be "retained" whether it results in an organism that seems simpler or more complex than the one that started reorganizing.

Best regards

Rick

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Date: Mon, 21 Jan 91 13:33:42 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Increase in Complexity

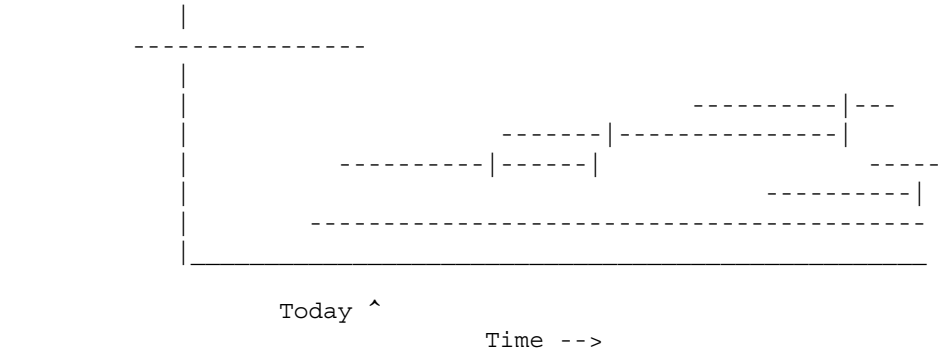
Marken (910121)

>I still would like to hear more detailed comment from Bill or Gary or Hugh
>or any other evolution freaks about why changes appear to move toward increased
>complexity.

Rick, here's one conceptualization you may find helpful.

Imagine that there is a certain minimum level of complexity needed for life. Less complex than this level and the organism cannot survive and reproduce. Now this organism mutates. The "less complex" mutations will not survive to reproduce. The more complex ones will if they are ADAPTIVELY complex. Offspring which are the same as the parents may also

survive, but maybe not if the new competition is too much for them. Less complex mutatins CAN survive if they are descended from more complex organisms, as long as they are still above the minimum level. This continues and so we have something that looks like this with the y axis being complexity and the x axis evolutionary time:



Since there is a minimal leve of adaptive complexity needed, and since more complexity is sometimes advantageous (although sometimes less complexity an sometimes survive as well), there is a general trend in the biosphere to higher coplexity. But this doesn't mean that all organisms must become more complex and that less complex forms are necessaritly eliminated. But today we have more complex organisms then ever existed before, but we also have some of the the less complex ones as well. Of course, the figure is not accurate since most species that ever existed are now extinct. But it does suggest why (a) there appears on average to be an increase in adaptive complexity, (b) there can also be decreases (although not below a certain minimum) and (c) why we see such a mix of organisms, from simple to more complex around today.

I hope this helps. I thought you control theory types would like this since it doesn't depend on positive feedback in the form of evolutionary arms races or runaway sexual selection (see Dawkin's _Blind Watchmaker_ for these latter ideas).

This idea is taken from a chapter by an author in a volume edited by John Dupre on optimization in evolution. I can get you the reference if you want it.--Gary

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Date: Tue, 22 Jan 91 15:31:28 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG

Subject: hello

Hi. Is anybody out there? Anyone get my last message? I'm not getting any ACK after sending these.

Rick Marken
marken@aerospace.aero.org
(posted at 3:30 pm PDT)

Date: Tue, 22 Jan 91 12:12:09 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: selection/complexity

I am sending this out again-- I got no reply in my mailer so I hope this is not redundant:

Gary (910121)

Thanks for the complexity increase graph. I have some questions (of course). It seems that each new iteration of this process must have a new threshold for the amount of complexity needed for survival. That is, once an antelope becomes a girafe there is some new minimum complexity level for giraffes that is higher than the minimum needed for their predecessor. This just seems to push the question back to (or "begs the question") why does the new iteration need a higher minimum level of complexity to survive? A model that explains increased complexity should produce increased complexity over generations as a side effect of the underlying mechanism of the model. So the "increasing threshold of complexity" model is not really a model since it really just describes what we observe.

But before we start modeling, I think it is important to know what it is we are
trying to explain. I guess what I think is going on is that the fossil record shows there are "layers" of organisms and that organisms at each layer can be traced, morphologically, to ancestral organisms in earlier layers. The later organisms seem to be at least as complex (in some sense) as their ancestors. The "simple" organisms living now are, I presume, examples of decendents of simple organsisms that just havn't changed much. Thus, horses and e. coli are ancestors of some common single celled entity (the first cell);
but e. coli has changed less than the horse. What I think you don't see is a sequence in the fossil record where you get a simple ancestor going to complex decendant and then a simple decendent (as often happens in human families; simple grandpa, complex son, simple grandson -- Mozart's dad [Leopold was only simple relative to his kid], Mozart, Mozart's kid).

Of course, the problem is defining "complexity". I think I can perceive that a dog is more complex than a sea slug -- but how; is it number of cells? neurons? degrees of freedom of motion? Maybe some organisms are complex in one way and not in others. What is it that seems to be increasing as we move from first cell (or cells) to the critters running around today? Help!

Other little remarks:

I didn't get any mail today. Hope my mail feed is up.

Tom Bourbon -- is there any news about the meeting? How are you doing?

I am working on my promised paper on behavioral/perceptual hierarchies but I'm afraid its been tough for me to concentrate what with world events as they are. I will try to post something on it by next week.

Best regards to all

Rick M.

****

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Date: Tue, 22 Jan 91 10:30:16 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: complexity/selection

Gary (910121)

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Rick M.

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Date: Tue, 22 Jan 91 05:57:00 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: complexity & control

Some comments regarding COMPLEXITY and CONTROL:

The locomotion of E coli seems a good place to consider the question of complexity and control, because E coli's methods of locomotion appear to be very primitive or simple, and simplicity is a unique degree of complexity: the least complex.

As I understand it E coli are continuously spinning, except for brief bouts of tumbling. The spinning propels them forward. The tumbling alters their heading relative to environmental coordinates, with the new heading being a random variable. In effect, the only output VARIABLE is the rate at which this heading changes (i.e., the rate at which tumbling bouts occur). By increasing this output when sensed food concentrations LINGER below (also above?) some acceptable range of values (what exactly are E coli's sensory abilities, Bill, Rick, Tom?) the E coli controls the EFFECTS of its inevitable spinning; that is, the heading is altered so that, at least eventually, the inevitable spinning tends to propel the organism in an ERROR-REDUCING DIRECTION. Hence, what is being controlled is the POLARITY of the feedback, as well as the value of the sensory input. Therefore, E coli may be thought of as illustrating either reorganization (controlling polarity) or the control of sensory input, or both. However, thinking of the

effects of spinning is one thing and thinking of the cause of spinning is another. In what sense, is spinning error driven? Must not spinning be error driven if tumbling is to be regarded as the control of polarity (i.e., reorganization)? Anybody?

As long as the polarity of feedback loops are negative, the complexity of an organism's output will reflect the complexity of the organism's environment. Once a boy has learned to ride a bicycle the layout of the paths and sidewalks determines the complexity of the routes he takes. Once a girl has learned to read aloud, the sounds she makes vary as a function of the book she has in hand. The boy and girl are each doing something relatively simple (cycling or reading); the complexities of their particular actions are largely dependent upon the particulars of their respective environments, and these particulars may vary endlessly.

Note that in a hierarchical system, the reference signals of subordinate loops serve at the mercy of two masters: the reference signal of the superordinate loop and that superordinate loop's environmental disturbances. Hence, the complexity of the hierarchy's reference signals will reflect the complexity of the environmental circumstances. We must guard against the tendency, all to commonplace in cognitive psychology, of representing (or re-presenting) the environment's complexities in the organism. For instance, consider the complexity of the movements of the individuals in Bill's Crowd program; the individuals themselves are not complex.

To shoot a basketball is to launch a ballistic projectile. What one controls is the launch. The sensations defining a successful launch have to be discovered and retained as reference signals, but, it seems to me that the essential control systems are already in place. The discovery of the best reference signal from among a variety of imperfect reference signals is by a trial and error process (resembling E coli's tumbling). Further, what Gary is noting is that our "trial and error" learning (as opposed to E. coli's tumbling or Darwinian evolution) is not altogether random or blind. We can interpolate between several prior reference signals, one which proved to be too far to the right and another which proved to be too far to the left, implying a memory for several prior reference signals.

I have a feeling I am going in circles. The central question appears to be: To what extent is learning a reference signal the development of a control system? Anyone?

Warm regards, Wayne

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Northern Illinois University
DeKalb IL 60115 Bitnet: tj0wah1@niu

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Date: Thu, 24 Jan 91 09:17:24 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: DAVIDSON@UIUCVMD.BITNET

Subject: Where are you, Gary Cziko?

First, sorry if this reaches everybody on the CSG network, but Gary Czkio's email appears not to be working (cziko@uiucvmd). Gary: if you are on another machine, now, please let me know. Please also let Brian Lynch know so he can change the language testing network address list to your correct identifier. Brian is at: imi7bkl@uclamvs
Thanks, -Fred Davidson

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Date: Sun, 20 Jan 91 20:41:39 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: MCI Mail

From Bill Powers: This is partly for David Goldstein, because the system I'm on won't allow sending to a fax number (Northeastern Illinois University, where Dick Robertson will soon also be signed on). I get an error message back when I sent to CSG-L, specifically refusing to send to David.

Some information on sending and receiving via MCI-mail.

I've found some information from the bulletin board on the Northeastern Illinois University system. Here it is, condensed:

To get help from MCImail, send a message (what kind, I don't know -- I suppose HELP) to

0002671163@mcimail.com

The general way to address mail is 000nnnnnnn@mcimail.com, where nnnnnnn is the recipient's MCI-mail address.

I quote the information for sending from David K. Ely, dely@NRI.Reston.VA:

> Users of MCI Mail can also send messages to the Internet. At the
>"Command:" prompt, type "create <carriage return>". Then the user performs
>the following (NOTE the "TO:", EMS:", and "MBX:" strings are prompts by
>MCI Mail):

```
>        Command: create<return>
>             TO: David K. Ely (EMS)     [Name of recipient]
>            EMS: INTERNET               [ use caps]
```

>This address is translated to

> "David K. Ely" <dely@NRI.Reston.VA.US> by the Gateway.

>Mail sent from MCI Mail to the Internet is charged by MCI Mail [see
>later].

>One final note: Feel free to use the gateway as often as you'd like, but
>be forewarned: The Gateway is still not considered fully operational;
>sometimes mail will be delayed (usually less than 24 hours).

> If you have any more questions, or if I can be of further assistance,
>please feel free to email me.

>David K. Ely
>Manager, EMS Systems
>Corportaion for National Research Initiatives (NRI)
>Phone (US) 703/620-8990>
>Internet mail: dely@nri.Reston.VA.US
>MCI Mail: dely
(account name, can be used in place of id number)

I called MCI to get rates. You do not have to be signed on with MCI to use MCI Mail.
MCI Mail itself costs $35 per year basic charge
There are no charges for receiving mail. Sending mail costs
0.45 for 1-500 words
0.75 for 500 to 2500 words
1.00 for 2500 to 7500 words, and
1.00 for each subsequent 7500 words.

I've sent for Easylink information (Western Union) and should get it next week. We used Easylink at the Sun-Times because it was the cheapest of all ways to send email.

Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Mon, 21 Jan 91 04:10:44 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Correction, MCI Mail

Left out a line. the correct sequence to send from MCI to Internet is:

Command: create<enter>
TO: David K. Ely (EMS) [name of the repicient]
EMS: INTERNET
MBX: dely@NRI.Reston.VA.US [this is where Internet address comes in]

the rest still seems to be correct.

Bill Powers uppower@bogecnve
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Date: Wed, 23 Jan 91 07:39:35 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: retention, evolution

Gary Cziko, Rick Marken (910121) --

Gary says:
>But the only way the better mutation can be selected is through the
>elimination of those which are less fit.

I agree with the basic principle that elimination of the less fit leaves the

more fit to propagate. I'm not trying to say that natural selection doesn't work or that some other principle should be substituted for it. I'm trying to add a level of control that can achieve selection without requiring elimination
of the less fit, and which can be far more effective in achieving new degrees of fitness. This capacity, I presume, would have been an outcome of the underlying Darwinian process.

The model I'm proposing rests on assuming two functions. One is the ability of a system in the organism, at the level of biochemistry, to increase mutation rates over the background level. The other is the ability to sense the state of
some variable that, in effect, reflects the internal status of the organism and, by comparison with a reference signal, provides information needed to either advance or postpone the next mutation (i.e., vary the rate). These abilities alone are sufficient to give a highly systematic effect to mutations that would otherwise have no systematic effects in the long run. The E. coli method of locomotion, which uses this principle, shows that it is nearly as effective as systematic control.

This hypothetical internal variable effectively substitutes for survival as an indicator of adaptive success, in much the same way that pain signals substitute for actual damage as indicators of external threats. Campbell might refer to such variables as "vicarious" indicators. Of course WHICH variable is to be sensed and will prove to be a good indicator of adaptive success must be decided not systematically, but through simple Darwinian selection. If environmental conditions change so much that one variable is no longer an adequate indicator, again natural selection will step in and select for organisms that use a different variable.

The basic concept is much simpler than my description of it -- it's difficult to wrap words around any closed-loop process. We have to remember that environmental "pressures" are not abstract; they consist of specific effects of
an environment on an organism, that have specific deleterious consequences inside the system such as lack of substrate raw materials, dehydration, excessive temperature, and so on. Threats to survival affect specific physical variables inside the system, changing their values. There is no reason why even
a biochemical system can't adapt to sense the states of these variables, and to
find some action that will oppose the deviations of these variables from reference states that the experience of the species shows are sufficient to assure survival. The simplest kind of "action" possible is simply to change at random -- mutate. Control can be achieved just by varying the spacing of mutations.

A completely primitive system that does not monitor its own state at all can't know of environmental pressures: it can only survive or succumb. A more advanced system, however, can come to sense not the environment, but the effects the environment is having on it (we're talking about processes at a level where exteroceptors don't exist). It can take measures to oppose those effects before they become dangerous. This prevents the environment from exerting the kind of pressure that makes survival a relevant concept. As long as this more advanced kind of system continues to work -- as long as the internal indicator continues to represent the organism's actual status correctly -- there need be no occasion for Darwinian selection. Even lines of

descent that begin with a mutation that makes the error worse will consist of individuals that survive to reproduce, but another mutation will occur sooner because the error is now larger. Thus all lines of descent could conceivably find a sequence of mutations that restores the variable to its reference state,
without any individual having been weeded out before reproducing.

An implicit assumption here is that the self-induced mutations have only small effects. One mutation is insufficient to make a life-or-death difference. Only a long run of bad luck, in that case, could make the error continuously worse until finally the line succumbs and Darwin's Hammer comes down.

Even when lines of descent that follow different paths of mutation manage to restore the critical variable to its reference state, there is no reason to suppose that the superficial characteristics that then exist will be the same in all lines. There would normally be many alternatives that would work just as
well. All we can say is that the elevated rate of mutation will fall back to the background level when the critical variable is restored to its reference state. It's therefore conceivable that some of these lines of descent could end
up in radical alterations of the original organism, even a new species, all without ever requiring that any individual die before reproducing.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Wed, 23 Jan 91 23:04:55 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: complexity

Wayne Hershberger, Rick Marken (910123) --

To start with the last: Wayne, E. coli has a sensory system that starts with recognition of molecules at the cell surface. This process releases internal "messenger" molecules that represent (by their concentration) the concentration of external substances. There are also dynamics in this sensing process that permit sensing time rates of change. What happens to the messenger molecules (perceptual signals) after that is not known. E. coli can sense both attractants and repellants (meaning, of course, that it can sense substances for which it has both positive and negative reference levels). I believe the number of discriminable substances known to be involved in chemotaxis is 27.

There are about 6 or 7 flagellae, each of which can spin CW or CCW. I don't know if they ever exactly stop. Tumbling occurs when some are going CW and the rest are going CCW, or perhaps also during the transition. When flagellae all go CW or
all go CCW, the bacterium swims forward at a constant speed (with more efficiency with CW spinning).

For each flagellum there must be some output signal that can pass from a positive to a negative effect, causing CW or CCW spinning. So the lowest-level output signal has, basically, two states: CW and CCW. Assuming that
can equate the output signal to a two-valued signal: swimming or tumbling, with tumbling occurring when some of the flagellae but not all are

reversed. Extremely large time rates of change of concentration of the input substance can actually cause the flagellae to ALL reverse, in which case the bacterium does not tumble but simply swims in a straight line again. The flagellae conform to a single spiral of the opposite hand.

All this is in Koshland, D. E., "Bacterial chemotaxis as a model behavioral system" New York: Raven Press (1980).

The behavioral VARIABLE here, at the lowest level, is CW or CCW spinning of a flagellum. But at the level connected to the sensors in question, it is the INTERVAL THAT PASSES BETWEEN TUMBLES, where at the conclusion of each interval the outputs driving some of the flagellae are momentarily reduced through zero to the REVERSED polarity. That is what produces a tumble. The system that steers E. coli does so by making the interval between tumbles vary inversely with the error. The error is the difference between a reference signal and a signal that represents time rate of change of concentration. The reference signal is set to a positive value for an attractant, a negative value for a repellant. Positive and negative values, I assume, must be carried by different chemical messengers, as concentrations cannot become negative.

All this is by way of saying that the feedback in this system is NEVER positive. It is always negative. If the time rate of change of concentration is too low, for an attractant, the interval before the next tumble is shortened, which gives the low (or negative) rate of change only a small contribution to the average rate of change. If the rate of change is high, the next tumble is put off, giving the higher rate of change more weight in contributing to the average rate of change. So there is a monotonic relationship between sensed rate of change of concentration and interval between tumbles. This relationship never changes sign.

This leads to your next point, Wayne, which I think is correct as far as it goes: the complexity of behavior reflects the complexity of the environment. But we can carry that idea further. A very simple organism can control only a few variables. The "environment" for that organism is not the one WE see, but one made up of only those few variables. Disturbances that relate to behavior must affect those variables or they will not be reflected in behavior. In fact, the complex environment we see is made much simpler when it is seen by an organism with only a few sensory discriminations.

So, Wayne and Rick both, this may be the key to answering the question as to what complexity is. Complexity is defined not by us, the observers, but by the behaving system. Clearly, an organism that can control only a few variables can control only a few ways that the environment can affect it. It cannot discriminate between different environments that affect the sensory variables in the same way. This means that it must find a niche in which only those crude discriminations suffice to keep it alive. An organism without any exteroceptors must not live in a way that requires it to control environmental variables remote from itself. If it senses only local conditions, it must live in such a way that local conditions are all that matter to it. Of course this means it is vulnerable to many kinds of disturbances that work from afar, such as radiation from sunlight, or grasping tentacles.

The evolutionary pressures that require an increase in complexity are precisely the ones that require the organism to increase the number and

kind of variables it senses and controls, in order to survive. E. coli's ancestors probably just swam without tumbling. Their ancestors probably anchored themselves to something while currents brought them food. Their ancestors drifted with the current (when food was always locally available). With each step backward, fewer variables had to be controlled. The drifter needs to know only how to ingest. The anchorer also has to know how to attach itself. The swimmer needs to know how to move itself. And E. coli needs to know when to move and when to tumble. Each new behavioral capacity goes with new variables to be controlled. And each new way of controlling exposes the organism to a more varied environment, which eventually will require learning to control even more variables. E. coli's swimming toward food also carried it toward poisons, which it had to learn to detect and avoid.

I don't think the fossil record tells us much about controlled variables, at least as it is normally interpreted. Maybe with some re-examination we might be able to guess at the range of variables each "layer" could control, perhaps even by examining the habitat. There doesn't have to be any difference in morphology for a new level of control to arise; that all happens in parts of the organism that aren't preserved in rock. There could be enormous increases in capacity to control that would remain completely invisible to the paleontologist. Can you tell from looking at a mummy how smart or skillful that person was? For all we can know now, dinosaurs discussed reptile philosophy with each other over elevenses.

This same argument applies to the acquisition of many control systems of comparable type, and to the development of multiple levels of control. With each such addition, the environment is capable of disturbing the system in more ways and more subtle ways, thus producing ever more complex resistance to external events and ever more ability to deflect natural processes from the course they would otherwise have taken. Complexity can't be measured by counting the number of cells in an organism. It can only be measured appropriately by counting controlled variables.

Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Thu, 24 Jan 91 14:03:57 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: quick question

I just read an article by Dennet called "Where am I." It's a beautiful piece of writing which humorously illustrates some of the philosophical issues in cognitive science. I'm hoping some of you have already read this and as a reminder I will state that the story has to do with Dennet having his brain surgically removed and then having transmitting and receiving devices located at the end of all the severed nerves. The idea is that the person behaves like normal with his brain outside of his body. Anyway , at one point in the story he mentions how his transmitting mechanisms on his brain (or the receiving mechanisms on his body) stop functioning and thus he is unable to behave. This point is obvious. My question/clarification: if the receiving mechanisms on his brain (or the transmitting mechanisms on his body) stopped functioning, would he be able to behave? If behavior is the control of perceptions, then it seems the answer is "no," but somehow it seems like some form of behavior would be possible. Any thoughts?

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Date: Thu, 24 Jan 91 14:34:20 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: David Goldstein

David Goldstein is listening in on CSGnet via a dedicated FAX line. Apparently, anyone now sending a message to CSGnet will get a notice stating whether the FAX was successfully sent or not.

This needn't concern you, unless you want to make sure that David has received your message. If his FAX number is busy after four tries, the message will fail.

Because of the unreliability of FAX and the extra acknowledgement now sent individuals sending mesages to CSGnet, I see this as a temporary setup only. Hopefully, David will soon have a regular email link for receiving and sending messages.--Gary

Gary A. Cziko Telephone: (217) 333-4382
Associate Professor FAX: (217) 333-5847
of Educational Psychology Internet: g-cziko@uiuc.edu
Bureau of Educational Research Bitnet: cziko@uiucvmd
1310 S. 6th Street-Room 230
Champaign, Illinois 61820-6990
USA

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Date: Fri, 25 Jan 91 08:55:17 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: humor

A tired old joke that has a little different meaning when I heard it again the other night:

NEWS FLASH: Researchers cause cancer in laboratory rats.

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Date: Fri, 25 Jan 91 09:25:57 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: retention, evolution

Bill Powers (910123a)

Bill: I thought I had a good understanding of what you were saying last time, but I was wrong. For future reference to help you get through to others, here is the key sentence that finally made it sink in:

>Thus all lines of descent could conceivably
>find a sequence of mutations that restores the variable to its reference state,
>without any individual having been weeded out before reproducing.

As a Campbellian, use of the world "vicarious" also helped me.

It's quite amazing (and personally a bit discouraging) that it's taken me all this time for me to understand your control theory of evolution. I have put lots of effort into understanding evolution over the past several years and almost as much effort over the last year and a half trying to figure out control theory. And yet, grasping your ideas here have been very difficult for me. I can only assume that nobody else on the net has any idea about what you're talking about, or that I am a lot slower than most! (Let me know out there.) Thanks for being so patient about this.

Yes, I finally see now how this could be possible, although I am not SURE that it is possible, or that it actually happens. And I am not sure how we can find out if it does. Any suggestions? What can the fossil record tell us? Models? Simulations? Now that I understand your argument, I will try to bounce this around some biologists to see what they think.

Here are some related comments.

First, you must realize that Darwinian natural selection does not require that anybody die before reproducing. At least in principle, everybody can reproduce. But the genes of those organisms who are more SUCCESSFUL in reproducing (leave more offspring) will eventually (and quite quickly) dominate over their alleles in the population. So we don't even need Darwin's hammer for Darwinism. Just varying rates of reproductive success. However, if I understand what you are saying, it should be possible to have adaptive evolutionary change and speciation even if all organisms leave exactly the same number of offspring. Now that seems to me to be a novel idea.

Perhaps another way of getting this idea across is to see it as a mechanism whereby natural selection can eliminate certain genes without necessarily destroying the organisms carrying them (or otherwise preventing them from reproducing). If you carry "bad" genes which are responsible for intrinsic error (they give you a short coat of fur when the Ice Age is coming), you mutate and don't pass them on to your offspring. Instead you pass along genes that will give your descendants a different coat than you have, either longer or shorter. If it's getting colder, the those descendants with longer coats will have reduced error. Those with shorter coats will have more intrinsic error, but hopefully not enough to kill them off, but enough to drive some more mutations so that they will then have descendants with longer coats (or perhaps they will stick with the shorter coats and change their metabolism, or migrate further toward the tropics in winter, etc.).

Another way of seeing is to imagine an organism's evolution control system saying to the sex cells, "Hey, this organism has lots of error. Something in this genome is not good. I don't know what it is, but try something different. MUTATE NOW!" This way the bad genes are more likely to get eliminated BEFORE THEY LEAVE THE PARENT ORGANISM. So from the point of view of the genes, it IS Darwin's hammer working (though not at the level of the organism). The only support I know for this idea is the work that Cairns et al. have done on starving E. coli (our old friend again!). If you raise E. coli in a medium containing nutrients that they cannot use, they appear to mutate at a faster rate than non-starving E. coli.

But if this is possible, it seems to me that even more is possible. Through prior natural selection, why couldn't we see a host of different

evolution control systems which actually direct mutations on specific parts of the genome? This way temperature error would more likely cause mutations in those genes responsible for those systems (fur, circulation, fat retention) most likely to be able to correct that type of error. This sounds quite wild and would look like Lamarckism, but need not be (of course, couldn't be if it worked since Lamarckism can't work). However this might not be possible since the genes for any physical trait tend to scattered all over the genome and changing a gene may have have lots of different effects (e.g., being albino makes it easier for you to hide in the snow, but it also makes your eyesight worse). You see, Bill I think I really do finally understand what you're talking about!

However, there must be some environmental pressure that is not suddenly lethal for this to work. A slow-footed deer may have no trouble feeding itself. But then just it is about to mate for the first time, a swift-footed wolf comes knocking on the door carring Darwin's hammer in it's teeth. Where was the internal error here? I think lots of natural selection works like this. Either you've got what it takes or you don't and everything is just fine until some predator comes by, or you fall out of your tree, or winter arrives, or some unforeseeable catastrophe happens. In fact, Stephen Jay Gould makes a strong push for catastrophe as a force in evolutin (one reviewer called it "survival of the luckiest") in his latest book, _Wonderful Life_..

Also, my earlier comments on superfecundity need to be considered. In the wild, I think the proportion of offspring that actually reproduce is very low, especially for the simpler organisms. I also mentioned sexual selection as a potential problem.

Now that I finally recognize that CT evolution is a possibility, how about commenting on some of these points? Here's where we need some biologist types to let us know if there is any evidence for this gentler, kinder view of evolution. I'm forwarding your note and my reply to some I know. Maybe they'll join the discussion.--Gary

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Date: Mon, 28 Jan 91 12:23:34 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Fuzzy Logic

Ok -- since nobody is posting let me take this opportunity to try to stir up a new topic. The LA Times Science section had a long, fairly uninformative article about the wonders of "Fuzzy Logic". I've heard about this before -- on radio news and whatnot. The idea seems to be that this fellow invented a logic where the variables are not just true or false but have probability

distributions. Thus, I suppose, a logical statement like "It is raining" is not just true or false. It has a probablity distribution ranging over the "truth space" -- from 0 to 1.

I'm sure there are lots of fun games you can play with this logic system. I imagine that the results of logical operations (and,or,not, etc) are also probability distributions. But the articles I'm reading are talking about all the incredible things fuzzy logical can do that regular logic can't in terms of controlling things. For example, they are talking about putting fuzzy logic chips in washing machines to control the wash cycles or something like that. I've heard that there is fuzzy logic in cameras to help with the automatic focus (focus control).

I'm wondering if anyone out there knows anything about fuzzy logic and why people are all excited about its possible uses in control. From what I know about it, fuzzy logic seems like a sneaky way to bring the real world of analog variables back into the make believe world of digital electronics. Thus a switch is not just in two possible states -- on or off. Now there is a probability that it is in either on-- sort of a grungy way of recognizing that the "throw" of the switch must move, continuously from one place for "on" to another for "off". Even the current isn't just "on" or "off" but takes some time time to start or stop flowing.

Since most control deals with continuous variables (even if those variables someimes have only two states) I wonder what fuzzy logic could possibly add to our understanding of control. I have this feeling that fuzzy logic is another one of these things like expert systems -- an overly complicated way of doing something that can already been done simply another way. Like expert systems, I think people are excited by the name "fuzzy logic" and care only remotely about why such a model (because people do treat these as models of "intelligence") is needed.

Anyway, some informed thoughts about this might be interesting. Actually, from what I know about it, fuzzy logic might be a nice way to model the category level of perception. What say ye?

Best regards

Rick M.

****

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

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Date: Mon, 28 Jan 91 15:15:58 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: Fuzzy Logic

Marken (910128)

Rick, my control system engineer friend, Petar Kokotovic, showed my an

article from an EE magazine about this a couple of months ago. I found it interesting but had a hard time integrating it into what (little) I then (and still?) know about the "standard" type of Powersian analogue control systems.

I will see if I can find this for you, but my fuzzy memory of where I put this makes it recovery only probabilistic at best.--Gary

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Associate Professor FAX: (217) 333-5847
of Educational Psychology Internet: g-cziko@uiuc.edu (1st choice)
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1310 S. 6th Street-Room 230
Champaign, Illinois 61820-6990
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Date: Mon, 28 Jan 91 16:21:11 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Fuzzy logic, fuzzy theories

Rick Marken (910128) --

I don't know enough about fuzzy logic to back you up in your opinion of it, but I have the same opinion. Most computer modelers "don't do analog." Fuzzy logic looks like a way to bend digital logic to accomodate continuous variables -- as you say, the hard way.

I've been reading some stuff by W. J. Freeman on chaos and perception (The physiology of perception, Scientific American 1991, 264(2). Cliff Joslyn send me some other articles by Freeman on the same subject, that I don't have immediately at hand. I suspect that the same sort of phenomenon is going on here, compounded by the fact that Freeman uses EEG measures and a rather arbitrary model using second-order differential equations. The problem with EEG measures is that they show oscillating signals, which makes it seem that the systems in question must be oscillators -- but the same things would be seen if the signals were frequency-modulated pulse trains. Freeman doesn't do circuits, so he isn't really modeling. He's setting up equations with feedback paths, and the equations oscillate, so he can reproduce some of the oscillatory phenomena in the EEG. But there's no telling what the signals mean. EEGs make wonderful filters.

In the SA article he draws some nice phase-space diagrams and claims that the tracings represent chaotic behavior; he also shows some contour plots of activity in the olfactory bulb and shows that the patterns change when different smells are being smelled. But he doesn't say what or who recognizes these patterns, whether they be phase-space diagrams or contour plots. I have a strong suspicion that these ways of presenting data are gratuitous. The guy writes with great conviction and persuasiveness, but two hours later you want to read it again. He really doesn't say HOW he knows that any of his conjectures is true: he just says they are true. I can't help feeling that his general approach could tell us something, but I just can't believe that "chaotic attractors" are the answer. That's too superficial a level of analysis.

In this case I can't say that he's doing anything the hard way because I don't know the easy way yet. But I have a gut feeling that if we came up with perceptual functions that would identify smells, modeled them, and then modeled an EEG electrode that picks up the ionic potentials, we would find the sort of stuff that Freeman is talking about. His measurements certainly depend on what's going on, but I think that the EEG approach throws away an enormous amount of information, at the same time concealing the sort of information we would want: the way one signal depends on others.

At some time in my career, this sort of doubtful-Thomas reaction is going to degenerate into senile mumbling. I hope it hasn't happened yet.

Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Tue, 29 Jan 91 11:11:32 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mar@CS.ABER.AC.UK
Subject: Searching tracking systems

Actually I'm working on behaviour-based robotics (trying to grasp it, that is to say) and very soon I will be moving to computer simulations. What I'm proposing to do is to characterise some basic cognitive processes which make engineering sense in order to model and simulate them in a computer. The first of such processes I would like to explore is anticipation (with very useful applications in behaviour-based robotics).
I already have a clear characterisation of it. I'm now in the modelling business and the answer I found (not suprisingly of course) in control theory.

Does any one have a source code (any language) of any of the already traditional tracking systems? I would be very grateful if you could please send me a copy. I will write my own program of course because anticipation involves interaction of multiple inputs. I just want to speed up the process by learning some tricks you may have.

Thanks in advance,

Marcos Rodrigues.
Univ. College of Wales, Dept CompSci, Aberystwyth, UK. mar@uk.ac.aber.cs

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Date: Tue, 29 Jan 91 10:13:28 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Fuzzy Logic
In-Reply-To: Message from "CSG-L@VMD.CSO.UIUC.EDU" of Jan 28, 91 at 12:23 pm

Greetings. It just so happens that I work with things fuzzy.

> The idea seems to be that this fellow invented
> a logic where the variables are not just true or false but have probability
> distributions. Thus, I suppose, a logical statement like "It is raining" is

> not just true or false. It has a probablity distribution ranging over the
> "truth space" -- from 0 to 1.

Almost. Fuzzy theory tries to separate itself distinctly from probability theory. So while it is true that a fuzzy variable has a "degree" of bening "on" from between 0 and 1, this is not a probability. I.e., the sum of the "fuzzinesses" of a set of things happening does not add to one.

I should also say that Possibility Theory is a kind of information theory that is based on fuzzy sets, and that also tries to separate itself from probability. A posssibility distribution is a set of numbers between 0 and 1 such that the MAXimum of them is one, and thus their some is >= 1. In possibilities we use the "non-sepcificity" measure as an analog to stochastic entropy.

While probability measures are additive and entropy highly non-linear, possibility measures are non-additive and nonspecifities are linear. This makes possibility theory (at least) computationally more efficient for many kinds of problems that would use stochastic entropy instead.

Fuzzy sets and possibility theory operate on an algebra of the min/max operators, corresponding to the +/* operators for probability. Thus they represesent "weaker" forms of uncertainty than probability. In the most General Information Theory, probability and possibility are just two special cases.

The mathematics of fuzzy sets and possibility theory are direct generalizations of non-deterministic processes. For example, consider a system in state A which can non-deterministically transit to any of states B, C, or D. In probability theory, you would assign 1/3 pr. of each transition. In strict non-determinism you just say "all are possible" and nothing more. In fuzzy you can say "possibilities of transitting to states B, C, or D are .9, .2, and .8 respectively." Note that the sum <> 1.

I can send my dissertation prospectus and a bibliography to anyone who wants one. If there's enough demmand I can post it to CSG-L.

> I'm sure there are lots of fun games you can play with this logic system.
> I imagine that the results of logical operations (and,or,not, etc) are also
> probability distributions.

Again, fuzzy is not probability. But fuzzy, probability, and traditional logic are all based around similar algebraic structures:

Logic	Set theory	Fuzzy theory	Probability
AND	Intersection	min	*
OR	Union	max	+
NOT	Complement	1 - x	1 - x

> But the articles I'm reading are talking about
> all the incredible things fuzzy logical can do that regular logic can't in
> terms of controlling things. For example, they are talking about putting
> fuzzy logic chips in washing machines to control the wash cycles or
something

> like that. I've heard that there is fuzzy logic in cameras to help with the
> automatic focus (focus control).

Yes, that's all true. They're highly effective at traditional control tasks, e.g. there's a subway system in Japan that can stop on a dime with absolutely no "jerk". I have a demo of a fuzzy control system that I can upload. It's in binary, so if you understand uuencode and pkarc you can use it.

By the way, the Japanese are all over this stuff, much more than American or Europe. At a recent AMERICAN fuzzy conference I was at, about 1/2 of the people there were Japanese engineers.

> From what
> I know about it, fuzzy logic seems like a sneaky way to bring the real world
> of analog variables back into the make believe world of digital electronics.

Excuse me, but you seem highly suspicious and defensive. Why isn't traditional STOCHASTIC control mechanisms also a way to bring analog back into digitial control? I wish I knew more about traditional control theory, Powersian control theory, and stochastic control theory so I could argue this better. My use of fuzzy is not in control, but in modelling and automata models. Perhaps someone can educate me.

> Thus a switch is not just in two possible states -- on or off. Now there is
> a probability that it is in either on-- sort of a grungy way of recognizing
> that the "throw" of the switch must move, continuously from one place for "on"
> to another for "off". Even the current isn't just "on" or "off" but takes some
> time time to start or stop flowing.

I suspect that that's a description of stochastic control theory. It's not fuzzy control.

> I have this feeling that fuzzy logic
> is another one of these things like expert systems -- an overly complicated
> way of doing something that can already been done simply another way.

Actually, fuzzy methods are a simpler way of doing stochastic things. Yes, they are a more complicated way of doing deterministic control, but then again non-deterministic control is just plain more complicated than deterministic.

> Like
> expert systems, I think people are excited by the name "fuzzy logic" and
> care only remotely about why such a model (because people do treat these as
> models of "intelligence") is needed.

No doubt there are many people jumping on the fuzzy bandwagon who care little except for the name. But, as I'm sure you can imagine, there are also many very serious researchers, companies and institutes who validly see fuzzy methods as very important. Personally, I regard both fuzzy and possibility as critical elements of a General Information Theory which can move us beyond stochastic theory.

O-->
| Cliff Joslyn, Cybernetician at Large, cjoslyn@bingvaxu.cc.binghamton.edu
| Systems Science, SUNY Binghamton, Box 1070, Binghamton NY 13901, USA
V All the world is biscuit shaped. . .

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Date: Tue, 29 Jan 91 12:06:43 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Tracking models

Marcos Rodrigues (910129) --

I can send you some programs I have developed. You can run them if you have the following:

An AT-compatible computer with
A graphics screen (CGA, Hercules, EGA, VGA), and
A mouse or a game joystick (mouse works better),

The programs are as follows:

Demo1: Illustrates the phenomenon of control using a tracking task. Shows some of the basic properties of a human control system, as well as a number of phenomena that can't be explained by S-R models.

Demo2: Develops the theory of control for the tracking task through an interactive model. Develops the basic equations of control for a simple system. Ends with a real tracking task and allows user to adjust a model to reproduce the subject's behavior.

Arm Version 2.0. Shows a little man who can reach out and touch a target in three dimensions. The user can move the target using the keyboard or a mouse/game port/AD converter. 3-D graphics. There are 8 control systems involved: three kinesthetic and five optical. The kinesthetic systems control two angles at the shoulder and one at the elbow. They receive reference signals from three visual control systems, which compute x, y, and z location of finger relative to target. Ray-tracing is used to determine retinal position of fingertip and target images. binocular vision is used to derive depth perception. Head orientation is based on retinal images too -- the target image is kept centered in x and y.

In this system, there is no mass, so lower-order systems are assumed to be working. There is a version 3.0 under development. It adds full arm dynamics, alpha and gamma feedback loops, damping using first-derivative of stretch information, muscle nonlinearity in both force and spring constant versus length parameters. In preliminary tests it works just like version 2. Greg Williams of the CSG is working with me on this project.

These are shareware programs. If you have a budget, they add up to about $150 US. If you don't have a budget, feel free to use them. Copy them and pass them around at will. Before I ship them, please indicate whether you (a) want them, and (b) can run them. I'll include a disk with full Turbo Pascal 5.5 source code for everything, including Units and .obj files that are needed, with assembler source. The source code is sparsely commented, I'm afraid, but you'll be able to pick out the highlights by tracking down function calls. Just running the programs should be informative.

These programs don't get into anything cognitive, but the arm model has now reached the point where we hope to be able to start building upward toward more cognitive-looking control systems. The nature of what a higher system has to produce in order to carry out a cognitive task is now clearer. For example, to write the answer to the question "What is 5 minus 5?", the cognitive system would have to emit two sine waves ninety degrees out of phase, to draw an "O." These sine waves would be reference signals for the visual control systems controlling fingertip position in the x and y directions.

Welcome to the modeling world!
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Tue, 29 Jan 91 10:39:44 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Fuzzy Logic/War

Cliff J. (910129)

I am suspicious but I don't really have anything to defend. I'm really just curious about how fuzzy logic contributes to the solution of control problems. I would love to see the fuzzy logic control program you mention. Could you just post the source code (or pseudo code) or perhaps some simple equations
showing what it does. I think it should be possible to give even a verbal description of how fuzzy logic is used in control loops; a level of description that is accessible to most folks on csg-list might help us all appreciate the value of this apparently very popular new tool.

Chuck Tucker (910129)

It was a little tough reading your post -- it came to me in a most peculiar format. But I did make out some comments about the Gulf War. As a person, not a control theorist, I find myself in the peculiar position of being pro-war. Part of the reason why is that I think we should be able to win it with a minimum of casualties (if not, I'm against it). I also think it is right -- people should not be able to go in and cream weak little countries and
literally rape them. This is true even if the country is run by over-rich monarchs.

As a control theorist, of course, I like to hope that conflict can be avoided; that we can look for compromise and ways of manipulating our mutual reference signals to find states of variables that are reasonably satisfying to every-body. But the fact of the matter is, some people won't adjust all all, no how. I also think that there are system concepts that people develop that make certain kinds of adjustments impossible -- in principle! I think there are often circumstances where you try to avoid the conflict (like the jews who managed to get out of europe -- Hitler was not going to change his mind) or you fight back. I think the latter is a reasonable course of action if you either have a very good chance of winning or running away (course 1) won't work. Control theory has no magic solutions to these kinds of problems. It just helps you understand them a bit and suggests ways to avoid them. But ultimately, when it comes to 2 or more control systems interacting, if there

is going to be minimum conflict then (as Perez de Cuiller (sp?) said) it takes two to tango. No other magic formulae.

I think that in those nice periods when conflicts are at a minimum, control theorists might help by pointing out the arbitrary nature of system concepts and how certain system concepts can lead to problems (who needs a system concept that says that you can't even write certain things!!! and that if you do you die!!??!!).

Let me say as a last point that my sympathy for this war is in no way an endorsement of US middle east policy (which, I think, stinks) or of any other things the US has done in the past. All I'm saying is, that from the point of view of my own system concepts, this little effort strikes me as "right". I believe Kuwait has a right to exist and, more important, that the folks living there have a right not to be tortured, raped, and robbed. I think the entire middle east (this includes israel) has failed to take a step that I believe the West (which used to be as bad as the middle east -- remember the crusades, the inquisitiuon, etc) took around 1700. It developed the ability to look at its system concepts (at least some westerners did) and it saw the value in exploring and permitting others to explore alternative system concepts. This was the birth of the idea of freedom of thought (I think that's what the phrase means -- freedom to explore alternative
ways of perceiving at the system level). Whatever else may be wrong with Western civilization (and there is plenty) this one developmen is worth a hell of a lot (at least to me). And I hate to say it -- because its a generality that probably can't be tested or falsified -- but it sure seems hard to think of instances of war between nations which shared "freedom of thought" as one of their ideals.

Enough for now

Best Regards

Rick M

****

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

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Date: Tue, 29 Jan 91 15:39:50 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: reorganization etc.

I WAS NOT SURE THAT THIS WAS SEND TO EVERYONE SO THIS MAY BE A REPEAT
Chuck Tucker(910129):Discussions from 910116-910124 on reorganization

I have not posted anything on the NET for awhile; I have just tried to keep up with the discussion and have found all of it

interesting, informative and the most learned discussion I know of on any NET of which I am a member. I hope that all of us realize how fortunate we are to have such a discussion available to us. I also hope that we can find a way to preserve major portions of this discussion. What we have here (as my friend Clark noted the other day) is another monograph which advances and develops in rather significant ways cybernetic control theory. Keep it up folks.

Now for some specific and I hope useful comments on the topic of reorganization.

I find that the notions of structural and functional reorganization very helpful distinctions as long as we remember the both can occur at various levels at the same time. The example provided by Bill regarding the furnace was rather apt since two years ago I had my oil furnace removed to replace it with a gas furnace but if we stay with the example of this replacement being a structural change it should be noted that a person who does not know I replaced the furnace would not know this by being in my house and feeling the heating operation; my house sounds just as it did before, heat feels just as it did before only smells different since my oil furnace was working improperly. They would note a change in my cooling since I did not have any air conditioning at all except to open the windows. So perhaps the air conditioning is structural change while the replacement of the furnace is somewhere between functional and structural change; I really don't know. But these examples point out the importance of providing examples of the conceptualizations which are put forth - they are very helpful in that they may point out the application and usefulness of the conceptualizations. With this in mind, what would be an example of a structural reorganization in contrast to a functional one using the same level in a single human system? I am asking for a human system example to compare with a physical system example.

I also found the basketball example very useful. I hope when we get together next time we can work on this example in actual performance (I believe I could do better that 60% but remember the best professional performance is about 91%). The wonderful aspect of the basketball example is that it involves a distance perception which is used to regulate several bodily movements and it used several systems that have to interact and make adjustments with each other and not simply the movements of the arms and hands (Powers 911016 mentions others). Again, it could be used as a very handy example in our discussions since it involves a number of sytems interacting and operating at the same time as most movements do but we rarely think of them in that way. And the point again ----- examples are very important to improve understanding and also to "test" ideas. {I hope that all of you will see the movie "Awakenings" since it has a number of very interesting and maybe disturbing examples}

Petrie (910118) and Powers (910118) did an excellent job of summarizing the previous discussions - this is also a procedure I would encourage - pause and bring the threads together if you will allow such a metaphor. But in this regard I would like to

know how "adaptive complexity" differs from what I would call "problem solving" in my pragmatic sense of overcoming a difficulty in action - accomplishing a purpose - achieving a goal - and the like. Again an example would help - especially one which is comtemporary rather than evolutionary since there is quite a bit of speculation and religion to evolutionary ones. By the way - as someone pointed out (?) most species and most societies are not with us any more so they failed to adapt or solve the problem of survival - perhaps we will find out the difficulty of survival if the so-called "Gulf War" continues for years - but again an example would be quite helpful.

The "Gulf War" example brings up a very difficult issue for me to deal with - what error will have to occur for either party to the conflict to realize that it is best to stop the conflict? Will it require a nuclear bomb? Will it require the killing of thousands of people? Since the purposes are so unclear in this encounter, can an analysis be made that is useful? Obviously (I think) such questions involve reorganization but can we figure it out and come up with a solution to the problem?

Hope for Peace Chuck Tucker N050024 AT UNIVSCVM 910129

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Date: Tue, 29 Jan 91 20:28:52 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Dennis_Delprato@UM.CC.UMICH.EDU
Subject: Tracking Models & Update

REALLY FROM Dennis <Delprato@um.cc.umich.edu>

Bill Powers--

I am interested in Arm Version.

Bill, Tom Bourbon, Rick Marken--

Mark Warner is in Mark Rilling's lab full time. It won't be too long, we hope, until he is able to begin training a bird for the tracking task we worked out with Tom.

Secondly, Mark W. is in the final stages of software that will enable me to run two labs based on the E. coli experiment. I have been saying that we need specific lab exercises that can be used for introducing students to control system research. There needs to be a problem, easy manipulation of variables (as by menu), and data collection, storage, and retrieval for lab reports. We have all this!

Right now I'm thinking of presenting the labs as "Goal Seeking with random consequences of responding." In one, we measure time to target. In another, we make it a game by giving points every time the cursor is moved inside the target after it was outside the target. I think both will arouse the interest of participants. Of course, experimenters can serve as their own subjects--the best kind of labs in my view. One obvious variable

that can be manipulated is the presence or absence of a disturbance acting on the cursor. There are many other variables, as well.

I am looking forward to get your reaction to the preparations. My next hope is to get the software written for IBM systems--presently its on the Macs.

Dennis Delprato
Dept. of Psychology
Eastern Mich. Univ.
Ypsilanti, MI 48197

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Date: Wed, 30 Jan 91 10:41:50 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Pragmatism, Evolution, Reorganization

Tucker (910129)

Welcome back Chuck. Some reactions.

> I would like to
> know how "adaptive complexity" differs from what I would call
> "problem solving" in my pragmatic sense of overcoming a
> difficulty in action - accomplishing a purpose - achieving a
> goal - and the like.

I suppose one could say that adaptive complexity is what allows an organisms to solve new problems. I am very much influenced by Popper's evolutionary epistemology in this. He also states that there is no increase in knowledge (which I see as an examle of adaptive complexity) without problems. If we see problems as causing error, then this fits nicely into control theory as well. So in this respect I see me evolutionary and your pragmatic view quite complementary. However, when I think if pragmatism, I think of Dewey. I know quite little of his philosophy, but I have read comments that his view of education was Lamarckian in that he saw education as the transmission of acquired culture from adults to children. If so, then I would be led to reject this aspect of his pragmastism. Any reaction?

> Again an example would help - especially
> one which is comtemporary rather than evolutionary since there
> is quite a bit of speculation and religion to evolutionary ones.

Don't you see "contemporary" instances of adaptive complexity all around you? Why are your lungs the way they are, cramming an amazing amount of surface area into a relatively small volume? Look at the human eye. All of the amazing ways that animals move about. A student's ability to solve problems concerning falling bodies after studying calculus.

> By the way - as someone pointed out (?) most species and most
> societies are not with us any more so they failed to adapt or
> solve the problem of survival.

The best that evolution or reorganization can give us is TENATIVE, FALLIBLE solutions to problems. Nothing is guaranteed to work no matter how long it

has worked up to now. All solutions are solutions to PAST problems. If things change too fast, we may not be able to adapt. This is clearly the case for biological evolution, and I would bet it is the case for reorganization as well. This is why control theory suggests that in education we have to be careful in providing problems for students which do not cause too large an error. The larger the error, and the more reorganization required to find a solution (new adaptive complexity), the less likely it is that a successful reorganization will be found.

This leads me to pose a question. I have been finding that students are often quite receptive to control theory while my colleagues are quite resistant. It seems that the student reaction to CT ideas (which causes error) is to learn more about it while my colleauges' reaction to the error it introduces is simply to reject it after little real consideration.

I'd like to now what experiences people have had out there in communicating CT ideas to others. And what makes us so different? Why have we reorganized the way we have? I'd like to think that we're smarter than everybody else, but that seems hardly likely. Are we rebels? More sensitive to the error inherent in traditional behavioristic and cognitive psychology? If CT really does provide important and useful insights into human behavior and development, then why are we still a tiny minority? Is the error recognized by CT just too large so that the reorganization necessary is unlikely and we are just the lucky mutants?--Gary

--Gary

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Date: Wed, 30 Jan 91 13:17:40 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mar@CS.ABER.AC.UK
Subject: Re: Tracking models

Bill (910129)

Thanks for your mail. Yes, I'm very interested on your programs.
In our lab we have real IBM ATs, and I can get all sorts of software
with the people in the Computer Unit (case I need them).
I cannot meet the costs because I don't have a budget at the moment,
but I will have it (hopefully) in July. I'm very grateful for your offer.

Best wishes,

Marcos.
mar@uk.ac.aber.cs

Marcos Aurelio Rodrigues

The University College of Wales
Department of Computer Science
Aberystwyth, Wales SY23 3BZ
UK

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Date: Wed, 30 Jan 91 14:18:29 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>

Chuck Tucker(910130):Discussions from 910116-910124 on reorganization

Apparently my previous posting was garbled in transmission or was damaged in someway when I put it on the main frame so this posting is a repeat of that note with this paragraph added about the comment by Marken (900129) on being pro-war. First, I really doubt that anyone of us is "pro-war" in that fact that we see war as a reasonable and viable solution to problems that nations are having with us or each other. Secondly, my position is based on a different view of the events that took place prior to 2 August and a different view of the history of the events which have transpired over that last 50 years in that region. My view of these events does not justify the movement of Iraq forces into Kuwait but it sees the events from the perspective of the Iraqis (what was their leadership controlling for !) so that the way they have been characterized by the leaders in the USA does not fit with my view. Finally, I think that we will find out that the military leaders have been trying to get a good war going somewhere for a long time mainly to continue their organization and its importance in the scheme of things in the USA. I am a firm believer that the people should decide whether we go to war and the military should be responsible for doing their job but not deciding or trying to force us to go to war. I am very suspicious of the information that the military supplies to anyone, even the President, when it comes to a war; they have a vested interest in keeping the war going not because the are bad people but because they have trained themselves and continually tell themselves that war is the way to peace - they do believe that war is the best solution to a problem. It is only after they retire (watch them of the various programs) that a FEW of them change their view on war; most still retain war as THE solution to problems. By the way, I believe that England and the colonies both believed in "freedom of thought" and the North and South both believed in the "freedom of thought" yet in the first instance we had a revolution and in the second we had the worst war that USA has ever experienced to date. For other instances of our wars I refer all of you to Geoffrey Perret's <<A Country made by War>>. 1989. Random House.

I have not posted anything on the NET for awhile; I have just tried to keep up with the discussion and have found all of it interesting, informative and the most learned discussion I know of on any NET of which I am a member. I hope that all of us realize how fortunate we are to have such a discussion available to us. I also hope that we can find a way to preserve major portions of this discussion. What we have here (as my friend

Clark noted the other day) is another monograph which advances and develops in rather significant ways cybernetic control theory.Keep it up folks.

Now for some specific and I hope useful comments on the topic of reorganization.

I find that the notions of structural and functional reorganization very helpful distinctions as long as we remember the both can occur at various levels at the same time. The example provided by Bill regarding the furnace was rather apt since two years ago I had my oil furnace removed to replace it with a gas furnace but if we stay with the example of this replacement being a structural change it should be noted that a person who does not know I replaced the furnace would not know this by being in my house and feeling the heating operation; my house sounds just as it did before, heat feels just as it did before only smells different since my oil furnace was working improperly. They would note a change in my cooling since I did not have any air conditioning at all except to open the windows. So perhaps the air conditioning is structural change while the replacement of thefurnace is somewhere between functional and structural change; I really don't know. But these examples point out the importance of providing examples of the conceptualizations which are put forth - they are very helpful in that they may point out the application and usefulness of the conceptualizations. With this in mind, what would be an example of a structural reorganization in contrast to a functional one using the same level in a single human system? I am asking for a human system example to compare with a physical system example.

I also found the basketball example very useful. I hope when we get together next time we can work on this example in actual performance (I believe I could do better that 60% but remember the best professional performance is about 91%). The wonderful aspect of the basketball example is that it involves a distance perception which is used to regulate several bodily movements and it used several systems that have to interact and make adjustments with each other and not simply the movements of the arms and hands (Powers 911016 mentions others). Again, it could be used as a very handy example in our discussions since it involves a number of sytems interacting and operating at the same time as most movements do but we rarely think of them in that way. And the point again ----- examples are very important to improve understanding and also to "test" ideas. {I hope that all of you will see the movie "Awakenings" since it has a number of very interesting and maybe disturbing examples}

Petrie (910118) and Powers (910118) did an excellent job of summarizing the previous discussions - this is also a procedure I would encourage - pause and bring the threads together if you will allow such a metaphor. But in this regard I would like to know how "adaptive complexity" differs from what I would call "problem solving" in my pragmatic sense of overcoming a difficulty in action - accomplishing a purpose - achieving a goal - and the like. Again an example would help - especially one which is comtemporary rather than evolutionary since there is

quite a bit of speculation and religion to evolutionary ones. By the way - as someone pointed out (?) most species and most societies are not with us any more so they failed to adapt or solve the problem of survival - perhaps we will find out the difficulty of survival if the so-called "Gulf War" continues for years - but again an example would be quite helpful.

The "Gulf War" example brings up a very difficult issue for me to deal with - what error will have to occur for either party to the conflict to realize that it is best to stop the conflict? Will it require a nuclear bomb? Will it require the killing of thousands of people? Since the purposes are so unclear in this encounter, can an analysis be made that is useful? Obviously (I think) such questions involve reorganization but can we figure it out and come up with a solution to the problem?

Hope for Peace Chuck Tucker N050024 AT UNIVSCVM 910130

Date: Wed, 30 Jan 91 13:54:31 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: E. Coli/War-Reorganization/Dogma

Dennis (910129) -- The goal seeking programs sound wonderful. I would love to see a copy of the Mac versions. Excellent idea. You might try measuring the efficiency of the random walk strategy in terms of total distance traveled (compared to straight line distance). This measure is only better than the time measure you mention because you might want to have the cursor move at different rates (when it is "swimming".

Chuck (910130) -- You are right . I am certainly not pro-war. War is not a solution. It is the lack of one. I think it is reorganization at the national level . It is loss of whatever collective control we might have. I think war is like reorganization in functionally connected hierarchy of control systems (like those in an individual). It reflect failure of current control structure to keep error at an effective minimum.Our current ways of perceivng and acting on variables in the mideast were obviously not working and the Kuwait invasion was the disturbance that drove the system into reorganization. Like reorganization, I think the consequences of war are basically unpredictable. Things may change but not necessarily for the better.

Perhaps there is a better way for nations to reorganize than through the use of war. I don't know. Reorganization in individuals can sometimes be catastrophic (as in a "nervous bvreakdown") or more graceful (as in helpful educational environments). Maybe the latter is not real reorganization. As I undertand it, reorganization means CHANGE -- I don't know where I'm going but I'm going. The rate at which reorganization occurs (on average over time) should give an indication of any progress toward better control. Maybe there have been fewer wars over the last 10 years than there have been in previous ten year periods and the world is, on average, better organized. I don 't know.

I bet I agree with your perspective on the middle east over the last 50 years. I think the people in that region have been yutzed on by the west in spades. I think Isreal (and I speak as a person of Jewish heritage with

relatives who were killed by Hitler so hold the anti-semitic or self-hating crap) was one of the most insane ideas of all time (though I can understand why people felt like it was a good idea at the time). So there are complex problems in the mid-east and the west is responsible for many of them. So maybe this is an avoidable war that could have been avoided with a change in the west's way of doing business with the mid-east. I think it could. But I think the invasion of Kuwait made war inevitable. It is a tragedy because
all parties have done things wrong, leading up to this situation. But the "rightness" I feel about this war pertains only to the liberation of Kuwait. Whatever got us there (including creepy things Kuwait might have done to Iraq) does not (in the context of my own principles) justify this kind of crap on Iraq's part.

I guess I currently labor in the context of a system concept that does not see historical wrong as a justification for present bullshit. Germany was treated like shit after WWI; for me, that does not justify Hitler. The Palestinians were treated like shit by the British and Israelis. That doesn't justify (for me) terrorism. The American Indians were treated like shit by the settlers. That doesn't justify massacres. The jews have been treated like shit in most christian countries. That does not justify taking over prime beachfront property in the middle east. Again, remember that this is just the way I see it-- not the "right" way. But it is from this frame of reference that I back (but do not like) desert storm.

I hope that the result of this awful reorganization is a change for the better on the part of both the west and the middle east nations in terms of how they get along with each other. Such an end requires the development of new and unheard of ways of perceiveing and controlling relationships between these nations -- but then, the possibility of considering these "unheard of" new control methods is what reorganization is all about. Still, it is only a possibility and the net result may be the status quo or worse. But, again, that is the risk of reorganization. You try something new and maybe it will make things better. But there are no guarantees. The only guarantee is that if you don't reorganize things will definitly continue to be as bad as they are.

By the way, I also thought of the American revolution also as example of "freedom of thought" nations fighting right after I wrote the comment so I'm prepared to accept the idea that freedom of thought is not a solution to war. There, publicly admitting that I'm wrong. Oh jeez it hurts.

Gary (910130) -- Why, indeed, don't more people get excited about the control theory model of human nature -- especially people who are already in the business? I've been trying to understand that for 10 years. One thing I'm sure of, it's not because we control theorists are any smarter than anyone else; there are some very nimble minds out there who are not control theorists.
I think there are several reasons, all having to do with what people (smart or simple) are controlling for. One thing people control for is the model of human nature that they have been taught to control for; the one that they get promotions and prestige for articulating in a pleasing manner. There are many, many people who claim to be controlling for understanding, who are really only controlling for for a good experiment or result that is understandable and exciting in the context of the current model. This seems to be articularly true in academia -- so you find many control theory fans outside of academia. Anyway, I think those of us who are

control theory freaks are controlling for something somewhat unusual. Maybe it's understanding. Maybe its peace and quiet (because so few other people seem to be interested). For me, I think it's the fun of watching people who are much smarter than I miss the obvious.

Best Regards

Rick M.

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Date: Thu, 31 Jan 91 09:18:31 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: misc.

Various comments (910129-30),

The one thing I don't like since coming into contact with CT is that I am constantly distracted by the "bigger picture" (eg. war) as I try to work out specific applications of CT to things like language. CT principles are almost always on my mind as I overhear other people, as I watch the President on TV, the reports of the war, etc. So I have throw in some random thoughts in order to clear the system and get back to work.

Rick's and Chuck's comments reminded me that just because we might understand how and why people behave, that's no guarantee things will be nice. Setting aside all the socio-historico-cultural arguments for or against the existence of countries like Israel or Kuwait, the fact is someone decided to appease his reference levels at the expense of someone else's, and that's a no no. The basic problem is, when that happens, who is the next guy to go and correct the agressor? That's when we get into hairy concepts like justification, right and wrong, etc. And I think that's why so many are ambivalent about the U.S. involvement. Sometimes we stand up for the oppressed and sometimes we don't (in fact, we aid the oppressor) and so you hear people say things like "Well, I don't know if we should be there but now that it's happened let's take care of it." And then the situation becomes more complex because not many people at home, and probably noone in the Middle East, believe the U.S. and others are simply controlling for an error named Hussein, there's some larger perception of the world to be maintained--We Are Americans, We Stand For Freedom--as Bush put it Tuesday...

On a more personal level, I've been thinking about the intuitiveness(?) of linear models of behavior, that is why they seem to be so popular. Besides the perpetration of the thinking in academe, don't the common folk kind of think that way too? Look at the way we talk: She pissed me off; That made me so happy; Don't get me upset; etc. What is it about human nature that seems prone to attribute our behavior to outside influences? Has CT

addressed this somewhere and I've missed it?

The reason I ask is that I've been watching my six year-old lately, and she's going through a stage where ABSOLUTELY NOTHING is her fault. When she suddenly got up the other night and accidently elbowed her sister in the head, it was her sister who was at fault for having her head in the way. And whenever I ask her to move or pick up something, the reply is invariably "And what about Nicoel?" Is it safe to assume that we all fight at some time or another the urge to blame things on others or the environment? Do we really teach children to feel this way, or is there a real propensity to see things behavioristically?

Joel Judd

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Date: Thu, 31 Jan 91 09:11:34 -0800
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Reinforcement/Behavior Control

Dennis Delpratto -- I had another thought about what you might like to include in your "goal seeking using random means" lab. This is one I've always wanted to do but just havn't had the time to set- up nicely. But it would be a great lab demo and, possibly, a good basis for a paper about behaviorism (which you are eminently qualified to write). The suggestion is this:

I assume you have the program set up so that a person can change the direction of the cursor by pressing, say, the space bar on the computer keyboard. The direction of movement after each press is randomly selected. I assume that each direction has an equal chance of being selected after a press. I suggest setting up a condition where someone else, besides the person doing the "pressing", can change the distribution of probability of the different directions after a press. Call the person who does the pressing the subject (S) and the person who determines the probability of different directions the experimenter (E). E should be able to change direction probabilities by, say, moving the mouse. The E could then demonstrate his/her ability to control the "pressing" behavior of S.

Actually, what the mouse must determine is the probability that the cursor moves toward or away from the "target" or goal posiiton on the screen after a press. By varying this probability, E can control S's rate of pressing (just like in operant conditioning). For example, suppose that moving the mouse to the left makes it so that the probability of moving away from the target after the press is 1 and the probability of moving in any other direction relative to the target is 0. It is obvious what will happen -- after each press the cursor continues to move away from the target and the subject presses and presses, trying to get it to change direction. Thus, we get a very high rate of pressing (and probably a very frustrated subject after a short time). If we move the mouse so that the probability of moving toward the target is 1, the subject will not press at all.

There are probably better ways to change the probability of different directions relative to the target. For example, at one extreme you might have all directions tangential and away from the target have a probability greater than 0 and all directions toward the target have 0

probability. (Think of the possible directions of cursor movement as a pie with the cursor at the center. The direction toward the target is straight up -- a line from the enter to the top of the pie. Think of the possible directions that the cursor can move after a press as the shaded area of the pie. Then the probability distribution I just described has the lower half of the pie shaded). This distribution should produce relatively high rates of bar pressing. Any shift in the shaded area away from the lower quadrant should reduce the rate of responding.

The demo would show that you CAN control behavior by controlling its consequences. But you can show, unequivocally, that this control depends on your knowledge of the subject's purpose (you must know where the subject is trying top move the cursor; then you can influence the subject's ability to achieve that goal). You can show how this control breaks down immediately if the subject decides to move the cursor to a new target or if the rate of bar pressing creates an error for the subject.

There are probably all kinds of things you can do with this kind of demo as well -- plot out cumulative records of responses, schedule the "reinforcement" in various ways. The point would be to show that positive reinforcement is negative feedback. I think that if this is done properly it could be a real eye opener for students (I sure that it would not be an eye opener for behaviorists--just an annoying mote).

Let me know if you think its worth a try; maybe I could talk to you and your student and clarify these ideas so that they could be presented to students as simply and clearly as possible.

Take care.

Rick M.

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: War, higher levels, S-R

Joel Judd (910131) --

It's hard for me to think about the Big Picture, too. The world goes on. People keep talking and doing things, without any nice pauses between experiments in which you can think about what happened. We can't put the scene into slow motion so we have time to hear and see what's going on at a higher level, and neither can they. All we can do is try to pick out key points and try to understand what they mean -- and in the meantime the action has passed on to something else.

Another difficulty is the multiplicity of control systems that gets engaged in this sort of complex event. I just heard the Prime Minister of England saying that he was "very pleased" at the conduct of the war. In fact, if some reporter at that moment had flashed a photograph of some dead Iraqi civilians, or the huge oil spill, or the families of the dead Marines, and had asked if he was pleased about that, he would very probably have said something like "of course not." When I heard a briefing officer state that we could now put a smart bomb "through a doorway," my immediate reaction was "Wow!" That is a really admirable technical accomplishment, and a lot of my control systems, at many levels, admire technical accomplishments. This, of course, leads to instant conflict, because another set of control systems sees putting a bomb through a doorway as an anti-life action. This may be why I was not asked to command the forces in the desert.

I think that General Schwartzkopf would reward some serious study. This is a man whose systems are clearly well integrated, top to bottom, with all serious inner conflicts firmly resolved. He can detest "body counts" and be genuinely disturbed about casualties, and at the same time do what the battle plan demands, including creating body counts. He said in an interview today that he believes in loving your neighbor -- and added, with respect to Hussein, that "I can love him, but by God I'm going to fight him." Think of the structure of perceptions and goals that it would take to support such apparent contradictions within compatible system concepts!

There is work here for a control theorist. I've had a notion for a long time about "mapping" a person's control systems, simply trying to lay out the goals and the perceptions involved in a person's behavior with the aim of completeness. Instead of trying to find generalizations that apply across populations, I think that we need to study individuals in great depth, trying to understand how they are put together -- what is done for what purposes, at all the levels we can find. Such a study would probably be indistinguishable from intensive psychotherapy, although the goal would be only to understand, not to change the person. I think we would learn an immense amount about the higher levels of organization in this way. Whenever I try to think about behavior in terms of the higher levels I get a strong feeling that programs, principles, and system concepts do not do justice to the higher levels. I feel cramped within the confines of three categories. But I don't see how we can improve the picture without devoting the time it will take -- a lot -- to studying individuals in depth from the CT standpoint. If anyone is looking for a doctoral thesis ...

Last remark: I think that the SR model was intuitive long before it was formalized. Disturbances clearly elicit behaviors that cancel their effects on controlled variables -- but controlled variables are generally invisible to anyone else. It looks as if the disturbances cause the behavior. This was and is common sense. Behaviorism grew out of common sense. The CT view is subtler; it requires you to look at what DOESN'T happen. So perhaps we could say that children are natural behaviorists, and that they have to develop more understanding to become control theorists.

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From: UPPOWER@BOGECNVE.BITNET
Subject: Linear thinking

[From Mary Powers]

To Joel Judd on the popularity of lineal models -

I think your six-year-old is going through a stage that for many people lasts the rest of their lives (I don't exclude myself from this group). You'll notice that anything that might make her look bad is externally caused, while I'm sure she'll be happy to take credit for the good stuff. When things go well it's because of one's brains, talent, and charming personality, and if they go badly it's because of one's childhood in a dysfunctional family, or whatever.

An odd example of trying to give the environment all the credit for everything is B.F. Skinner's memoir of his youth. In his effort to be consistent and true to his principles he writes a book (as I remember - it's been years since I read it) that is peculiarly flattened in tone and impersonal.

Not everyone blames the environment for screwing things up. Some people are self-belittling, and can't credit themselves for anything good (oh, it was just dumb luck, etc.) while blaming themselves when things go wrong - the exact opposite of what you describe.

I do think we teach our children these things, though not necessarily deliberately or consciously or specifically. Suppose your daughter had said to you yes, she elbowed her sister on purpose because she doesn't like her very much. Or even, it was an accident but I'm glad it happened. Wouldn't that have put her at risk with you?

Learning to take responsibility for feelings and actions can be heavy stuff. It may require saints for parents! Not only to set the example by their own lives, but to create a climate in which taking responsibility for one's darker side is possible.

I wish I'd thought about some of this stuff 30 years ago.

Mary Powers