CSG_9010

==
Date: Mon, 1 Oct 90 08:55:10 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Dave Carpenter <DCARP@SBU.EDU>
Subject: Re: CONFIRMING TRANSMISSION

This is to confirm that I am receiving the CSGnet messages. I wish I had more time to respond. Maybe later. Where did BARAT College come from in my address? I am at St. Bonaventure University in New York.

Keep the messages flying. It is interesting reading even if I can't contribute right now.

Dave Carpenter - SBU

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Date: Mon, 1 Oct 90 10:26:00 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <arti6!chen@VUB.UUCP>
Subject: Re: Scheduling

1. yes.
2. any time.

Chung-Chih Chen

==
Date: Mon, 1 Oct 90 08:16:46 -0700
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: 1991 CSG Meeting

Tom
Yes, schedule for Oct (or any other month for that matter).
Any week in Oct is fine with me. It sounds like a great place.
Regards
Rick

==
Date: Mon, 1 Oct 90 18:17:00 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: David McCord/Psych <MCCORD@WCUVAX1.BITNET>
Subject: RE: Scheduling

Subj: Scheduling

1. Should we schedule for October, 1991?
2. If you say, "yes," to #1, then when? The first week (2-5 Oct), the second week-12 Oct), or later?

Tom,

Yes to #1. Either week is fine for me, but 2-5 is probably better.

David

David M. McCord, Ph.D.   (w) (704) 227-7361
Department of Psychology   (h) (704) 293-5665

Western Carolina University   mccord@wcuvax1   (Bitnet)
Cullowhee, NC   28723   mccord@wcuvax1.wcu.edu (Internet)
==

Date: Mon, 1 Oct 90 22:37:46 EDT
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: Esp. to Rick Marken on "Selection of Consequences"

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

Rick, at the Fri. meeting of our CST reading group, I made available the remarks of Midgley, you, and Bill re. your "Selection of Consequences" study. I am getting them copies of the 1985 report itself, but here are some questions that came up.

Can you give details on the models that did not work--and the one that did? The more details the better.

Can you supply specifics on how nay-sayers (operant analysts) have tried to account for the selection of consequences data and other data. Two of the people in the group are sympathetic to operant accounts and agreed that it is not a good sign if so-called authorities have offered conflicting *verbal* explanations of the same data. They reported that they suspect that the theory is too shall we say flexible, and your observations may help get them even more inclined to the control theory alternative. Obviously, they *are* critical enough to at least study control theory at this point. As I say about such cases--dangerous, for they are self-critical; hence, may contribute to the overthrow of views learned in graduate school!

Can you supply the program you used in the "Selection" study so we could do it on our Macs?

Finally, and departing from "Selection," our discussions led one of us to think of what EAB types refer to "adjusting schedules." These can be run with two manipulanda concurrently, and responding on one manipulandum can adjust both schedules in different directions, for example. This might be a convenient way to establish a reference level for relative schedule "reinforcement" rates, perhaps, and then one could introduce disturbances to observe control. I don't know--it just came up-- I have no experience with such schedules.

Dennis Delprato

==

Date: Tue, 2 Oct 90 13:54:14 -0700
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 of consequences

Dennis

In response to your questions about my "selection of consequences" paper (for those who might be interested it is in Marken, R. (1985) Selection of consequences, Psychological Reports, 56, 379 - 383)

\>   Can you give details on the models that did not work--and the one
\>that did? The more details the better.

Per your request I am sending a listing of the BASIC program (written for the Mac) that does the "random reinforcement" demonstration like that in the "Selection of consequences" paper. I am also sending a copy of a more recent paper on the same topic: Marken, R. and Powers, W. (1989) Random walk chemotaxis: Trial and error as a control process. Behavioral Neuroscience, 103, 1348-1355.

The BASIC program is not real well commented so please feel free to call or write with questions. But the modelling is really very simple. For those who are not familiar with the experiment I will give a brief description. A subject watches as a dot moves across the display screen in a straight line. The subject can change the direction of dot movement by pressing the space bar. After the press the dot moves in a new, randomly selected direction. The subject's goal is to move the dot to a specific location on the screen (the target). All subjects are able to do this. The problem is that the subjects are producing a consistent result (dot on target) although the consequences of responses (the bar presses) are random. What, then, guides the target to the goal? My conclusion was that there must be a reference, inside the subject, for that result. The subject responds whenever the dot is not approaching the condition defined by the subject's internal reference.

The sucessful model is really very simple. The input to the model is the "gradient" being experienced by the dot on the screen at each moment. The gradient is the angle of dot movement relative to the target: it is 1.0 when the dot is moving directly toward the target and -1 when it is moving away from the target. The input gradient is compared to a reference gradient (set to 1 if the goal is to reach the target). The difference is added to an accumulator. When the value of the accumulator reaches a threshold value (one of the free parameters of the model) the model "presses the bar", the accumulator is flushed and the dot takes off in a new direction.

We tried various approaches to building a model consistent with reinforcement theory. It seemed that reinforcement theory would say that a response (bar press) resulting in a "good" result would tend to recur more quickly than one that led to a "bad" result. The "bad" result in this experiment is movement away from the target. But we know that the subject is more likely to press the bar after a bar press that resulted in movement away from the target so we called this the "reinforcer". The gradient resulting from a bar press is actually the reinforcer. The next problem is deciding what response is being reinforced. If it is just the bar press then the model works like this: after each barpress an accumulator is incremented an amount proportional to the gradient experienced after that press. The proportionality constant was negative so that negative gradients (away from the target) would lead to faster accumulation and, hence, less time until the next response. The accumulator kept accumulating after the press until it reached a threshold. Then there was a bar press and the accumulator flushed. Thus, the model responds more quickly after a reinforcement (movement away from target) than after a movement toward target.

This model WORKS. It is really just the control model without the reference signal. The reference signal determines which gradients are reinforcing. The problem with this model, then, only becomes apparent when you ask the subject to move the target to a new target (of the subjects choice). Suddenly, reinforcing gradients are no longer re-inforcing.

Another version of the reinforcement model that works is one that treats gradients as "discriminative stimuli". This is implemented exacly like the previous model; you just think of the gradients as stimuli determining the occurance of the next press rather than as consequences determining the next press.

The verison of the reinforcement model that fails completely is the one which, I feel, is most in the spirit of the classical description of reinforcement. Each gradient (from -1 to 1) is a stimulus associated with some probability of response (all .5 at first). The probabilities associated with the response to each gradient are then modified depending on the consequence of that response (also a gradient). Thus, if the gradient prior to a response is .75 and the gradient after the response is -.4, then the probability of making a response to gradient .75 the next time it occurs is incremented (from .5) because negative gradients are reinforcing. This model is very easy to implement (I'll send you sample code if you like) and it produces a random walk.

> Can you supply specifics on how nay-sayers (operant analysts)
>have tried to account for the selection of consequences data and
>other data.

The nay sayers have said all kinds of things:
1) That I forgot about discriminative stimuli.
2) That the reinforcement is not really random because subjects press only when dot is moving in "bad" direction so new result is more likely to be a "good" one (this is just completely false and it came from a reviewer for the journal Science).
3) That I'm beating a dead horse - because reinforcement theory has gone way beyond Skinner (though they don't say where).
etc, etc

As you can see, there are versions of the reinforcement model that can work. It can look like random "discriminative stimuli" or "consequences" produce a consistent result. When this works, it is because stimuli or reinforcers are defined in terms of the results to be produced: the reinforcing gradient or stimulus is implicitly the one pointing toward the target (or away if it's negative reinforcement). The most important feature that is missing from the reinforcemt models is the REFERENCE SIGNAL - something that specifies that a particular aspect of the environment constitutes the goal. In actual behavior the reference signal's existence is made known most dramatically when it changes -- when the subject selects a new target. The value of the reference signal (if it is not a fixed reference) is determined by the subject. The REFERNCE SIGNAL is that aspect of the situation that cannot be controlled by anyone except the subject. It is that aspect of the situation that behaviorists find most annoying. It is that aspect of the situation that behaviorists (because they cannot control it) will go out of their way to try to ignore, deny or explain away.The REFERENCE SIGNAL is probably the only thing that really distinguishes a control model from a reinforcment model.

Ultimately, I agree that my "random reinforcement" experiment is not the coup de grace for reinforcment theory since there are versions of the theory that can be made to work. The point is that the versions of reinforcement theory that do work are just control theory without an explicit reference signal. Maybe there is something to the claim by the behaviorists that control theory and reinforcement theory can be reconciled; I guess Bill and I did reconcile them. Reinforcment theory is control theory without a reference signal. I am now working on experiments to show that a reference signal is not just a mathematical nicety; it is essential for understanding what organisms are doing.

> Finally, and departing from "Selection," our discussions led one of
>us to think of what EAB types refer to "adjusting schedules." These
>can be run with two manipulanda concurrently, and responding on one

>manipulandum can adjust both schedules in different directions, for
>example. This might be a convenient way to establish a reference level
>for relative schedule "reinforcement" rates, perhaps, and then one could
>introduce disturbances to observe control. I don't know--it just came up--
>I have no experience with such schedules.

This seems relevant to "matching law" stuff; Bill's paper on that may be worth looking at. I do think there are some great experimental possibilities with multiple "schedules" (really, feedback functions). It would be very easy to make working models (actually, Bill already has) of birds controlling food where the food comes from two different "schedules" from two output devices. Have your student write to Bill.

Richard S. Marken   USMail: 10459 Holman Ave
The Aerospace Corporation   Los Angeles, CA 90024
Internet:marken@aero.org
213 336-6214 (day)
213 474-0313 (evening)
===
Date: Tue, 2 Oct 90 22:42:50 EDT
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: selection of consequences

Rick,
Thanks much for all the information. I imagine others on the Network are interested as well.
Dennis <DELPRATO@UM.CC.UMICH.EDU>
===
Date: Tue, 2 Oct 90 09:27:24 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <arti6!chen@RELAY.EU.NET>
Subject: I am a new member

Hi, everybody.

I am a new member in this list. I am a Ph.D. student. My interests are in artificial intelligence (especially in neural networks), physics, and evolutionary theory etc.

I have read "A Manifesto for Control Theorists" by Powers (thanks to Gary Cziko, who sent me this!). It is really very interesting. I like the idea of being a revolutionary. That is always what I want to be. But it seems to me that it's very apparent that a living system can be regarded as a (feedback) control system used in engineering. So I am very surprised that the manifesto claimed that it is a new idea for life science. I wonder why life scientists didn't discover it before.

Chung-Chih Chen
Artificial Intelligence Laboratory
(Building K, 4th Floor)
Free University of Brussels
Pleinlaan 2
1050 Brussels, BELGIUM
(email: chen@arti.vub.ac.be)
===

Date: Wed, 3 Oct 90 10:31:36 EDT
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: Re: Scheduling
In-Reply-To: Message of Sun, 30 Sep 90 22:45:30 CDT from <TBOURBON@SFAUSTIN>

Anytime in October is find with me; I just would not prefer it to be toooo cold.

Chuck

Charles W. Tucker (Chuck)
Department of Sociology
University of South Carolina
Columbia SC 29208
O (803) 777-3123 or 777-6730
H (803) 254-0136 or 237-9210
BITNET: N050024 AT UNIVSCVM
==
Date: Wed, 3 Oct 90 09:01:19 -0700
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: revolution

Dear Mr. Chen:

Welcome to CSGnet.

Let me try to respond to the following point that you make:

>   I like
>the idea of being a revolutionary. That is always what I want to be.
>But it seems to me that it's very apparent that a living system can be
>regarded as a (feedback) control system used in engineering. So I am
>very surprised that the manifesto claimed that it is a new idea for life
>science. I wonder why life scientists didn't discover it before.

What is new, I think, is that the control of perception (which is what feedback control means in organisms) is the fundemental organizing principle of living systems. It is the fundemental organizing principle because what living systems do, at all levels of organization, from the cell to the organismic level, is carry out purposes -- ie: they control. It is the fact that organisms control, rather than what they control, that is of central importance to control theorists. Control theorists are more impressed by the fact THAT organisms control than by WHAT they control. It is just as amazing that a dog controls the texture of the food it eats as it is that a person controls the network of contingencies that produce checkmate in chess. It is the organizing principle that is revolutionary: behavior is the control of perception.

AI types seem to be more impressed by the kinds of complex variables that people can control than they are by the phenomenon of control itself. This is certainly understandable. I'd rather watch my kid play chess than watch my dog chew (actually, I have a cat, no dog). It is the contents of control, rather than the organizing principle, that interests AI and cognitive science types (in my opinion). But AI types certainly know about control theory and some have a pretty good feel for what it is about. I was just looking over Minsky's "Society of Mind" book this weekend. He has a couple chapters on "Difference engines" which reflect a definite understanding of the purposeful nature of their behavior (A difference engine is just a feedback control system). He definitely understands that these systems produce goal results in the face of disturbance. But he doesn't really grasp the idea that this means that they are controlling perception, not "output". So near, yet so far.

Ultimately, AI and cognitive science seem to have concluded that control theory is just a sub component of a more overwhelming model of human nature. I think if you look carefully you will find that this overwhelming model is some form of external causation -- where "external" could mean in the environment or in the brain/nervous system. Just like the behaviorists, the AI people often get very close to the underlying principle of control (purpose) and then go off and do something else instead. Still, much of the AI/cognitive work is relevant to control theory. I see it as explorations of some program level perceptions that people control and how they might control them. They also are more explicitly concerned with control of self-produced perceptions (those that don't get produced via the external loop through the environment) such as memories and imaginations.

So, finally, the control revolution is really based on taking purpose seriously and understanding that purpose must be organized around the control of perception. For research purposes, this means that a large part of understanding the human mind must involve learning the nature of the perceptual variables that it controls.

Best Regards

Rick Marken

Richard S. Marken   USMail: 10459 Holman Ave
The Aerospace Corporation   Los Angeles, CA 90024
Internet:marken@aero.org
213 336-6214 (day)
213 474-0313 (evening)
===
Date: Wed, 3 Oct 90 15:18:47 -0500
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: Branching Out

As you have seen from recent correspondence, our network now branches out onto the Old World (Belgium). This reminds me of the Plooijs.

Does anyone know if they have an email address? If not, how about a mail address and/or a phone number? I would very much like to get them on the network.--Gary

P.S. BIG SURPRISE COMING SOON. KEEP GLUED TO YOUR MONITORS!!!

Gary A. Cziko   Telephone: 217/333-4382
Associate Professor of Educational Psychology   FAX: 217/333-5847
Bureau of Educational Research   Internet: g-cziko@uiuc.edu
1310 S. 6th Street-Room 230   Bitnet: cziko@uiucvmd
Champaign, Illinois 61820-6990
USA
===
Date: Wed, 3 Oct 90 21:07:00 CDT

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: comments

TO: CSG
FROM: Wayne

Tom, regarding the annual meeting, the 2nd week (Oct.) would be better for me.

David, I appreciated the bit about conflict resolution. You seem to have a firm grip on the essential ideas for a theoretical paper with an applied slant. Why not write it?

Chung-Chih Chen, welcome! I understand your incredulity. I still do not understand how psychologists can fail to recognize the fact that animals control their environments, to the degree that they are able. Indeed, we are all puzzled; read the introduction of William T. Powers (1978) Quantitative analysis of purposive systems: Some spadework at the foundations of scientific psychology. Psychological Review, 85, 417-435.

Rick, you asked about your specially edited issue of ABS. This week I have received 4 requests for reprints of the chapter, "Control theory and learning theory." People are not only reading the issue, some (learning theorists?) are expressing an explicit interest. It seems to me that we might do well to encourage these people to consider joining CSG and the CSG E-mail Network.

Gary, regarding the previous sentence, do you want to reserve access to the csg listserver to dues-paying CSG members? Perhaps we should; people appreciate what they have to pay for. More to the point, Gary, are you incurring any personal expense that should be remitted by us users? Tom, perhaps the CSG should have a policy on this matter. Please give me some guidance fellows.

Wayne Hershberger <TJ0WAH1@NIU> signing off
==
Date: Thu, 4 Oct 90 09:46:22 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bill Powers/Greg Williams by way of Gary A. Cziko g-cziko@uiuc.edu" <FREE0536@VMD.CSO.UIUC.EDU>
Subject: motor control

Sent through Bill Powers via Gary Cziko from Greg Williams at CSG modeling conference.

NO IDEAL FEEDBACK DOESN'T NECESSARILY MEAN NO FEEDBACK

Several motor-control researchers appear to be throwing out the baby with the bathwater when they criticize closed-loop models of limb trajectory formation because such models can't achieve perfect control. Consider the following:

"Biological feedback is indeed characterized by transmission delays of the order of 30 milliseconds. A position servo relying on feedback with such large delays would be prone to instabilities... and would be ineffective in compensating for disturbances at frequencies above 2 hertz..." (E. Bizzi and F.A. Mussa-Ivaldi, in D.N. Osherson, S.M. Kosslyn, and J.M. Hollerbach, eds., VISUAL COGNITION AND ACTION, 1990, 223)

Bizzi and Mussa-Ivaldi argue instead for "local passive" feedback due to elastic muscle properties, because such feedback has no delays. But control of actual limbs certainly isn't perfect -- in fact, control starts to look quite poor at frequencies upwards of about 2 Hz!

Another example, in the same vein:

"... the use of feedback control alone has its own set of problems. Generally speaking, the larger the error in state, the larger the corrective torque generated by feedback control. Thus, feedback control is error driven, so that there must by defintion be an error for this control to be active. Moreover, there are limitations on the magnitudes of the feedback gains. Hence, a desired trajectory cannot be followed without significant error with feedback control alone." (J.M. Hollerbach, ibid., 174-175)

The claim expressed in the last sentence does not follow from the (qualitatively true) statements in the preceding statements. The problem here is that a (semi-)quantitative conclusion has been derived invalidly from purely qualitative premises. This sort of illicit deduction is common in the motor-control literature. Another example occurs in an article which is basically sympathetic to the notion of closed-loop active control in limb trajectory formation:

"We can now turn to consider why, if negative feedback appears to be so powerful in compensating error, closed-loop negative feedback systems are none the less defective as controllers in general, and models of human skill in particular. Far from always guaranteeing good performance there are certain situations in which large error and gross instability may arise. That such systems can never totally abolish error for more than a few moments at a time is obvious from the fact that error is used to correct error, and therefore some error must become apparent before control actions can be taken.... A most important factor causing error and instability is transmission delay in the feedback path.... The attenuation of high-frequency signals shown by certain transfer functions is another source of error in CNFL systems. Strictly speaking, this means that no system, whether closed loop or open loop will be able to transmit high-frequency signals faithfully if it contains an element with such a transfer function.... Thus, despite the great virtue of CNFL systems as controllers, they cannot in all circumstances guarantee the elimination of error and the achieving of the desired output (goal), irrespective of the nature of the components which combine with the human operator to form a skilled system." (N. Moray, in D.H. Holding, ed., HUMAN SKILLS, 1981, 26-27)

Welcome to the real world! Of course no CNFL system achieves perfect control -- it does not follow that there is no closed-loop control of limb movement. In particular, it doesn't follow that there is no closed-loop control of fast movements; such control might be poor (because of conduction and processing lags, as well as inertia), but poor doesn't equal nonexistent. Probably the most blatant category of fuzzy thinking along these lines involves the pseudoimplication that if response-time latencies are of the order of movement times, then trajectories "must" be ballistic. In truth, a CNFL with delays and/or low-pass transfer functions will do a poorer job as tracking frequencies increase, but that just happens to be what is seen experimentally. That is, closed-loop control LOOKS open-loop for quick movements.

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Date: Thu, 4 Oct 90 09:45:14 -0700
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: trendy seance

Thanks to Greg Williams for the post of quotes by authoritative people saying stupid things about control theory. I am trying to collect some more and will post them when I get a chance.

Right now, I want to post another little item on "Trendy Science". (Incidentally, I know I posted earlier on this topic -- maybe it was one of my first posts -- but I don't seem to have it in my archive. If anyone has a copy of my earlier post on "trendy science" could you please send it to me? Thanks).

Anyway, the latest issue of the APS Observer (the newletter of the American Psychological Society) has two articles of interest to control theorists. One is an obituary of Skinner. The other is a discussion with F. A. S. Kelso, the founder and co-director of the Center for Complex Systems at Florida Atlantic University. Kelso is one of the leaders of a trendy group of behavior theorists (the group includes M. Turvey, one of the few psychologists who tried a direct critique of Powers model in a 1978 paper with Carol Fowler) who study "how complex, biological systems containing very many components generate coordinated, spatio-temporal patterns of behavior". Obviously, the Center for Complex Systems is interested in things that a control theorist might be interested in. But this group was courted by Florida Atlantic U and given big bucks to start this Center and control theorists were not. Why? Because the "complex systems" types are considered "hot"; they are at the leading edge of science in the study of living systems. Here is a sample of the amusing quotes from the (adoring) interview with Kelso:

Kelso explains his research interests this way: "in a large number of physical and chemical systems, non-equilibrium phase transitions are at the core of pattern formation. The patterns are formed in a self-organized fashion... I was interested in whether non-equilibrium phase transitions are present in behavior". When the interview asks "Non-equilibrium phase transitions? Self-organization?" Kelso reveals what is really going on here:
"Working at Haskins labs with Turvey and Peter Kugler in the late 70s, we realized that this language might be central to understanding coordinated behavior... Haken coined the term 'synergistics' in the 70s to define an interdisciplinary field to study cooperative phenomena in nature. The task was to see if synergistic concepts were relevant to human behavior."

Clearly, the goal is to say things that sound cool about coordinated behavior. It is the descriptive language that matters; not a working model. Trendy science flourishes where the value of a theory is evaluated in terms of how it sounds rather than what it can do. Even the mathematical side of trendy science is descriptive (and therefore more like language) rather than generative.

Another example of this interest in description is found later in the interview:
"the issue is to find an adequate level of description to enable

us to abstract the essential, lawful aspects of the system under
study".

Gee, I wish I could talk like that.

One of my favorite quotes from the interview is the following. To
really appreciate it, imagine that you are in a jr. high science
class and you are studying human behavior. Kelso is the teacher:
"Most behavior involves a spatiotemporal pattern of some sort...
How do you capture the essence of these patterns [students]?
Through nonequilibrium phase transition theory."

Well thanks, teacher, that sure clears things up for me.

I think if I read enough of these papers I might be able to absorb the
language well enough so that I can be trendy too. Let me try explaining
picking up a glass of water in terms of "Complex Systems" theory as I
currently understand it:

Picking up a glass of water is a complex, spatio-temporal behavior pattern
which involves many a priori degrees of freedom. This behavior is produced
by a dynamic synergy of components which make up a complex dynamic system
with non-equilibrium phase transitions. The interation between the components
of the dynamic system results in the non-equilibrim stability. The arm is
an open system with phase transitions that lower the actual degrees of
freedom required to produce the dynamic end-point of system behavior. The
glass will be picked up (remember the glass?) because the arm is a materially
complex system which, being open and not at equilibrium, will form
a self-organizing pattern. The low dimensional movements of the arm are
entrained with those of the glass to produce the low frequency, rhythmic
movements of lifing the glass.

As an exercise, you might try applying this Unnecessarily Complex Systems
theory to another behavior, say, cancelling your subsription to the
APS Observer.

Hasta Luego
Rick
****

Richard S. Marken   USMail: 10459 Holman Ave
The Aerospace Corporation   Los Angeles, CA 90024
Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)
===
Date: Wed, 3 Oct 90 20:30:51 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: MEETING DATES

Thanks, to all of you who shared your preferences about the
dates for the meeting next year. Everyone who replied, and all
of those not on the net whom I called, agreed on early October.
Phil and Margaret Runkel will try to reserve a weekend during
that period.
Welcome, to Chung-Chih Chen! I wondered who you were, when you

replied to the inquiry. In case you don't know the Runkels, they are members in Eugene, Oregon. Phil is an emeritus professor of education who wrote an excellent book on research methods in the behavioral sciences (Casting Nets and Testing Specimens, Praeger, 1990). And I am president of CSG for this year. If you want to attend the meeting next year, see if there is any agency that might provide money for you. I will do what I can to help you get it. I am doing that for someone in England, where there are such funds, and will try to do the same thing for Frans and Hedwig Plooij, in Amsterdam, if we can identify a source.

Gary, the Plooijs are not yet on a network, but they will be before long. I told them to contact us as soon as they are on and we will add them to the net. Their addresses are:

Dr. Frans X. Plooij
Paedological Institute of the City of Amsterdam
Ijsbaanpad 9
1076 CV Amsterdam
The Netherlands;

and

Hedwig H. C. van de Rijt-Plooij
Dept. of Orthopedagogiek
University of Amsterdam
Ijsbaanpad 9
1076 CV Amsterdam
The Netherlands.

They have some recent publications on human child development that incorporate control theory in exciting ways.

Tom Bourbon <TBourbon@SFAustin.BITNet>
===
Date: Wed, 3 Oct 90 21:30:54 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: HEADS, GUTS AND CONNECTIONS

When Chung-Chih Chen expressed surprise that the life sciences don't embrace control theory, Rick replied that they are close to the model, but are not quite there. I'm not sure I agree, at least not entirely. It seems to depend on which sources you read. If you look at accounts in physiology, and in "neuroscience," of the control of movement via skeletal muscle, then there is little doubt that few life scientists appeal to control theory as an explanation, and that many of them reject the control model.

But the picture is quite different, when the discussion shifts to internal variables. There, for several years, many physiologists use a fairly good control-system model. Not the old, rather static models of "homeostasis," but models in which the "set point"(our "reference signal") is compared to a a negative feedback signal from sensors that detect the present state of a controlled variable. And the present state of the controlled variable is a function of the output of the system (they now recognize that the external variable, not the output function, is important) PLUS the effects of disturbances of all sort If you want a good, representative, text, try Human Physiology, R.F. Schmid and G. Thews (Eds), Springer Verlag (1983). There are many more. This version of a control process is so widespread that most authors do not even cite a source -- it seems to be taken for granted.

The biggest differences I see between their models and ours are these: they still refer to to comparator as a controller; the error signal is still called a command signal; and the perceptual signal is their negative feedback signal. And they do not yet realize that the perceptual signal is the variable the system really controls. Of course, we don't help the situation very much with OUR terminology -- calling the external variable the "controlled variable," then chastising people when they do not realize that the system controls its perceptual signal, is not terribly fair, on our part.

As for cognitive models ... ! If there were any remaining doubts that they reduce to S-R models in I-O model clothes, those doubts are over. Read "What connectionist models learn: Learning and representation in conncetionist networks," S.J. Hanson & D.J. Burr, Behavioral and Brain Sciences, 13, no. 3, 1990, 471-518. On page 473 is a re-creation of Egon Brunswick's old "lens model" in which many environmental "inputs" converge on, and are "focused by," a lens (now called "unit processing"), then there emerge many expanding outputs. The inputs are now called "fan in," and the outputs, "fan out." I'm not sure the model explains anything more than Brunswick's did.

More important, the authors clearly identify the goals of conncetionist modeling, as they see them: to show how the "hidden layers" in the model allow it to match outputs to inputs. There it is, clear as day, the thing we have known all along, but were criticized for saying: most "cognitive" models reduce to stimulus-response models by another name. The implications of this fact are great, given that cognitive-neuroscientific theorists declare behaviorism "dead," and their models both superior and ascendant. And a majority of them view control models as just another version of cybernetic feedback models, able to account for only a protion of "mere" sensory-motor coordination, if even that. (See, especially, their remarks on p. 472, right hand column) and p. 481, right hand column).

On the subject of "gut feelings" and behaviorism, I have only a brief remark. The problem seems to be in the head, not in the gut. At a gut level, I believe every behaviorist knows that THEY act to control things: for decades, that was the stated goal, and it was called a goal. Every text, and many articles, declared the goals of behaviorism were to observe, describe, predict and control the behavior of organisms. The problem comes with an IDEA, not a feeling, that the causes of behavior MUST be found in the environment. That notion is sometimes a working hypothesis, but often a dogmatic assertion. It was around at the time of the Russian physiologists who pre-dated Pavlov and it was eagerly embraced by American behaviorists. But the behaviorist literature is replete with detailed instructions about how to tinker around with experimental animals until you see then doing what you want to see them doing. (An important point on which they seem far from control theorists, but are actually close, is when they say that, to study and understand a phenomenon, the behavior of the scientist must "come under stimulus control by the phenomenon." I see that as our idea that, to control a variable, one must put one's behavior "under the control of" any changes in the state of the variable.)

This has gone on too long. The points I really wanted to make are those about the physiological (i.e., life science) literature and about the S-R model lurking in connectionist models.

Tom Bourbon <TBourbon@SFAustin.BITNet>
==
Date: Thu, 4 Oct 90 13:16:55 CDT

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: SEANCE?

My. what a disrespectful group this is! I often give my students the assignment of describing behavior -- simple things -- in the language of the theories we are studying. Sad to say, they are good at at it: that is precisely what they learn to do in their experimental psychology classes -- repeat what they see in the literature. When they start to do thesis writing, as graduate students, my most frequent comment to them is, "say it like a real person, not like a psychologist!" Trendy, indeed! For some eloquent (?) examples, see the B&BS article I cited in my most recent postin..

Wayne, the network should be free. When our traffic mounts to whatever is the threshold level, we will be listed as an interest group, for the whole world to see -- and to join in on. I agree that something will be lost, but there will be gains. When things get too bad, each of us can make up our own smaller list to whom to send material, if we wish.

Tom Bourbon <TBourbon@SFAustin.BITNet>
===
Date: Fri, 5 Oct 90 08:10:56 EDT
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: ON USEFULNESS AND TAPES OF BILL AND ED

Perhaps I am just late on this one but I recently purchased the tapes of Bill presenting control theory and found them not only useful for me but I think potentially useful for my classes and seminars. The tape by Ed Ford is also quite good and very helpful. I would encourage all of us to obtain these form Ed. He can be reached by 1-800-869-9623 (yes that toll-free).

I believe that the major argument for the usefulness of cybernetic control theory (or what I call Sociocybernetics) is that it is a model of how a system and process works. This is the point that we have made over and over again in our meetings - the model tells you and everyone how living systems both individually and collectively work - how they do what they do - how to fix something when it goes wrong - how to make it possible for a system to destroy itself [positive feedback] - how to suggest a system solve problems - how problems can be located - and much more. This is basically the argument for the type of model we use and it differs drastically from the types of models (theories) that are used by almost everyone in the life, social and behavioral so-called sciences. Now perhaps we need to catalog or collect illustrations, examples, stories about how the model has worked so we can have handy to present to persons with whom we interact and ourselves. I suspect that this network would be a good place to begin our list of WORKING EXAMPES OF CCT. How about it mates???

Regards, Chuck
===
Date: Fri, 5 Oct 90 08:22:22 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Greg Williams <FREE0536@UIUCVMD.BITNET>
Subject: Neuron Simulation Prog

NERVOUS SYSTEM CONSTRUCTION KIT FOR IBM COMPATIBLES

PRELIMINARY VERSION, 10-5-90

FOR A COPY OF THE PROGRAM AND TURBO-C SOURCE CODE, SEND A 5-1/4" DISK AND A SELF-ADDRESSED MAILER WITH POSTAGE; REQUIRES EGA OR VGA, MATH COPROCESSOR RECOMMENDED; COMPILED WITH TURBO C VERSION 2

By Pat and Greg Williams, Rt. 1, Box 302, Gravel Switch, KY 40328, 606-332-7606, based on the ideas of Randall Beer, Case Western Reserve University, as found in his INTELLIGENCE AS ADAPTIVE BEHAVIOR: AN EXPERIMENT IN COMPUTATIONAL ETHOLOGY, Academic Press, 1990

We came upon Dr. Beer's beautifully done book (a revision of his 1989 dissertation) recently, and immediately decided to attempt a replication of his computer simulation of the real-time behavior of a simplified cockroach. The simulation is the first we've seen which starts at the level of (reasonably realistically modeled) individual neurons and ends up with whole-organism behavior. Dr. Beer's hexapod bug walks with various gaits, wanders, follows edges, moves toward "food" when its "energy" supply is low, "eats," and manages to avoid conflicts among its various types of behavior. As programmed by Dr. Beer, the bug has about 80 neurons, with about 150 connections and about 500 user-settable parameters. The program allows arbitrary connection of neurons (and, via modifications to the source code [Turbo C compiler needed for this], arbitrary specification of organism and environment models), making it a general "construction kit" for small neural networks. Note that these networks are NOT of the generalized type beloved by "connectionism," but rather are designed to perform specific functions within the context of an organism's ecological niche. Dr. Beer's bug's nervous system isn't organized as a Powersian hierarchical control system, but networks organized in that way certainly can be constructed using the program.

With a 80286/287 machine, the program (with the complete bug) runs at approximately 1/15th real time; on an 8086/8087, it runs at about 1/30th real time; without a math coprocessor, it runs about a factor of 10 slower (painfully slow!). We hope to speed up future versions significantly, mainly by more efficient graphics routines. If there is sufficient interest shown by folks without coprocessors, we could convert to fixed point. Suggestions, questions, modifications, etc., are welcome. Let us know your thoughts about this stuff... down the road possibilities include simulations of Aplysia (modifiable synapses), spiders (prey-catching, web building?), and op-amp-circuit realization of neurons (for speedier computations in parallel).

We recommend INTELLIGENCE AS ADAPTIVE BEHAVIOR not only for its development of the bug model, but for its extended critique of traditional approaches in Artificial Intelligence. Here's the foundation for a new field, folks: non-verbal AI. Thank you, Dr. Beer!
===

Date: Fri, 5 Oct 90 09:16:56 -0700
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: HEADS,GUTS,CONNECTIONS

To continue Tom's thread...

> When Chung-Chih Chen expressed surprise that the life sciences
>don't embrace control theory, Rick replied that they are close to
>the model, but are not quite there. I'm not sure I agree, at least
>not entirely.

I think we agree more than you think. I do think that the life sciences are often close to control theory (in my perception of closeness) but, in science, a miss, even a near miss, is a mile. The reason they are close (in my perception) is because a stimulus-response model can look an awful lot like a control model. It can even behave like one! And, as you correctly point out, the model that the life sciences are ultimatly trying to defend is some version of a stimulus- response model. As you note in the context of the B&BS article on neural nets:

>There it is, clear as day, the thing we have known all along,
>but were criticized for saying: most "cognitive" models reduce
>to stimulus-response models by another name.

I pointed out the fact that stimulus response models can work like control models in my post to Dennis regarding the "reinforcement" model we used in the "selection of consequences" experiment. A stimulus-response (or response-selection) model works when you DEFINE THE STIMULUS IN A WAY THAT IMPLICITLY INCLUDES THE REFERENCE CONDITION. The stimulus-response model works because behavior is occuring in a closed loop. The behavior (delay between presses) affects the input (gradient relative to target).

So the model can be called a stimulus-response model but it is really a control model with the reference signal implicitly set to zero. An excellent example of this same thing can be found in some work on computer animation that I have stumbled across. Here are some references for those who are interested:

J. Williams and R. Skinner (1990) Motion Control: A notion for interactive behavioral animation control. IEEE Computer Graphics and Animation, May, 14 - 22

V. Braitenberg (1984) Vehicles... MIT Press

C.W. Reynolds (1987) Flocks, herds and Schools. Computer Graphics (Proceedings of SIGGRAPH), 21, 25-34

These folks have built little control systems that follow things or move to targets on the screen. But they don't think of them as control systems: they have sensors and effectors so they "must be" stimulus-response devices. The devices exhibit some pretty impressive, goal seeking activity. These researchers are sure that they are s-r devices with no inner purposes. But they are actually control systems. The sensor input does affect the effector output but the effector output also affects the sensor input; there is a closed loop. The loop is stable because there is 1) negative feedback because they have set up the s-r rule so that the output nulls the input and 2) proper dynamics; there is slowing of the output effects of the sort that we use when we write our models of control. That is, the output at time t is proportional to the integral of the stimulus over time. In difference equation form $o(t)=o(t-1)+k(s(t)-s(t-1))$ where $s(t)$ is the stimulus at time t (we would say it is the error, and it is since, in their simulations s is defined relative to the target ie $s=t-s'$; where t is the target stimulus and s' is the actual current stimulus).

These "stimulus-response" devices are really control systems. They will reach their targets even when there is disturbance. But they illustrate what I mean by "close, but no cigar". These people are building control systems and watching them behave purposefully. But the researchers don't see this because they are guided by the unseen principle that behavior must be guided by external events.

One thing that might be fun is to build some of these simple organisms but put in an explicit reference signal. This should be a variable reference signal and, for now, it could just vary slowly and randomly. Now we have an organism that is still "S-R" in the sense that these researchers imagine but one which is always changing the definition of the stimulus on its own. Maybe I'll do this this weekend. The random changes in the reference produce "spontaneous" behavior that cannot be controlled by an external observer. But it is possible to demonstrate that the behavior is still purposeful and organized (non random) by applying disturbances and seeing that they are resisted. In fact, what I think I'll do is set up a reverse version of my mindreading game where now there will be a bunch of objects moving around the screen randomly; but only one will be a control system. The observer's goal (should s/he choose to accept it) would be to discover which one is the control system. This would be done by applying a disturbance with the mouse -- the same disturbance to all objects -- and "feeling" which one resists the disturbance.

PS. Tom. I hope you didn't really think I was being too nasty in my satire on the "Complex systems" explanation of behavior. If so, I apologize (really). Also, I think think that you are doing a sterling (sp?) job as president of CSG. I wouldn't be surprised if CSG made the cover of Time by the time we get to Oregon. Happy weekend to all.

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)

===

Date: Fri, 5 Oct 90 15:48:01 -0700
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: Research

One of the problems of having a relatively small number of people who are interested in the study of living control systems is that there are few of us who do much research in the field. Not only that, but some of us are in positions where there is really no time to do the kind of research we would like to do. Although it may not seem like it, I have a job -- and that job does not allow me much time to do much research in control theory. I spent most of today, for example, building a prototype computer interface for a satellite ground control station (we talk about control here all the time but we build the tools to help people control; we are not in the business of exploring the nature of human nature). So I basically have evenings and weekends for control theory reserach and I have one or two other things to do during that time -- among them, trying to write up some of my thoughts on control theory. Actually, I am using this network as a way to do some of that writing so that I will have more time to do the research on the weekends. But it is really impossible to do all the things I would like to do, partly because it takes time to try these things out and find out why they don't work (or how they can be made to work). Even though we have a powerful and elegant theory, when you actually sit down and try experiments they aften don't work exactly as you thought. For example, I spend a great deal of time several years ago trying to develop a two level control task. It was a tracking task and it seemed to involve control of one variable in order to control another. But it turns out that the task, in almost all of its incarnations and variations, could be cone as a single level task. Bill Powers finally suggested the polarity reversal approach and that worked. But it takes time and effort to do this.

What control researchers need is time and STAFF -- people to help. Some of us are working with students and colleagues. That's great but still I'm sure you would all agree that it would be nice if we had the resources of 30 or 40 people working at an institute where everyone was dedicated to the study of living control systems. I don't see that happening in the near future. So what I propose is that, those of us who are into research, jot down some of the things you would like to see done so that others on the network (or who have access to this stuff) might run with it. Of course, you don't want to give away the Nobel Prize ideas -- you can keep those for yourself and then present the results at a CSG meeting after they are published.

To get the ball rolling (I hope) here are some of the topics that I would like to study if I had the time.

1. Using Speed of Events to Identify Levels of Perception. I started this work and just havn't had the time to continue with it but I think it looks very interesting. The idea is to use the Method of Adjustment to find the rate at which a subject can just detect a perceptual/cognitive event. For example, I had numbers (two digit) flashing on the screen one after the other. When the rat is fast enough all you see is a blur. A bit slower and you can see the individual numbers (configuration) but not their sequence. A bit slower and you see the sequence but you can't see the rule that creates the sequence (if odd then even, if even then odd). It seems like an interesting approach to testing whether there are character-istic speeds for the different levels. There is some evidence that this may be the case; for example, the speed at which one detects a visual sequence is the same as that required to detect an auditory and a tactile sequence. Most intriguing.

2. Conflict. This is one that should be done by someone. My idea was to have the computer AND the subject control the same cursor in a compensatory (or pursuit) tracking task. The idea is to change the reference level of the computer model and see at what point conflict becomes intense. This test would also have to be done with variations in the sensitivity of the computer controller. It seems that, for some setting of the computer model, you will precipitate a runaway condition where the subjects attempts to compensate for actions of the computer lead to even stronger actions by the computer which lead to even stronger actions by the subject. I'm not sure of the best way to measure the "conflict-fulness" of the situation but if you could think of a clever way, this simple experiment could be a real useful piece of basic research

on the quantitative aspects of conflict. And remember, only control systems can be in conflict -- the downside of control.

I will try to think of some more stuff over the weekend. Now it's time to go. If you have it, happy Veteran's Day.

Rick
     ****

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

Date: Fri, 5 Oct 90 20:43:46 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers/Tom Bourbon <FREE0536@UIUCVMD.BITNET>

get a10 billp

Rick,

Talk about the ease with which worda can be misunderstood! I didn't think your satire on "complex systems" jargon was too nasty. Heavens! I thought it was a little masterpiece. My reply was drafted and posted too hastily, so my meaning was not clear.

As for how close the life sciences might be to an understanding of control, look again at the reference I cited as an example. There are MANY similar examples. These people are NOT talking about motor control. Instead, they are describing the control processes for internal variables. In that field, the understanding has progressed dramatically since only a few years ago -- so much so that we risk alienating a very large community when we say, flatly, that the concept of control is not understood in the life sciences. The reference signals (aka set points) are explicit, not implicit; the OUTPUT is not the object of control, rather, there are CLEARLY IDENTIFIED controlled variables (external to the control system); disturbances affect the controlled variables; and so on. Obviously, these were not the people who reviewed our manuscripts!

Look at the reference (Schmid & Thews, Eds, Human Physiology, 1983, Springer Verlag), or at one of Mountcastle's more recent editions of Medical Physiology. What you see will in no way resemble the literature on motor control, or most of the literature on "cognitive neuroscience." I think you will be pleased: it is science, not seance.

I hope your weekend was a good one. (Get a modem, and you can read your CSG mail at home, after hours.)

Tom Bourbon <TBourbon@SFAustin.BitNet>

Date: Sat, 6 Oct 90 09:36:30 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <arti6!chen@RELAY.EU.NET>

Thanks for all comments on my surprise. I am looking for the suggested papers and studying the feedback control system from the beginning (My undergraduate was in EE). I am still not sure if the control theory is a revolution.

I will tell you when I understand better.

Chen
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Date: Sat, 6 Oct 90 15:30:08 EDT
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: Correcting an "Error" in *Volitional Action*

Correction of an "Error" in *Volitional Action*

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

The following is the main part of a letter dated Oct. 1, 1990 that I received from F. H. Kanfer of the Univ. of Ill. All I can say is that I am very happy that I am not his student. Ordinarily it takes considerably more words to generate the amount of fog hovering over these comments. Rick Marken undoubtedly will appreciate Dr. Kanfer's show of respect for Trendy Science. I have taken pains to proof the below so as to accurately transmit the letter.

"I do want to correct an misperception (or editorial error) in regard to my views. In our work we have portrayed external and intra-organismic variables as supplementary in theory, though more distinct in practice. Since our 1970 papers I have suggested that self-regulatory processes kick in when a high-strength response chain in response to cues of *whatever* origin, internal or external, is not available, disrupted or ineffective. It is *not* an external vs. self issue (as you note on p. 462) but availability of a high-stength response. This can be construed as a lack of a well-established conditioned response, or in current cognitive terminology, a shift from automatic to controlled processing. See p. 47 in Kanfer and Schefft, 1988 or in greater detail Kanfer and Gaelick, p. 287-88, *Helping People Change, 3rd ed.* (Kanfer & Goldstein, Eds.) 1986."

"In more recent work we note the "feedforward" loop more, to describe the anticipatory or selective effect of the outcome of self-regulation in entering a new situation."

"I hope this clarifies our view, should you perhaps wish to refer to it in future publications."
==
Date: Sat, 6 Oct 90 15:33:34 EDT
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: From Bill Powers

Bill Powers on CST Experimentation; "Behavior Regulation and Learned Performance: Some Misapprehensions and Disagreements" (Timberlake, J. exp. anal. Behav., 1984, v. 41, 355-375); "The Kinetics of Choice: An Operant Systems Analysis" (Myerson & Miezin, Psychol. Rev., v. 87, 160-174); and Quantitative Analyses of Behavior

CLARIFICATION FROM Dennis Delprato: The following contains most of a letter on the above topics that Bill sent to me. In the first part (prior to the "New Subject"), Bill is discussing the beginnings of what I hope will be the first of several studies. This goes back to the last CSG meeting when Tom Bourbon and I discussed the idea of collaborating on research, perhaps taking off from an innovative pigeon tracking preparation that a mutual friend (Mark Rilling), it turns out, had developed at Michigan State University. I now have a student in Rilling's lab who is learning the intricacies of the advanced technology used in collecting data there. The plan is for Tom and Bill to work with us to take Rilling's work to control systems modeling.Ù

September 30, 1990

Dear Dennis,

A Pigeon Mouse! DD: Bill previously had asked about the possibility of having the pigeons track continuously, and I mentioned that a graduate student in Rilling's lab had mentioned that he would like to build a mouse for the birds.Ù I didn't realize we would be getting into genetic engineering. Tell me more. I knew you guys were smart.

Yes, something simple that has a chance to work out. The simplest thing I can think of is to show that a variable is under control by disturbing it and seeing that the behavior changes to have an equal and opposite effect. Of course the variable then has to change a lot less than it would if the behavior were randomly related to the disturbance or absent. The test for the controlled variable is still basic to control theory, making it falsifiable.

We have to keep in mind that Skinner didn't and his cohorts don't distinguish between learning (reorganization, acquisition of a new control skill) and performance (execution of control under varying conditions that a fixed organization can handle). They treat these as a single phenomenon. To make the distinction confidently, it's necessary to have a system model that can imitate behavior with reasonable accuracy. As long as the same model continues to imitate the behavior as you introduce new conditions, you know that no learning is taking place -- only performance. When you have to change the model's parameters in order to explain behavior under new

conditions, then reorganization may be taking place (or your model may be too simple). If, every time you change back and forth between conditions, the model's parameters must be changed in the same way, by the same amount, and in a predictable time-course, then you know the model has to have a new level and that you're not seeing random reorganization or learning.

Rick Marken and I have investigated reversals, for example, by reversing the connection from joystick to cursor in the middle of tracking. A simple model can't handle this because the sign of the response to error has to reverse. But human recovery from reversals always takes about half a second to begin and involves a fairly regular pattern of recovery of control, so we clearly need a higher level in the model: this is not reorganization because it is too repeatable. We know that a higher-level system is needed and not just an adaptable one-level system because before the recovery from reversal begins, the human behavior goes into a positive-feedback runaway condition that closely matches what the model does during the entire half second. Then the real behavior suddenly begins departing from the model's behavior (the model self-destructs).

In other words, the existing control system continues to work without modification during the half-second lag before correction of the reversal starts. We haven't done enough experiments to know whether the change required is a simple sign change (with gain and lags remaining exactly the same), whether the gain drops smoothly to zero and rises again with the opposite sign, or whether the system characteristics change back and forth between two different but stable sets of parameters with each reversal (which would imply two separate subsystems being chosen in alternation). We can't really do this until we add nonlinearities and dynamics to the model that make it match behavior much more accurately -- the model predictions are off by five to ten per cent which is enough to prevent reproducing details during the changeover. If we had our Institute (with a free video game room out front where endless streams of teenaged volunteers would happily participate in our experiments?) we might get somewhere with this. But I guess we have to use our energies overcoming reviewers for a few years yet.

This experiment might be quite adaptable to the animal studies, once you find control tasks that the animals can execute skillfully.

According to a certain control theorist, reorganization is driven at a rate proportional to intrinsic error. If you want to see skillful repeatable behavior in animals, you can't use the standard laboratory conditions that behaviorists have used for 60 or 70 years -- maintaining animals at 80-85 per cent of free-feeding body weight, or chronically depriving them of water, and so on. Chronic errors of life- threatening sorts theoretically cause the parameters of control to begin varying at random (this should be measurable given a good model). From what I've seen in the literature on obesity, if you let animals maintain essentially an ad-libitum level of all their own necessities, but entirely through operant behavior, you get very stable and repeatable behavior, exactly enough to provide the animal's needs. Injecting disturbances then gives rise to immediate and reliable changes in behavior of the kind we would expect.

This means that you can't keep the animal in storage between experiments and subject it to drastically-different conditions during the experiment -- it has to be continuously in the experimental apparatus, conducting its life as normally as possible. Control has to be easy and must not conflict with other things the animal is controlling for. That in turn means that you can't plan for an experiment to take place between 8:00 and 8:15 every morning; you have to record continuously and take advantage of control activities whenever they start. The animal has to be in control at all times. The only animal behaviorist I know who is set up this way is Timberlake (and his sidekick Gary Lucas, who is on our mailing list and with whom I have corresponded a little). Maybe Mark Rilling does this too -- I don't know.

New subject.

I think Timberlake is the better choice for possible collaboration. I ran into the Myerson and Meizen "Kinetics" article some years ago, and wrote up a long critique of it that I sent to the Haskins Lab. Never got a reply other than "Thank you for your interesting comment." I was probably too furious to be taken seriously.

You have to have had a little experience with system modeling to catch all the mistakes in the M&M paper. Some of them are Herrnstein's fault but most are original with M&M.

1. The "Matching Law." M&M cite the Herrnstein formula, which reduces to (as they say) R1/R2 = B1/B2, or (the one permutation they overlooked) R1/B1 = R2/B2. The 1 and 2 refer to two different keys with two different schedules of reinforcement, R means rate of reinforcement or total reinforcements, and B means rate of behaving or total behaviors (responses), respectively. If there are multiple conditions then you can add .. = R3/B3 = R4/B4 ... and so on.

In any apparatus on any kind of ratio schedule, there is some mean ratio of rewards to behaviors set by the apparatus: that is, for each key, the average ratio R/B is fixed by the setting of the schedule for that key. No matter how many times or in what pattern a key is pressed during a session on a ratio schedule, at the end of the session the total rewards will be some constant times the total presses; divide by the duration of the session, and you have (approximately) the rate of rewarding being the same constant times the rate of pressing. You could put a machine in the apparatus and let it press the keys randomly, and this would still be the case. I'm confident that people who talk about the matching law have never tried simulating the situation with random responses, but if they did they would find the same degree of match with the so-called Matching Law. I finally realized a while ago that these people don't test their ideas. They search for a way to show that they're right. They don't seem to realize how easy that is to do even if the idea is wrong.

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What the matching law says is that all these ratios of reward to behavior on all keys are the same. Of course that's not true if the schedules are different, but when you use variable ratios and variable intervals, you introduce enough slop to conceal the fact that the matching law can't possibly work -- it's a contradiction -- except for the case of identical schedules. And in that case the matching is caused by the apparatus, not the animal.

2. On page 162, M&M create two equations, (2) and (3). Either of these equations is sufficient to describe the two-key choice relationships. They then combine these two equations, which describe the same situation, to create a "complete system," equations (4a) and (4b):

$$\text{(4a)}: dp1/dt = kR1P2 - kR2P1, \text{ and}$$
$$\text{(4b)}: dp2/dt = kR2P1 - kR1P2.$$

They then declare that at equilibrium, $dp1/dt = dp2/dt = 0$. That is already wrong: it should be $dp1/dt = -dp2/dt = 0$, because the sum of p1 and p2 is 1, a constant: if p1 increases p2 will decrease by the same amount. Therefore their "equilibrium" condition is a mathematical solecism. Both $dp1/dt$ and $dp2/dt$ will, as they say, be zero when the system stops changing. I don't doubt that 0 = 0, even if one zero is negative. But it is also true that $dp1/dt = -dp2/dt$ for any other condition: the one is always the negative of the other even if they are nonzero and changing, so their sum is always zero (if k is the same in both equations -- see later).

Now add equation (4a) to equation (4b):

$$dp1/dt + dp2/dt = k(R1P2 - R2P1) + k(R2P1 - R1P2).$$

The left-hand side is always 0, because $p1 + p2 = 1$, and thus $dp1/dt = -dp2/dt$ at all times. The right-hand side is zero because

$$R1P2 - R1P2 + R2P1 - R2P1$$

is identically zero, even if you multiply it by any number k.

Therefore the solution of the "system of equations" M&M

present is

$$0 = 0, \text{ regardless of the value of } k.$$

This is a standard way of testing a proposed system of equations for linear dependence. They obviously didn't know about this. So they don't have a "system" of equations: they have one equation written twice. They then go on to show that either (4a) or (4b) can be used to derive $R1/R2 =$

$B1/B2$. That should have told them they only had one relationship.

They also should have realized that they put that result in when they set up the original equations: $p1 = B1/(B1 + B2)$ and $p2 = B2/(B1 + B2)$. Either of those forms was already shown to be equal to $Rx/(R1+R2)$. The k they are talking about then becomes Rx/Bx. I won't go through the tiresome manipulations, but it turns out that the k in equations (2) and (3) must be the ratio of rewards to responses for either key (thus making the schedules the same). All they did was assert that relationship and then get it back through a series of irrelevant manipulations.

To repeat: You will notice that they used the same value of k in all the equations. That is the same as saying that the ratio of rewards to responses was the same for both choices. So they have actually set up an equation to represent the case of equal schedules on the two choices. That is why they come up with $R1/R2 = B1/B2$ -- which is just $R1/B1 = R2/B2$, or $k1 = k2$, where k is the mean ratio set by a schedule.

These people are just playing around with algebra, not really understanding how you analyze systems mathematically, and blithely putting verbal interpretations on everything in any way that suits their purposes. Clearly they just mess around with the equations until something shows up that they can interpret; they spend essentially no time tying the meanings of the equations to the actual physical situation. This is why you need system models: system models make you say what you mean by every constant, every variable, and every statement of relationship. If you have a system model, you can tell how many equations you need -- every loop must be traversed just once. Without a model you can't say what the constants or the mathematical forms mean -- they're arbitrary.

There's lots more wrong with the M&M article, including confusing curve-fitting with deducing behavior from a system of equations (the logistic curve is dropped into the discussion out of the blue). They go through more and more complex manipulations -- I followed them all through a few years ago, having more patience then -- and every time all they are doing is writing the same equation over and over, bringing in dummy variables, and still assuming that they have two independent equations where they have only one. The whole thing shows how you can obscure your own thinking by pushing algebraic relationships around without the discipline of a working model. When I first went through that paper I ended up angry and disgusted. Since then I've seen lots more of this sort of mathematical masturbation in

this sort of literature, but I'm not angry any more. I just stopped reading it.

There's no getting away from the need to propose a model of the behaving system. When you don't do that, you end up doing abstract manipulations, not knowing whether you're going in circles, whether "new" considerations are just disguised ways of reiterating the old ones, or whether you've introduced something that contradicts the structure of the model (as M&M ended up doing). This is especially dangerous when you have an axe to grind as M&M and their colleagues do: everything has to come out so the environment is in control. That assumption is often used as if it could substitute for a detailed analysis that comes out with that (or another) conclusion. Maybe they see a different principle coming and use their manipulations to avoid it. Principles, after all, are the control level above logic, or so sez I.

They obviously don't have any direct knowledge of control theory or its uses in engineering. The only reason they bring up my work is to show that their analysis is better: " ... we believe that Powers has forsaken perhaps the most important attribute of systems analysis, its ability to describe both transition-state and equilibrium behavior." Jeez, guys, I'm sorry.

Maybe you can understand why I prefer Timberlake et al. At least they're trying to put real models together, and recognize that there's more to be learned about control theory than what they already know. I think we can get together with them on system diagrams, after which the differences in language won't matter. They're on the right track, and perhaps can help us get somewhere (that works both ways, I hope).

I'm sending you this on a disk, so you can import it into your word processor and extract any parts you might want to put on CSGNET. The file with the .ASC extension is straight ASCII with hard returns only at the ends of paragraphs, so you can reformat it. The other one can be output with a TYPE command: the left margin is zero, the right one 73. Gary Cziko is working on getting me a logon by some sort of skullduggery. I'll have to be sparing of using it because it will be a long-distance call to Urbana, but after January 1, he says that there may be a way to do it through Circle Campus in Chicago, a non-toll call. In the meantime he's mailing me weekly printouts -- what a guy.

END

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Date: Mon, 8 Oct 90 11:18:21 -0700
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: FindMind

It is monday and I have reviewed some of my mail.

To Tom: Thanks for the nice words about my "Trendy Science" satire. Thanks also for the tip about physiological control. I will try to get a hold of the Schmid & Thews Physiology book. I am not at all familair with what physiologists are doing but I would not be surprised if they are applying control theory correctly. If they are, and control engineers certainly are too, then there might be an interesting discussion of why failure to understand control theory is so specific to those dealing with behavior at the organism level.

I think there might be some definite reasons why behavioral scientists adopt principles that make it different to get control theory right. I have no idea what they are but after a recent family discussion, where a large number of people were suggesting that the solution to our social problems rest in more severe penalties for crimes, I think I have a feeling what they might be: people want control and, in particular, they want control of other people. It is a lot easier to control an s-r device than a control system. So people are willing to accept a view of themselves are s-r machines (which couldn't possibly do what they want to do -- control) as long as other people are also s-r machines that can be controlled by force. Some s-r machines (like the one's advocating punishment for others) just happen to emit better behavior than others; it's genetic. (PS. This does not mean that I think crime isn't terrible and should not be punished. I just think that more time should be spent figuring out ways to make it so that people can be more coorperative. I hope "killing the criminals" -- the ultimate form of interpersonal control --is not our only option.)

To Dennis: Thanks for the quote by Kanfer and letter from Bill. In that letter, Bill notes at least one more research project that I could add to the list I started on Friday. It would love to be able to work with someone on learning more about what goes on when a person reverses polarity in the tracking task Bill and I did (reported in a paper in Hershberger's Volitional Action book). Bill mentions several possibilities. With a little help from an interested student we could build the model and design and run the studies to see how people switch from one mode of control to another.

I would like to report the results of my weekend work where I set up a version of my "Mindread" progam where an observer has to figure out which of the squares on the screen is a control system. The "Mindread" program is called the "Five Squares" demo in my "Behavior in the first degree" paper in Hershberger's Volitional Action. In the "Mindread" program the subject uses a mouse to make a two dimensional, random pattern with one of the five squares. The mouse affects all squares but only one is being moved intentionally. The computer can detect the intentionally moved square by comparing the observed to the expected variance of each square. If the observed variance is much less than expected than that square is probably being moved intentionally.

This weekend I set up a verison of "Mindread" that I call "Mindfind". Now the subject plays the role that the computer played in "Mindread". Again, all five squares move around in random patterns driven by random disturbances. But one of the squares is actually a control system. The random disturbances are the references for the x,y position of the square at each instant. Again, all squares are also influenced by the mouse. But the mouse is a disturbance to the position of the control system square. Thus, the observer can tell which of the squares is "alive" by moving the mouse and seeing which square opposes the disturbance. The "obviousness" of the alive square depends on how you apply the disturbance (mouse movement). If the mouse is not moved at all, the five squares just drift around in different random paths; there is no way to tell the "alive" square from the others (which are just being pushed around by the disturbance). If you move the mouse relatively slowly and smoothly you still can't see which square is alive. The "alive" square is resisting these disturbances to its changing intended position but the opposition to the disturbance blends in with the other movements of the squares -- since all the movements are unsystematic.

The alive square really "pops out" when you apply an abrupt disturbance in some direction -- a sudden movement of the mouse to the left,say. All the squares then move abruptly to the left (though to slightly different extents because the mouse disturbance adds to the other disturbances) but the "alive" sqaure clearly "bounces" back from the push to whatever path it is trying to follow. The alive square "reacts" to the stimulus -- just like they say in the biology books. It reacts in that kind of lively way that we expect from living things. The other squares react as well. But it doesn't seem like a "reaction" because they just respond "as expected" to the sudden disturbance. The reaction of the "alive" square is much more noticeable because it is not what is expected; it resists the effect of the mouse push.

I'm going to keep developing this "FindMind" demo. It is really fun to play and it seems to be a good way to illustrate the difference between the "reactivity" of living and non-living systems. It also might help illustrate some of the considerations involved in studying living control systems. For example, using disturbances to detect controlled variables must take into account the dynamic characteristics of teh variable being controlled. Disturbances to a rapidly changing variable, for example, must be faster than disturbances to a more slowly varying one in order to see that the variable is actually under control. Also, the speed of the system itself must be considered when applying disturbances.

Bill P. talked about this stuff (about considering the dynamics of disturbances) in his 1973 book. Maybe this little demo can help those of us (like me) who are not up on our diff eq understand a little more about the interaction between the dynamics of system and environment.

Again, I want to thank Gary for setting up this terriffic forum. This little "Findmind" demo is (I think) one nice fruit of being able to think through my finger tips. I hope to have a nice version of FindMind ready for the meeting next year so you can see what I'm talking about (FindMind is more fun to experience then to talk about, but, then, that is true of so many nice things).

Regards to all

Rick M.

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

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Date: Tue, 9 Oct 90 07:51:58 -0500
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 as Control Theorist

The recent death of Skinner together with my new interest in control theory has motivated me to read and reread some of Skinner's writings. It sure looks different now after having some gained some understanding of control theory.

I read _Walden Two_ this past weekend and found an interesting passage. In chapter 12 (p. 97) the visitors to Walden Two are visiting the nursery:

"There is a much more efficient way of keeping a baby warm than the usual practice of wrapping it in several layers of cloth," said Mrs. Nash, opening a safety-glass window to permit Barbara and Mary to look inside. "The newborn baby needs moist air at about 88 or 90 degrees. At six months, 80 is about right."

"How do you know that?" said Castle, rather belligerently.

"The baby tells us," said Mrs. Nash pleasantly, as if the question were also familiar.

"You know the story about the bath water, don't you, Mr. Castle?" Frazier interrupted. "The temperature's all right if the baby doesn't turn red or blue."

"But I hope--" Castle began.

"It's only a matter of a degree or two," said Mrs. Nash quickly. "If the baby's too warm, it does turn rather pinkish and it ususally cries. It always stops crying when we lower the temperature." She twisted the dial of a thermostat on the front of a cubicle.

"And I suppose if frost forms around the nose it's too cold" said Castle, getting himself under control.
"The baby turns rather pale," said Mrs. Nash lauging, "and takes curious posture with its arms along its sides or slightly curled up. With a little practice we can tell at a glance whether the termperature is right or not."

Not only do we have here a clear reference level that the baby is maintaining (with Mrs. Nash controlling for the color, noise, and posture of the baby), but also a change in reference level over age.

Despite the assumption in _Walden Two_ that human behavior can be engineered and controlled, I discovered almost in spite of myself that I found the community quite an attractive place. Could control theory be used as Skinner used operant conditioning to create such a place? Or does control theory instead show us that such an utterly conflict-free community is an impossibility? (It's too bad we don't have more counsellors and clinicians on the network for this type of discussion.)

Gary A. Cziko
Associate Professor of Educational Psychology
Bureau of Educational Research
1310 S. 6th Street-Room 230
Champaign, Illinois 61820-6990
USA
Telephone: 217/333-4382
FAX: 217/333-5847
Internet: g-cziko@uiuc.edu
Bitnet: cziko@uiucvmd
===
Date: Tue, 9 Oct 90 10:51:05 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Skinner as theorist

Re: Delprato, Skinner as Control Theorist

Most people who watch behavior closely notice that control is going on. Skinner noticed it too. But he would have said that a baby turning red and crying when the temperature goes too high is under the stimulus control of the temperature. Control theory says *almost* the same thing: the baby's behavior is driven by the difference between the actual temperature and the temperature the baby wants to experience. But Skinner wouldn't have liked that proposition, because it invokes a causal factor inside the baby: the definition of the right temperature, which is determined by the baby and not by the environment. Control theory says that the baby's internal specification for the right temperature determines the stimulus value of any given temperature. If the specification changes (the baby develops a fever), the same external temperature that was satisfactory before is now "too cold." The baby acts as if the temperature has dropped, and won't be satisfied until somebody lets it get warmer. That's why we shiver and burrow into the blankets when we develop a fever: the reference temperature has increased. This makes it look as though we (sas babies or adults) sense our error signals, doesn't it? Hmmm.

Skinner described control behavior. He explained it as environmental control. If you just ignore all of SKinner's explanations of behavior, I suppose you could say he wasn't a bad observer.

PS: I seem to be fully on the net now so I can download and upload the CSG mailbox.

Bill Powers 1138 Whitfield Rd. Northbrook IL 60062 708-272-2731
Bitnet FREE0536@vmd.cso.uiuc.edu
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Date: Tue, 9 Oct 90 09:12:55 -0700
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: Skinner as Control Theorist

Gary -- the quote from "Walden II" is a gem. It sure looks like an example of Skinner understanding control theory. The quote clearly does reflect an understanding that the child has a reference level for "warmth". When the child is not experiencing the reference level of warmth it will do things to try to get warmth to the reference level. The problem is that kids don't have much of a repetoire for controlling variables like warmth so they are probably always reorganizing when they get this kind of intrinsic error. Thus, I doubt that you can reliably tell, by looking at the child's behavior, whether it needs to be heated or cooled.

Any parent knows how difficult it is to "debug" a child; about all you can tell from the child's behavior is that something is "wrong". Then you try to figure out which variable(s) should be returned to their reference levels. This is by no means an easy process and, when the child continues to reorganize (cry, squirm,etc) parents are likely to become frustrated. Skinner makes it sound a lot easier than it is to "control a variable" for the child. But he is right about one thing -- when you do get all variables to their reference state the baby becomes quiescent. Thus, Skinner does understand the idea that behavior is error actuated and that you can determine the reference level of a controlled variable by looking for the level of that variable that produces no efforts to change it.

I have found a couple of Skinner quotes that suggest that he understood something about control. For example, in "About behaviorism" he has a little section on control where he actually says something like "to behave is to control". After all, behavior produces consequences (reinforcements) and these often look like the ends towards which behavior is done (they are--but not according to Skinner). Skinner does seem to recognize controlling as a kind of behavior. It is what behaviorists do, for example. In "Beyond freedom and dignity" he talks about the behaviorist who trains a pigeon by doing a behavior called "controlling". "Controlling" is controlled by the behavior of the pigeon (who, I suppose, is doing a behavior called "being trained"). So there is reciprocal control. Clearly, Skinner's idea of what it means to control is pretty wimpy. When I control something I know how I want it to be and, if I can, I get it to be that way. The thing I am controlling has no say in the matter.If it does, then I am in a conflict with it. I lift my glass to precisely the level I want it to be. If the glass is also controlling me then it is possible that the glass wants me to put it somewhere other than where I want to put it. So far, I have been very successful at placing glasses where I want them and somewhat less successful at putting control systems (like my cat) where I want them.

Reciprocal control is a crazy notion. Control theory shows that there can be no such thing except in special cases where the two systems are either actively trying to cooperate or where they are controlling variables that are not in conflict -- as when an experimenter controls the pecking rate of a pigeon while the pigeon controls the amount of food it gets. Either of these special cases could end up in conflict anyway; one member of the cooperating pair might feel that their is an unfairness and the pigeon might not be able to generate the rates demanded by the trainer and just stop, leading to error and ugly corrective action by the trainer.

The attractiveness of Walden II comes from the appearance of lack of coersion; everyone gets rewarded for "good behavior". There is no punishment for bad. Bill Powers wrote a lovely letter to Skinner that was published in Science (~1976). In that letter Bill explains better than I ever could the problem with this "non-coercive" approach; it works as long as the behaviors that the community rewards are the behaviors you want to produce (assuming that you also want the rewards -- for simplicity we'll assume you do but that is another problem). But what a person wants to do is determined by his or her internal structure of intrinsic needs and purposes that have been learned to keep those needs satisfied. The problem with Walden II is that nobody can determine what someone else "should" do, even when the "should dos" are for the person and the community's own good (perhaps the downfall of every well-meaning attempt to create a perfect society).

There is no doubt that Walden II might work for those who want it to work and who are willing to live in the context of the community's rules. Skinner himself didn't choose to live in that society (a community built on Skinner's principles still exists somewhere on the east coast but Skiner himself didn't join when asked -- he was controlling for other variables). What Walden II shows is that coersion can be masked quite well by good intentions. I find Walden II a hell of a lot scarier then some repressive dictatorships where the coersion is at least up front and the hypocracy ("this is for your own good") transparent.

Regards
Rick M.

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

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Date: Tue, 9 Oct 90 21:06:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: Controlling temperature

Gary:
Skinner recently (1988) addressed the question of temperature control in a rejoinder to a comment of mine about his having overlooked controlled input as an important type of behavior. See: Hershberger, W. A. (1988). Some overt behavior is neither elicited nor emitted. In A. C. Catania and S. Harnad (Eds.), The selection of behavior (pp. 107-109). New York: Cambridge University Press. This volume, originally published as a special issue of Behavioral

& Brain Science, is a collection of Skinner's cannonical polemics, together with critical comments from Skinner's "peers." Also included with each comment is Skinner's response to that comment. I believe this is the only place Skinner ever systematically answered his critics in print. I'll send you a copy of my comment and Skinner's reply, Gary.

Bill P.:
Your fever example is illuminating; "Hmmm," indeed! There is surely more to perception than input just as there is surely more to behavior than output. Scott Jordan's dissertion research described at the CSG conference in Indiana Pa. is demonstrating that the perceived visual direction of a point of light in the dark changes when one INTENDS to alter one's gaze, and is illusory until the eye actually realizes the intended eye position: the spatial coordinates of the retina (known as retinal local signs) reflect the oculomotor reference signal (intended eye orientation) not the controlled input (actual eye orientation).

Warmest regards, Wayne Hershberger <tj0wah1@niu>
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Date: Wed, 10 Oct 90 08:12:41 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Misc replies

David M: Thanks, and yes. See you next week in Cullowhee.

Gary Cz: Most of the time I can tell you and Dennis apart. Sorry.

Rick M: Roger on letter. The "Mindfind" idea looks like a beautiful way to teach the test for the controlled variable. It also shows why we need a statistical method to help discover active systems in a natural setting where they aren't performing under our instructions. Looks publishable to me (but you've heard that before). [Aside to others: if you haven't talked in the phone to Rick right after yet another ridiculous rejection, you've missed a truly worthwhile display of artistry in despair].
Illegitimati non carborundum.

Gary Cz. Second thought on Skinner As. Marken said it right: Walden II works because everybody wants the rewards that are used to keep the society in line, and everyone works (funny thing) exactly as Skinner thinks they will. The real attempt to form a community of this sort didn't run so smoothly: lots of coercion. The problem is that you can't reward somebody who knows how to get the reinforcer without anyone's permission. So you have to make sure you're the only or at least the easiest source, and to maintain the behavior you have to be willing to leap out of bed with a tray-full of reinforcers whenever the person you are controlling this way does something right. I'll bet that isn't what Skinner had in mind.

I'm not enthusiastic about demonstration communities. They will work as long as everyone consciously tries to work the way the theory says things should work. Sooner or later human nature breaks up the act. This would be true even of control theorists (especially?). I think the community we need to form is already around us. If we can't help that community to shape up, we wouldn't do much better in an ashram (sp?). On the other hand, an Institute would be nice, as long as it wasn't formed to exclude Wrong Thinkers.

I'll be checking my mail most mornings and trying to reply at the same time, when the long-distance rates are low. Hard to keep it short, isn't it? Tom Bourbon (with Greg Williams) wants to collect some of the network stuff for people who aren't on it but are interested in control theory. Maybe he (one of them) could practice by working up a weekly summary or index to put on the net. Just as long as it isn't me.

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Date: Wed, 10 Oct 90 10:20:12 -0700
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: Replies 10/10 and MindFind

Replies to E-Mail 10/10/90

Wayne: Would it be possible to post your comments on control and Skinner's reply to them. I'm sure everyone on the network would be interested. If they are short enough, it might be worth the effort. I remember reading your "Some behaviors are neither elicited or emitted" paper -- a classic, in my opinion. It is a bit long to post unless you have it in a file that could be uploaded. I bet Skinner's comments on it are not too long (given all the other comments he had to reply to) so maybe you could just type those in. Anyway, if you can't post it, let me know and I'll go take a look at the B&BS issue this weekend.

Bill: Thanks for posting the letter to Estes (and thanks for writing it). I can hardly wait to see what he does. Of course, I will let you (and the net) know as soon as I find out. I already have the next journal picked out, however. For those who are interested, the "Degrees of freedom" paper, which is about coordination of two dimensional movements, has now been rejected (after considerable dialog each time) by:

JEP:Human Perception and Performance
Journal of Motor Behavior
IEEE Transactions on Systems, Man and Cybernetics
Psychological Science (Pending)

This paper has definitely been around the block. All of the reviews can be summed up as follows: "Nice experiments, clear results, obsolete theory".

If I were a normal person, I would have seen the light by now. But, being a megalomaniac, my next target is

Acta Psychologica

Because it is there. Any other suggestions for appropriate journals would be greatly appreciated. This could go on for years.

Also, thanks Bill for the comments on the FindMind (or MindFind) program. The more I play with it the more I like it. I'm not sure where to go with it next. Right now it is set up so that there are five numbers (1..5) roaming around the screen. The two-dimensional position p(i) of each number is just

p(i) = m(i)+d(i)+h

where m(i) is the output of the control model controlling the ith number with respect to a randomly varying reference, r(i); d(i) is a random disturbance (same statistical characteristics as r(i)); and h is the mouse. The model for only one number is acting at any time. Thus, for all but one p(i) the value of m(i) is a constant. I switch the model out for a particular number by setting that number's model gain to zero. I switch the model in by setting the gain to some non-zero value. The effect is a rather smooth transition from control of one number to another. Thus, for some period of time one number, say "3", is alive and the others are not. After some randomly determined period of time a different number "comes alive" and the "3" dies. The switch from "alive" to "dead" (better, from animate to inanimate) is rather smooth when done with the gain change. That is, when watching the numbers float around (the mouse is stationary) you can't tell which one is "animate" and you can't tell when a different number comes to life (not too well, but I can work on this, the gain change should be more gradual) when a different number comes to life.

I have set up a target box in the center of the screen. It is easy to keep any number in that box AS LONG AS THE NUMBER IS INANIMATE. If you try to control the animate number (keep it in the box) then you have problems. The animate number acts like its actively trying to avoid the target. Of course, its not; its just tryiny to follow its random path.

As it sits now, the MindFind demo can be used to say something about the problems of dealing with animate objects as if they were in-animate. When you look at the five numbers moving around they all look equally "animate". Even when you slowly push them around with the mouse the "animate" number cannot be easily detected. But when you start trying to control a number, the difference between animate and inanimate objects (numbers) becomes obvious. If you are controlling an inanimate number that suddenly becomes animate your ability to keep the number in the square instantly deteriorates. If you are trying to control an animate number that suddenly becomes inanimate, your control instantly and markedly improves.

MindFind seems like a nice way to illustrate the difference between animate (purposeful) and inanimate systems. It also shows the problems involved in trying to control animate objects by pushing them around (there will be conflict). I think I will also try to show the problem of trying to control by consequences. The animate object (not inanimate) can be controlled because you know its goal -- maintaining some two-dimen sional position. You can control the actions used to achieve this goal-- the m(i)--by pushing with the mouse. A representation of m(i) can be placed on the screen for each number. By appropriately pushing with the mouse you can control the representation of m(i). This mode of control should produce no conflict -- it is just like operant conditioning where access to food is limited in order to get a certain rate of responding. Pushing on the number is like access control to the food.

Conflict arises in operant conditioning when you want a reponse that causes other problems for the animal being trained (like exhaustion or starvation). This could be built into the number control experiment by having a limit to the value of m(i) that the model is willing to generate. Still, the demonstartion would show that operant type control only works with an animate system -- ie. one with purposes.

Comments and suggestions would be most welcome.

Hasta Luego

Rick

****

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

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Date: Wed, 10 Oct 90 15:06:00 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: David McCord/Psych <MCCORD@WCUVAX1.BITNET>
Subject: Causality

I recently submitted a control theory article to _The Psychological Record_. No word yet on acceptance/rejection. Dennis Delprato is on the editorial board of that journal, and he was selected as one of the reviewers. While I have not yet heard from the editor, Dennis did send me a copy of his review. He recommended publication, which I certainly appreciate, and many of his remarks were quite thought-provoking. He and I both thought that they may be of interest to this forum.

My experiment was rather simple. What follows is the abstract.

> _Summary_. According to Control Theory, the purpose of behavior is to minimize discrepancies between perceptual inputs and internally-generated reference signals which represent the desired or ideal state of the perceptual input signals. The present experiment was designed to test the hypothesis, derived from Control Theory, that input-oriented instructions, as opposed to output-oriented instructions, should facilitate performance on a compensatory tracking task. Volunteer freshman attempted to keep stationary a cursor on a computer screen, using a joystick controller to oppose a sinusoidal disturbance pattern. Subjects receiving input-oriented instructions did significantly better on early trials and, as a group, reached the criterion level of performance in significantly fewer trials than did subjects receiving output-oriented instructions.

Briefly, input-oriented instructions consisted of a graphic plot of what the cursor would do if unresisted, and output-oriented instructions consisted of the "ideal handle movement," a mirror-image of the plot shown to the output-oriented instructions group. Following is an excerpt from the introductory section.

> [Page 4] . . . Few modern psychologists adhere to the extreme position described by Watson. Yet the cause-effect model underlying most psychological theories is the same open-loop model espoused by Watson. And it is this basic element that makes the concept of purpose so difficult to manage. The idea that something internal to the organism is capable of causal rather than merely mediational influence renders open-loop models useless, as they can no longer predict outcome. Rather than question the underlying model,

psychologists, with a few exceptions (e.g., Tolman, 1932; Miller, Galanter, & Pribram, 1960; Sperry, 1988), have chosen to ignore the concept of purpose, or to treat it as an illusion.

Dennis took exception to the idea that I was referring to internal causation. His comments were:

> Page 4, Paragraph 1: To speak of "something internal to the organism" as causal is to undo the revolutionary idea of cybernetic ("circular") causality that underlies control systems theory and the integrated-field perspective generally. I know that we not infrequently find this type of statement made by behavioral control theorists, but basically it seems to be a careless statement rather than an ontological commitment. Here is where control system theorists (a la Powers) could profit from their brethren feedback control theorists and researchers of the behavioral cybernetic (K. U. Smith) branch. We would never find the thoroughgoing naturalistic and cybernetic thinker, Smith, speaking of "internal causes." This is a very sensitive and complex topic. Note, for example, that internalistic approaches that remain in front of us today are all open-loop (classic cause-effect) in configuration, contrary to the statement I am presently reacting to, e.g., mental structure ----> motoric activity, knowledge ----> performance, and so on.
>
> In a cybernetic control system, causes are everywhere and nowhere. This may be why it is preferable to refer to "control" without a "controller" as Powers puts it in "An Outline of Control Theory." Or as modern field theory suggests--cause refers to an entire set of conditions or event-field.

I have thought a lot about Dennis's remarks and am unsure of my conclusions. Certainly in a closed-loop system all elements within the loop may be seen as "equally causal." My point was that the reference signal comes from outside of the loop and may be seen as carrying more "causal weight" than elements within a particular loop. I noted that in Bill P.'s recent posting on Skinner as theorist he also made reference to internal causes, or at least that's how I read it. Following is a passage from Bill's posting:

> Most people who watch behavior closely notice that control is going on. Skinner noticed it too. But he would have said that a baby turning red and crying when the temperature goes too high is under the stimulus control of the temperature. Control theory says *almost* the same thing: the baby's behavior is driven by the difference between the actual temperature and the temperature the baby wants to experience. But Skinner wouldn't have liked that proposition, because it invokes a causal factor inside the baby: the definition of the right temperature, which is determined by the baby and not by the environment. Control theory says that the baby's internal specification for the right temperature determines the stimulus value of any given temperature. If the specification changes (the baby develops a fever), the same external

temperature that was satisfactory before is now "too cold." The baby acts as if the temperature has dropped, and won't be satisfied until somebody lets it get warmer. That's why we shiver and burrow into the blankets when we develop a fever: the reference temperature has increased. [Bill Powers]

What about it, y'all?

David M. McCord, Ph.D. (w) (704) 227-7361
Department of Psychology (h) (704) 293-5665
Western Carolina University mccord@wcuvax1 (Bitnet)
Cullowhee, NC 28723 mccord@wcuvax1.wcu.edu (Internet)

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Date: Wed, 10 Oct 90 14:15:13 -0700
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: Cause of behavior

David
Thanks for the great posting. Could you send me a copy of your "Psych Record" paper; it sounds great. Very interesting for those of us who might be in a position where we are asked to advise people on training strategies. Please send it to my home address which is in my signature below.

David and Dennis:

I think it is interesting to think about the appropriateness of calling an internal reference a "cause". In a hierarchy of control systems all but the highest order reference signals are, indeed, part of closed loop control systems and and, thus, effects of higher order causes are also causes of those causes. But I tend to think that it is legitimate to call a reference signal a cause inasmuch as it is possible to call a disturbance a cause; because it can be isolated from the behavioral loop of interest. Hershberger makes this point better than I can in his article on emitted and elicited behavior.

Consider a simple control system controlling the position of a line on a screen. The position of the line (p) causes mouse movements (m) and mouse movements cause changes in the position of the line. So m = f(p) and p=g(m) simultaneously. So, within this loop neither m nor p just a cause or effect; they are always both. Control theory shows that it is a mistake to treat p as a stimulus (which it is considered to be in a tracking task). But if p =g(m)+d, where d is a disturbance unaffected by m (so it is not in the loop), then, as Wayne has pointed out, it is perfectly reasonable to see d as one independent cause of variations in m (which it will be). So m = f(g(m)+d). Response m is a function of itself (via the closed loop) and of the independent effect of the disturbance.

The independent causal influence of a reference signal can be shown in a similar way. The causal influence of the reference signal, however, is on a different variable in the loop -- not the output, m, but the input,p; m=f(r-p) and p=g(m)+d where r is the reference signal. Then, treating f and g as (large) constants we can solve the simultaneous pair of equation to get p ~ r; perceptual input is dependent on reference input. So variables outside of a causal loop can cause variables in the loop to take on certain values. Both d and r, then, can be considered causes with respect to the variables in the loop that they affect. I refer everyone to Bill's article in Living Control Systems on the "Asymmetry of Control" for a nice explanation of why, although both r and d are causes (of p and m respectively), only r actually controls the variable it causes.

The mathematical basis for my argument could probably be made prettier but I think that even in its sloppy form you can see that there is reason to consider r the cause (and control) of behavior, where behavior is the perceptual consequence of outputs determined by those perceptual consequences.

Whew

Back to work

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: Wed, 10 Oct 90 19:48:00 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: David McCord/Psych <MCCORD@WCUVAX1.BITNET>
Subject: Herzog on Powers on Skinner

I forwarded Bill's comments on Skinner to a colleague here at WCU, an animal behaviorist, animal rights expert, sociobiologist, and general trouble-maker. I thought his response was worth sharing. -- David M

From: PRO::HERZOG   10-OCT-1990 16:29:21.09
To: PRO::MCCORD
CC:
Subj: RE: Powers on Skinner . . .

Powers message sounds great... But the problem is that it might seem to ignore the question of function. Using BP's example the reason that we shiver and pull up the blankets when we get sick is that the set point chnages with infectious disease. Big deal. Claude Bernard figured this out over 100 years ago. The more interesting question is why. Here we must turn to the most important area aof modern psychology - reptile research. It turns out that lizards that are allowed to voluntarily raise their body temp when exposed to pathogens get better quicker than lizards that are not allowed to get a "feaver" - no shit. Thus what we need to really understand behavior is a combination of control theory, Darwinism, and reptile ethology. I think that the Psych Dept at WCU will be on the cutting edge of this exciting new frontier.

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Date: Wed, 10 Oct 90 22:44:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

From: TJ0WAH1@NIU.BITNET
Subject: comments

David M.
Give my regards to Andy. Wish I could join you for Bill P's Visiting Scholar presentation--many of us got a preview during the CSG meeting this Fall in Pa. Outstanding!
As regards the question of "internal causes," I fully endorse Rick's very cogent remarks. I believe that the answer to Dennis's concern is that we are using the terms cause and effect synonymously with independent and dependent variable. We are NOT talking about absolute causality, which, as Dennis correctly observes is "everywhere and nowhere," and is, hence, better left unmentioned, even by field theorists. Right, Dennis?

David C.
"Volitional action: Conation and control" (Advances in Psychology, volume 62), edited by myself (Wayne A. Hershberger) and published by Elsevier/North-Holland in 1989, has the ISBN: 0 444 88318 5. Fifteen of the book's 25 chapters were written by CSG members. Copies can be ordered from:
Elsevier Science Publishers
Book Order Dept.
Molenwert 1
PO Box 211
1014 AG Amsterdam
The Netherlands
The book is expensive (about $110.00) but worth ever penny, if I do say so myself.

Rick M.
Thanks for the kind words. As for Skinners behavioristic explanation of temperature regulation, I'll try to type it this weekend for posting on Monday. Although his remarks are anything but lucid, they comprise a marvelous display of verbal magic. Skinner was a consummate polemicist.

Warmest regards, Wayne Hershberger <tj0wah1@niu>
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Date: Thu, 11 Oct 90 08:11:46 -0700
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: Herzog

David M.

Your Herzog is a pretty funny fella.

I do have one little question -- how are lizards "allowed to voluntarily raise their body temp"? How do you control the voluntary behavior of an animal. Maybe lizard ethology is something we should look into if lizard reference inputs are so readily accesible.

Regards
Rick

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Richard S. Marken   USMail: 10459 Holman Ave
The Aerospace Corporation   Los Angeles, CA 90024
Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)
==
Date: Thu, 11 Oct 90 11:21:01 -0500
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: Darwin & Control Theory

I agree with Herzog that if we want to big picture, of course we need to consider Darwinian evolution. Powers has almost convinced be that evolution itself is a type of control system in that the rate of mutations increase when environments change so that existing organisms can no longer control their environment as they did before. The recent research by Cairns and his colleagues shows that E. coli increases its rate of mutation when starved.

Also, from a Darwinian perspective, it would seem to make good sense that organisms with control systems would have a great advantage over S-R organisms. Indeed, it is hard to imagine how an S-R organisms could ever survive for long at all unless the environment (e.g., food sources) was solidly nailed down (i.e., no disturbances) and things are certainly not nailed down very well in the ocean currents).

I am having lunch tomorrow with Norman Packard (one of the early pioneers of chaos theory) who is now working on artificial evolution. The problem as I see it, however, is that he is starting out with artificial bugs that have lookup tables, that is, essentially S-R systems to learn how to find food. I find this strange since chaos theory itself should suggest that S-R cannot work over the long run since errors will just accumulate and get out of hand.

Should be an interesting meeting.--Gary

Gary A. Cziko   Telephone: 217/333-4382
Associate Professor of Educational Psychology   FAX: 217/333-5847
Bureau of Educational Research   Internet: g-cziko@uiuc.edu
1310 S. 6th Street-Room 230   Bitnet: cziko@uiucvmd
Champaign, Illinois 61820-6990
USA
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Date: Thu, 11 Oct 90 19:21:40 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Powers on Delprato on McCord

Gary Cz. I'll want to leave by 8:00 pm -- want to be home in the morning on Thanksgiving. Mary will be working Wed.; can't come. Thanks for the nice invitation, though.

David Mc and Dennis Del:
   I had thought I understood the "integrated-field/systems perspective" as meaning the method of modeling or system analysis. You describe all the relevant variables and relationships in the physical environment of the system (including its actions, their consequences, and independent influences). You then posit the minimum number of unobservable variables

and relationships needed to make a complete system (i.e., you can see how stimuli depend on actions because the direct links are visible, but you have to guess how actions depend on stimuli because the connections are hidden inside the organism). Then you solve the system of equations for the observable variables analytically or by simulation (given the varying states of all variables not originating in the loop) and compare the behavior of the model against observable phenomena.

If the predictions are wrong, you can't change the part of the model that represents the visible variables and relationships unless you made a mistake in observing or got the physics wrong. Juggling descriptions isn't going to fix what's wrong. All that's left to change is the part of the model representing properties of the organism. This is how the method of modeling gradually converges on a picture of the world that underlies observable phenomena. The hard sciences and engineering work by constructing highly consistent models of unobservables. Acceptance requires that the model's behavior match that of the real system within the errors of measurement EVERY TIME. Only those features of a model that ALWAYS work are taken seriously. That's why, when you learn how to see inside the system, the parts of many models are actually found.

The most profound error made by behaviorists was to confuse description with explanation. They didn't understand that explanations based on external descriptions alone leave out the properties of the behaving system and therefore leave the whole system underdetermined. Zero behavior and zero reinforcement fit behavioristic explanations as well as any other values. The observed values can be explained only by saying, "Well, that's how rats (or people) behave." Psychologists haven't distinguished model-based prediction from prediction that amounts only to saying that what has happened before is likely to happen again. That's precisely the distinction between post- and pre-Galilean science -- in those disciplines that have made the transition.

I really don't understand the difference between the "integrated-field/systems" approach and the method of modeling as exemplified by control theory and practiced for several centuries in physics.

The other puzzlement is "naturalistic observation." I have supposed that this term refers to reporting only what the observer can actually experience, as honestly and completely as possible. In my lexicon, that's just called "observing." What's the alternative to "naturalistic" observation, Dennis? I think you're going to have to spell out exactly what you're talking about for those of us who have trouble catching on.

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Date: Thu, 11 Oct 90 19:22:44 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: MODEL REVISION

Out-of-the-blue department. Hershberger's recent comment, plus past suggestions by many others (resisted by me), plus some unknown extraterrestrial force, has created a REVISION OF THE MODEL (maybe, if you think it checks out). The basic problem is that we seem to know what we are doing before we do it. The "imagination connection" partly takes care of this apparent perception of reference signals (i.e., apparently perceiving an output signal), but requires a clumsy and mysterious switch to bring the outgoing signal into the incoming channels where I still think perception takes place. And you can't have imagination and real perception going on at the same time without some really ad-hoc design features that would probably turn out to be bugs. Scott Jordan and Wayne found out that subjects' brains compute the position of the light as if the eye were already in its intended position (but before eye movement to that position starts). Here, I think, is the model that takes care of that and a lot of other problems:

```
                       *r2
              p2       *        e2
                *****  C  *****
                *             *         (you all know which way the
                *             *          arrows go)
                Sense         Gain
           -e1 * * r1         *
              *   *           *           sensor function is
        ^    *     ************           p2 = f(r1 - e1);
        |    *     (Imagined)*            but e1 = r1 - p1, so
        |    *              *             p2 = f(p1), just as in
      Lower  *             *              the old model.
      order  *            *
      ERROR  *          *
      signal  *         * r1              But now the signal
               *        *                 going from lower to higher
            e1  ******  C *****  p1       is the error signal, not the
                *             *           perceptual signal.
                *             *
                Gain          Sense   (note reversal to keep lines
                *             *         from crossing)
```

This does a number of nice things. If some other system completely inhibits the lower-order comparator (which turns off the lower-order system, because you can't have negative frequencies in neural signals), the higher system is automatically in the imagination mode. The subject perceives the intended result, not the actual result. The higher system experiences NO ERROR. When you disinhibit the lower comparator(s), there should be a momentary error in the higher system until the lower one succeeds in making its error signal zero again. The result is exactly the same as in the former model, but the process of getting there is different, and the experience is different.

Comments?

Is my signature file working? .... Bill Powers, just in case.
==
Date: Thu, 11 Oct 90 22:39:04 EDT
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: Delprato on Powers on Delprato on McCord

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

I have the feeling that I am missing something, i. e., a previous posting from Bill on my "causality" comments with regard to David McCord's submission to _Psychol. Record_, or perhaps an additional posting from David. I am unclear as to the basic source of puzzlement.

As far as the first part of Bill's "Powers on Delprato on McCord," I basically agree. I wouldn't restrict field-oriented behavioral science to quantitative modeling research _today_, for this is asking the discipline move too rapidly. I see control theory as a major force for taking the discipline quantitative. Others have tried, but I am not convinced they took us anywhere.

On unobservables: Most certainly I am not advocating any sort of quasi logical positivism that restricts explanations to observables, whatever this might mean. This is why one needs models that are founded in the natural world, as is the case with control theory.

On "natural observation." I am not sure where this came from. I don't think I brought it up in my comments on David's manuscript. There are various referents to naturalistic observation, one of which is that this is a research "method," others of which are correlational method and THE experimental method. I can buy this categorization if we restrict it to ways of categorizing the researcher's behavior. According to this view, naturalistic observation is akin to studying organisms in their everyday, real-life settings, as opposed to artificial settings such as labs and so on.

An alternative to naturalistic observation? This may sound like a "wise guy" response, but how about "supernaturalistic observation?" Unfortunately, the response is all too serious for it refers to an alleged way of knowing that has been offered as a superior way for centuries, i.e., revelation.

In closing, I probably should spell out how I see control theory in the big picture, the major feature of the latter being integrated-field thinking. Given (a) that I wear no one hat or wave no _particular_ banner (apart from an avowed concern with re-naturalizing) the study of behavior--this rules out my being one of those most dangerous types--eclectics) and (b) that I see control theory as a necessary part of the BIG solution, but not the only component, it is not surprising that I may say some things that don't quite sound right. Sometimes they are not. Perhaps at some other times they may feel better after they have been given some time.

Dennis Delprato

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Date: Fri, 12 Oct 90 07:36:04 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Reply to Delprato

Dennis --

I was sort of free-associating on phrases you have used frequently -- but not (as quoted) in the McCord excerpt. Apologies. Anyway, I got too wordy and obscured the point, which was really internal causation and what you DID refer to as "internalistic" explanation and most recently as "supernaturalistic" observation. Correct me if I'm wrong, but it seems to me that you equate the two terms. I see them as different.

The question is where you draw the line in deciding that an observation refers to the public world and when it refers to a private subjective world (or an imaginary supernatural world). I draw it between experiences we tend to accept as actually having occurred and those we accept as imagined. Among experiences that I, at least, accept as real -- because I experience them in myself -- are things like thoughts, imagination, attitudes, feelings, propensities, memories, intentions/purposes, and so on -- all that garbage. But these things are not garbage because of their nature. They are garbage because of the way they have been handled in the past, as quasi-supernatural or "mentalistic" (which, by common usage, means the same thing) manifestations. Or as just "there." While accepting that they exist, I approach them by trying to find explanations of them. Example: imagination. How can we experience something that isn't coming in through the senses? By creating signals in the same channels that perceptions normally follow. And where do those signals come from? My answer: they are rerouted reference signals that would normally reach a lower-order system. That explains (a) why they seem like (sketchy) perceptions and belong to all the same classes as normal perceptions, (b) how they happen to be interpreted appropriately, and (c) how we are able to manipulate them in the manner we call thinking, imagining, or planning (or at all). The Revision, by the way, now implies that the largest component of perception is imaginary, with only deviations from expectations (at lower levels) correcting the imaginary picture. This explains how (as Greg Williams puts it) we can finish reaching for the glass when the lights go out. I think it will explain deafferentation data too.

I think it's profitable to take phenomena that have always been thought of as mysterious and private and fit them into an explanatory model that is consistent with what we observe externally as well. Of course we have to think of tests before we take any such explanations seriously. Does this fit with your concept of naturalistic observation?

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Date: Fri, 12 Oct 90 14:54:00 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: HERZOG@WCUVAX1.BITNET

Subject - lizard temperatures

Body temperatures in can be manipulated in ectothermic species and even in mammals when they are very young by varying the external einvironment. I first read about his in an old article in Scientific American article. There are also references on page 270 in the 3rd edition of "Biological Psychology" by Jim Kalat. The references he lists are Kluger, M.J. The evolution and adaptive value of feaver. American Scientist, 66, 38-43. also Kluger and Rothenburg in Science,

(1979) vol 203, p 374-376.
Also take a look at Satinoff et. a. (1976) Science vol 193, p 1139. It deals with behavioral fever regulation in newborn rabbits.

Hope this is of some use. (The first Kluger paper appeared in a 1978 American Scientist)

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Date: Fri, 12 Oct 90 13:08:35 -0700
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: Genetics

Hi Gang

Time for my "post for the weekend" to get you all thinking and to get me a nice, full set of mail on monday. This post is motivated by a front page story in the LA Times this morning about some research on identical twins seperated at birth that is reported in the current Science. Apparently, this is the "definitive" data showing that "psychological traits" are inherited. The story is interesting to me for several reasons, not the least of which is that the fellow who led the study is Tom Bouchard. Tom has been an interesting character in my life. He was one of the first people I met as a graduate student at UC Santa Barabara. He was actually instrumental in getting me accepted into the program there. He also helped my then girlfriend move from the Dept. in Berkeley to the one at UCSB. At that time Tom was also a radical, being a big gun in the establishment of the Peace & Freedom Party (which is still active in California). He left UCSB to go to The U of Minnesota. Several years later I got a job at Augsburg College in Mpls. I had forgotten that Tom was at Minnesota but was reminded when a local chapter of the Committee Against Racism started protesting his work on behavioral genetics and denouncing Tom as a racist. Rather a funny situation, I though, for a fellow who helped build a polital party whose major platforms were peace in Vietnam and Civil Rights for blacks. A couple years later Tom was getting national press for his work on identical twins seperated at birth and I invited him to speak at Augsburg (which was a bit of a coup since he was already something of a celebrity). I was surprised to find that the long-haired, bearded radical I knew at Santa Barbara had turned into a balding, clean shaven professor in a nice suit.

Anyway, I am also interested in Tom's work for the same reason everyone else is; because it's facinating. For example, they mention in the article that there were these twin brothers, seperated at birth, who were reunited in Tom's lab 30 years later. They had these interesting similarities; for example, they drank the same beer brand and both crushed the can when finished. There are all kinds of peculiar little similarities like that: twins with similar tatoos (as I recall) or similar dress styles, etc, etc. Of course, some of these similarities could be chance. But some-times the similarity of personality styles and preferences just seems uncanny. I find it difficult to believe that there is a gene for "beer can crushing" behavior (or a gene for the reference signal for seeing a beer can in the state "crushed"). But I can imagine a gene for certain intrinsic or higher order references which, when the system is raised in a particular culture, is likely to develop control systems for those references that involve certain kinds of variables, such as beer cans and crushing. Of course, Bouchard and his group do their research in the context of a "trait" model of behavior. I think a trait is like an internal propensity to carry out one set of actions (in response to a stimulus) rather than another.

My question for the weekend; do you think there might be anything of interest in this kind of "behavior genetics" data that could be of use to control theorists? Could you conceive of some more informative way to go about looking for genetic bases for behavior other than looking for correlations between traits? How might control theory incorporate individual differences (and similarities) into the model?

Have a great weekend.

Rick M.

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

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Date: Fri, 12 Oct 90 16:45:32 EDT
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: Delprato: Internal Causes?

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

Wayne H./Causality & Bill P./Internal Causation

Wayne--

Wayne says, "I believe that the answer to Dennis's concern re. internal causationÙ is that we are using the terms cause and effect synonymously with independent and dependent variable. We are NOT talking about absolute causality, which, as Dennis correctly observes is 'everywhere and nowhere,' and is, hence, better left unmentioned, even by field theorists. Right, Dennis?"

Right, although if some sincerely asks about the cause of such and such, we don't say, "There are no causes." Instead, we proceed to describe as many participating field factors as we can. Presumably if a control theorist of goodly bearing has confirmed a model of the phenomenon, we can refer to the model, the point of modeling being to provide an "understanding" of events. And again, I reiterate Bill's "control without a controller." I give warning that I will pilfer this one, but out of good conscience will cite Bill--unless he wishes to retract the "slogan." Any chance for control without a controller becoming trendy science?

On the matter of independent and dependent variables: I find it desirable to restrict the referents to these constructions to the behavior of the scientist or technologist. That is, one can program a disturbance pattern and takes steps to contact the participant's life with it. This can be referred to as the manipulation of an independent variable. HOWEVER, the inherent cybernetic nature of behavior does not give the disturbance pattern a functional status that is independent of the participant's behavior. If the disturbance were merely a stimulus, THEN it would be correct to think of it as an independent variable having ontological status independent of the researcher's behavior. But interdependency, not independence and dependence, is the byword of field/system thinking under which control systems analysis falls. One more word on this before I leave to anticipate disgreement:

When one leaves S----->R, one _ipso facto_ abandons cause----> effect and true independent and true dependent variables. This makes selecting a textbook for Experimental Psychology difficult (I find), but that the way it goes.

Bill--

On the possible equation of "internalistic" explanation and "supernaturalistic" observation: Historically, explanations of psychological events in terms of processes taking place inside the organism have derived from the phase of our culture in which humans were made the repository of transnatural substances and processes. Thus, one is well-advised to be very careful when someone throws around explanations that imply even the participation of internal powers and forces. But, most certainly there are internal factors that participate in psychological events and they are not in the realm of the extra-natural. This is picked up below.

Bill: "The question is where you draw the line in deciding that an observation refers to the public world and when it refers to a private subjective world (or an imaginary supernatural world)." Now, you really said a mouthful. I am referring to your statement that

experiences such as thoughts, propensities, feelings etc. are garbage "BECAUSE OF THE WAY THEY HAVE BEEN HANDLED IN THE PAST, AS QUASI-SUPERNATURAL." And "I think it is profitable to take phenomena that have always been thought of as mysterious and private and fit them into an explanatory model that is consistent with what we observe externally as well." These words could have come from the pen of J. R. Kantor. It was this sort of thinking that stimulated me to go "beyond behaviorism" (actually beyond mainstream experimental psychology) to the strange world of interdependency, participating field factors, systems, radical phenomenology, and so on. My earliest excursions into this new world involved data-based studies on urges, hypnosis, and mind-reading--hardly topics of mainstream experimental psychology. I currently have in progress a study on the "personal world of private experience" that students have in conjunction with college classrooms. The problem with previous attempts to deal with these complexities (THE WAY THEY HAVE BEEN HANDLED) was that they were either denied (metaphysical behaviorism) or permitted to remain spooky (methodological behaviorism, most cognitive approaches).

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

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Date: Fri, 12 Oct 90 15:42:00 -0700
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: Body Heat

Dear Dr. Herzog (and other interested CSGNetters):

Thanks for the reply to my somewhat rude question about how one might go about changing an animal's reference setting for temperature. I really appreciate the references and I will try to look at some of them next week. Of course, I realized that there is a very simple way to get an animal to change its reference for temperture -- just give it the flu.

In your post you suggest that:

>Body temperatures can be manipulated in ectothermic species and even
>in mammals when they are very young by varying the external environment.

If you get a chance (before I get a chance to read the articles), could you please post a brief description of how the researchers were able to tell that the externally induced change in body temperature was a result of a changed reference setting for a controlled variable rather than, say, a result of inability to control temperature (the temperature change in the animal was the expected result of the temperature change in the environment) or the result of "poor" control with respect to an unchanged temperature reference (due to low gain or high sluggishness in the temperature control loop). For example, I could probably set my thermostat to 65 degrees and then keep my room at 75 by putting a pretty good heater in the corner, away from the sensor. If my thermostat has an inefficient cooler (or none at all) it will not be able to counter the environmental disturbance that I created. So it looks to me like the thermostat changed its reference from 65 to 75 when, in fact, it just can't keep the temp at the 65 degree reference. If I have a constant disturbance the system might stay in equilibrium at about 75 (if there are no other disturbances).

If the researchers really did determine that the temperature change in the animals was a result of a reference level change then it is just another example of what Tom Bourbon pointed out; there are people in fields other than psychology who are doing good control theory; which again raises the question I asked about behavioral scientists, viz. "what is their problem?"

Best Regards

Rick M.

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

===

Date: Sun, 14 Oct 90 02:25:31 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: McCord,Powers

DAVID MCCORD: If you have an extra copy of the manuscript you sent to _Psychological Rwould like to see it. Back when you described it, I tried to post some remarks, but I have been fighting our local computer, trying to get it to take uploads -- it seems to win, every time.
BILL POWERS: I have finished revising the program for the tracking tasks in WORLDS. The variable rate on the target in our second condition looks good -- it produces the damndest traces ever I saw, especially when a disturbance is added to the cursor. Perhaps it will fly, this time.

I haven't seen many comments on your suggested revisions to the model. I'm sure you know there are several people pondering your remarks before they reply. The idea looks like one possible solution to the intersensory mapping problems we discussed during the session at your place.

WAYNE HERSHBERGER & BILL POWERS: Wayne, Scott's study looks as though it is onto something good. The possibility that the coordinate system for the visual system changes with changing intendedsitions of the eyes suggests some radical changes in ideas about "motor control." I am not on top of the physiological literature on cortical "motor" centers, but some of that work might suggest similar effects for gross body movements. Are either of you, or is anyone else, aware of similar recalibrations of the coordinates in other sensory systems?

The physiological work I am thinking of is that of, for example, Vernon Mountcastle, who writes of the mtor cortex not as issuing "commands" for muscle contractions, but as indicating where all of the external parts will end up after a discrete movement ends. And Georgopoulos, some of whose work is in _Volitional Action_, might be showing the same sort of thing with his "population vectors" that sweep through "cortical space" quite some time before the limbs move along equivalent trajectories in external space.

It might be worthwhile to look for hints that recalibrations occur in other senses before we try to generalize the process from vision to all of behavio

Tom Bourbon <TBourbon@SFAustin.BITNet>

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Date:	Sun, 14 Oct 90 03:04:11 CDT
Reply-To:	"Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:	"Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments:	Please Acknowledge Reception,Delivered Rcpt Requested
From:	RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject:	WALDEN TWO

Some of the recent postings on Skinner and _Walden Two_ gave what might be a mistaken impression about the community founded on the principles in the book. The community is Twin Oaks, near Louisa, Virginia. It was founded in 1967. From the early 80s until as recently as 1986, I corresponded with a young man who lived there. He was the brother of a student here and we had several opportunities to visit.

From the beginning, I was surprised to learn that Twin Oaks was still there. I had assumed that it died an early death. I was even more surprised to learn that, within the first two or three years, the residents had abandoned many of the principles in Walden Two, and in behaviorism, in general. They were more devoted to their vision of a free community than to Skinner's utopian ideals, as they understood them. They decided, early on, that the society described in Walden Two was unrealistic for them, perhaps for anyone, and that the principles they had intended to follow stood in the way of their higher goals. So, like intelligent control systems, they began changing anything and everything that seemed to need changing. By the 80s, the place had a decidedly humanistic quality.

By 1984, I had sent them copies of what little was available on CST, for the community library. In return, I received two books written by residents. I recommend them highly, for anyone who is curious about the fate of the Walden Two experiment. The books are: _A Walden Two Experiment: The First Five Years of Twin Oaks Community_, by Kathleen "Kit" Kinkade, NY: Quill, 1973; and _Living the Dream: A Documentary Study of the Twin Oaks Community_, by Ingrid Komar, Volume I, Communal Societies and Utopian Studies Book Series, Norwood, PA: Norwood Editions, 1983. I assure you that the community described in those sources is anything but a coercive place operating under what they called "Skinner's scientist puppeteers" -- the "planners" envisioned in Walden Two!

Tom Bourbon <TBourbon@SFAustin.BITNet>

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Date: Sun, 14 Oct 90 10:05:08 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>

Brief comments.

Independent variable (Dennis D. et al).

From the viewpoint of the "scientist or technologist," the manipulated disturbance is a controlled consequence of action. Action varies to make the disturbance be what the manipulator wants to see happening. Both action and disturbance are DEPENDENT variables. The disturbance depends on the action, and the action depends on (a) the current state of the disturbance so far produced, and (b) external influences that interfere with producing the desired disturbance. From the viewpoint of the manipulee, the disturbance comes from outside the loop, arbitrarily altering a controlled variable; hence it is an INDEPENDENT variable. In both cases, a second independent variable exists: the manipulator's intention regarding the disturbance that is to be produced, and the manipulee's intention regarding the state of the controlled variable that is being disturbed. Higher levels are involved in both cases. That's the Powersoid interpretation.

Lizards (Herzog et. al).

Any controlled variable can be manipulated by applying a large enough disturbance (driving a car through a tornado; deep-frying a lizard). Fever can result from (1) an increase of the temperature reference signal in the hypothalamus, (2) loss of sensitivity in the temperature sensors or a decrease in their effect on the hypothalamic comparator (3) a large enough heat input. Presumably, (1) is functional, (2) might possibly be, and (3) is not. To say that (1) is functional for sure means finding the higher-level system that controls an EFFECT of temperature via the temperature reference signal and hypothalamic control system. Also presumably, higher-level or intrinsic systems limit the extent to which organisms can be persuaded to alter their chronic temperatures.

Propensity (Rick M.)

Is an instinctive behavior a propensity to act (pursuing a bug) or to perceive (keeping the bug's image approaching)? Depends on whether you're looking from outside (pursuing) or inside (approaching). I think modelers have to see it from inside. I don't think we can inherit the moves that compensate for the bug's moves. Somebody needs to do those wasp experiments again, paying attention to disturbances.

Instructions (David Mc, aside to Chuck T.).

Instructions need interpretation and so leave room for lots of variance when manipulated. How about this: Compensatory tracking, but also show disturbance as another cursor on one side of real cursor path. Tell subjects that disturbance affects cursor and so does handle. Explain that they are to hold cursor level with target, and also that to do so they must move the handle to cancel the effect of the disturbance. Now let them practice to criterion, opposing disturbance and keeping cursor still (cursor and disturbance visible). Then divide into two groups. One group sees disturbance and target alone and is told to cancel effect of disturbance on (invisible) cursor. Other sees cursor and target alone and is told the same thing (disturbance invisible). Which group shows least RMS error in holding the cursor still? Cursor error is the same as the RMS error between actual and "proper" handle position. I don't think the best subject in the first group will do better than the worst subject in the second.

Threads (All)
It's hard to juggle an infinite number of subjects. How can we keep this net from becoming a collection of random ideas? Or is that exactly what we want? Ow, I'm in conflict. What if we said we could handle, say, six threads at once, so we have to drop one to add another? I'm afraid that if we stick too much to one thread (as recently) we will create a lot of bystanders politely declining to interrupt; if we try to handle too many, we won't carry any of them very far. I think people should feel free to introduce a new thread at any time (even in the middle of an ongoing argument), but if nobody takes it up, put it off a while and try again -- and don't go over n simultaneous threads, where n is any number we agree on. Of course we have to name them and use them as Subjects. Or is this just another of my impractical ideas? Will it happen all by itself? Is the mere suggestion enough? What is the meaning of it all?

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD   (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

Date: Mon, 15 Oct 90 00:32:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: Skinner

Rick M., Gary C., and Herzog:
The following passage (between the rows of asterisks) is B. F. Skinner's rejoinder to my comment that he had overlooked a third type of overt behavior: controlled input. The reference is: Hershberger, W. A. (1988). Some overt behavior is neither elicited nor emitted. In A. C. Catania and S. Harnad (Eds.), The selection of behavior (pp. 107-109). New York: Cambridge University Press.

Skinner talks about temperature control because I used the example of closed-loop temperature regulation to illustrate my point; I wrote: "The household thermostat/furnace system is a commonplace example [of closed- loop control]. Setting the thermostat of such a system specifies the temperature its thermocouple is intended to sense, not the amount of heat the furnace is going to emit. Having set the thermostat, one can predict the indoor temperature, but not the fuel bill. The latter varies with the weather. The indoor temperature, however, is the mechanism's doing."

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BFS: When a room grows hot, I turn the furnace off; when it

grows cold, I turn the furnace on. I do so because I am a biochemical system that operates that way--with heat sensors, muscles, and a nervous system. A thermostat turns the furnace off when the room grows hot and on when it grows cold, and it does so because it is built that way--with sensors, electromagnetic switches, and wires. The resulting change in temperature does not affect the behavior of either of us. We do not show purpose in the sense of being affected by any future event. The difference between us is not so much in how we are built as in why we are built that way.

Some of the behavior with which I control my temperature is the product of natural selection. When I am cold, I reduce the surface of my body by wrapping my arms around me, and when I am hot, I sweat. I do so, not because I am them warmer or colder, but because variations in behavior which had those effects were selected by their contribution to the survival of the human species. Much more of what I do is operant. I cool myself by taking off my jacket and warm myself by putting it on. I do both of these things, not because of the consequences which then follow, but because of what followed when I did so in the past. Thermostats are built in given ways, not because of what they will now do, but because of what they have done when built that way.

Hershberger overlooks the important fact that only living things exhibit variation and selection. Feedback, in its original cybernetic sense, is a form of guidance. A feedback loop, as Hershberger says, is a "monitor." It lacks the strengthening effect of reinforcement. It is not true that "the indoor temperature...is the mechanism's doing," unless "mechanism" includes the furnace. It is only the value of the temperature that is its doing.

Biologists now rarely misuse the term purpose. The human hand is not designed in order to grasp things; hands grasp things well because variations in structure which have enabled them to do so were selected by that consequence and transmitted to later members of the species through reproduction. Psychologists are not yet as careful. I do not grasp a cup in a given way because I then hold it better; I grasp it in that way because when I have none so I have held it better. A variation having reinforcing consequences was "transmitted" to my subsequent behavior through processes commonly called memory.

A guided missile reaches its target because it is affected by radiation from the target. I reach the door of my office because I am similarly affected by radiation from the door. But neither the missile as a physical system nor I as a biochemical one is affected by this instance of reaching the target or the door. We respond as we have been built to respond, in our separate ways.
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Note that Skinner's remarks do not actually address the issue I raised; instead, he persists in beating a dead horse (teleology). His remarks nonetheless, reveal how he conceptualizes the process of temperature regulation (he actually says "I control my temperature"), which is incomplete at best. What he, as a radical behaviorist, could not afford to admit is that regulators keep the value of a controlled variable equal to a reference value. It is interesting to see him duck this point. There is a twist or turn in virtually every sentence. For example, his very first phrase, "When a room grows hot" jumps over the reference value notion by characterizing temperatures qualitatively ("hot") rather than quantitatively (degrees). Then by placing this word, hot, in a context in which the meaning "too hot" is implied he is able to introduce what we call an error signal (a sensed temperature which is above the reference value) without ever considering the notion of a comparison of variables (i.e., Reference minus Controlled). Impressive! Skinner was a verbal magician, a consummate polemicist.

However, Skinner dispatches more than a dead horse. He also dismisses the process of closed-loop control, as if its presence were an illusion all along: "Thermostats are built in given ways, not because of what they will now do, but because of what they have done when built that way." Skinner's distinction between what thermostats "will do" and what they "have done" is gratuitous nonsense--or some sort of weird metaphysics. However, the sentence simply repeats the previous refrain, "Much more of what I do is operant. I cool myself by taking off my jacket and warm myself by putting it on. I do both of these things, not because of the consequences which then follow, but because of what followed when I did so in the past." Since the sentence about thermostats is nonsense, we should be suspicious about this argument as well. Note the uses of the words "do," "operant," and "consequence." First, he says that "what I do is operant." Then he says that "I cool myself by taking off my jacket and warm myself by putting it on." What is the operant behavior, donning/doffing the jacket, or warming/cooling himself? When he then says, "I do both of these things...," to what does the term "both" refer? And, when he says he does both because of past consequences, what sort of consequence is he referring to? Is he saying that "cooling" is the consequence of doffing his jacket, or that some other event (such as, getting comfortable) is a consequence of "cooling himself." The point of this series of rhetorical questions is not to solicit Skinner's (or anyone else's) answer, but, rather, to make the point that his answers would be irrelevant; in point of fact, consequences have consequences--ad infinitum. To distinguish the "consequences of doing x" from "doing x" implies nothing about the nature of the processes involved. It does not imply that the consequences of "doing x" are uncontrolled consequences, any more that it implies that they are controlled consequences (he suggests that the uncontrolled consequences of "doing x" on a previous occasion are responsible for "doing x" now, which of course begs the question, which is, whether the consequence of "doing x" is a controlled consequence in the first place?). And, even more to the point, distinguishing the "consequences of doing x" from "doing x," does not imply that "doing x" is not itself "controlling a sensory consequence" (the many, desireable, uncontrolled consequences of the controlled temperature of my home, effected by a furnace-and-thermostat system, include a relatively warm crawl space in which water pipes do not freeze; the fact that the temperature of the water pipes in the crawl space does not control the indoor temperature does not imply that the indoor temperature is not itself controlled--far from it). Skinner simply missed the point. He appears to have been so enamored with the putative controlling effects of consequences, that he actually supposed that a controlled consequence would have to be a self-controlled consequence. This teleological notion of a self-controlled consequence is the dead horse that he is obviously beating. That is, he is arguing against the proposition that: I am cool now because the coolness I experienced when I took off my jacket caused me to take off my jacket in the first place. Since no control theorist that I know of has advanced such a silly proposition, I can not imagine with whom Skinner thinks he was disagreeing--but straw men of his own making.

Warm regards,
Wayne <tj0wah1@niu>
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Date: Mon, 15 Oct 90 10:23:21 -0700
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: Threads

Tom: Thanks for the post about TwinOaks -- very interesting. I didn't know how far they had strayed from the party line -- though I suppose I should have expected it. I based my comments about the place on my memory of a Nova TV show on Skinner that I saw about five years ago. They had scenes of Skinner visiting the place -- that's why I thought it might still be in existence. All I remember is that the people there seemed happy to have Skinner visit. Skinner seemed very uncomfortable; a patrician amongst the unwashed. There was some effort to communicate with the residents who were saying things like "well, we found that we needed to do it this way" and Skinner would mutter things like "oh, yes, you need to have positive reinforcement" or some such hogwash. Maybe, if you saw the show, you could give a more accurate report.

Wayne: Thanks for the post of your comment on Skinner and his reply to you. Your analysis of his reply was wonderful; really excellent. By the way, I haven't been deluged with any reprint requests for my chapters in Volitional Action; is it just me or is North-Holland just not pushing it to libraries. It would be nice if someone would read the book; there are many great articles. Any news?

Bill P (et al): I think it would be nice to cut the number of threads. I've apparently lost some of the current threads -- for example, I don't know why you were suggesting the "disturbance visible" vs "cursor visible" experiment to McPhail and Tucker. Was there something I missed?

I think that one way to cut down on the number of threads is to pick a thread that we can really continue with (and by stopping the starting of new ones - as I've been doing nearly every two days). One obvious thread that could be developed for some time is the "new model". I have a question for Bill (and whoever else was influential in producing the augmented model -- including extraterrestrials): was there some specific observation that motivated the change. What data does the model handle that cannot be handled by the original model? Any ideas about ways to test the model (especially, a test that would descriminate this model from the original)?

Thanks

Rick M.

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Richard S. Marken USMail: 10459 Holman Ave
The Aerospace Corporation Los Angeles, CA 90024
Internet:marken@aerospace.aero.org

213 336-6214 (day)
213 474-0313 (evening)
==
Date: Mon, 15 Oct 90 13:30:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET

Bill P.:

Your "revision" makes sense to me, but there is a problem. First the good part: Massaging an isometric muscle with a vibrator stimulates the muscle spindles so as to mimic the sort of error signals that routinely accompany a "stretched" muscle (i.e., a muscle length that is longer than the one intended). As your revision predicts, a subject whose biceps is vibrated in this way feels that his arm is being extended (e.g., Goodwin, G. M., McCloskey, D. I., & Matthews, P. B. C. (1972). The contribution of muscle afferents to kinesthesia shown by vibration induced illusions of movement and by the effects of paralysing join afferents. BRAIN 95, 705-748).

The problem: When subjects attempt to move a muscle that is "totally paralized" by curare, they say that there is no sensation of movement. (Although when muscles are partially paralized there is the expected "illusory" impression of an environmental disturbance, e.g., excessive gravity.) Eye muscles may be an exception, but not necessarilly.

Any good ideas?

Everyone concerned with the "control" of evolution:

I was talking to a geneticist at a party last Friday and he mentioned that there are enzymes which repair defective DNA. Perhaps this is the control mechanism involved. That is, the rate at which spontaneous variation ocurs may appear to increase if a control mechanism which normally repairs DNA defects is impaired, allowing defects to accumulate. He said that the typical introductory text includes a chapter about these DNA-repairing enzymes.

Warm regards to all, Wayne Hershberger <tj0wah1@niu>
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Date: Tue, 16 Oct 90 10:31:55 -0700
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

I didn't get any mail today so I'm just checking to see if my computer mailbox is alive. If it is I should get an ACK from the list server. If this does get through I would like to encourage posts from those who rarely post. It would be nice to perceive some feedback from others on the net.

I have not had a lot of free time in the last few days, but I have been trying to implement the new version of Bill's model in the spreadsheet. I want to see if there are any obvious, testable differences in the behavior of the two versions of the model.

Hasta Luego
Rick M.

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

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Date: Tue, 16 Oct 90 17:29:10 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: New Model

Rick M. and Tom B.

Differences with new model. I don't want to get TOO ad-hoc about all this. The new arrangement certainly doesn't apply at the lowest level, where the local imagination path doesn't exist. Maybe only at higher levels, if at all.

Just a glimmer of a notion. The higher system has an imagination feedback path, which can pass through an imagination function. This function can be a model of the way the lower systems operate. The upcoming error signals from lower systems can be used to modify the imagination-model, the criterion being to reduce those error signals over the long haul. If the imagination-model acquires exactly the same characteristics as the path down to the lower comparators, out to the environment, back up through the sensory path, and finally to the error signal in question, there will never be an error signal. By controlling a local model of the lower world, the higher system outputs just the reference signal that will make the lower systems operate without error. This makes the operation of the higher system look open-loop, doesn't it? But of course any deviation of the lower-order world from the model will create error, and normal error-correction. Obviously we have to try to get this thing modeled -- glad you've started, Rick. I'm going to be tied up until next Monday (pleasantly -- visiting David McCord). Surprise me when I get back.

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD   (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

Date: Wed, 17 Oct 90 10:02:50 EDT
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: Nets and threads and needles and knots

I am not worried about the number, type, frequency of the various ideas we have on this net although Bill is correct about this concern for other nets. We are a net with a fairly well specified concern, a knowledge of each other, and a concern for ideas. I think that it is fine to take up an issue that concerns you and go with it and see if others have any suggestions, corrections, or comments. I read "no answers" to a concern of mine as either no one has read their mail today, they have copied it and are thinking about it to answer later or they have no interest or know-ledge to offer. I'm sure that if I were off base someone on this net would tell me either on the net or privately. That is what is great about this net and our continued work on it; I see it as very productive for me just reading the mail and thinking about the issues. It is one of the few exciting things that have happened recently. Since only a few people around here are concern with human behavior it is a thrill to talk with people who are concern about it but have something to say. Keep the threads coming we may have a quilt before the Winter.

Chuck

Charles W. Tucker (Chuck)
Department of Sociology
University of South Carolina
Columbia SC 29208
O (803) 777-3123 or 777-6730
H (803) 254-0136 or 237-9210
BITNET: N050024 AT UNIVSCVM

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Date: Wed, 17 Oct 90 14:42:32 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: P02165@PRIME-A.POLY-SOUTH-WEST.AC.UK
Subject: bitnet.mail

Hello everyone,

Thanks to Tom Bourbon I have just had my name added to the Control Group mail system, and I received a whole lot of mail this morning.

I am now trying to see if I can get back to you. As someone who lives in the rural South West of England, I sometimes find these computers a little tricky.

First of all, a message to Gary Cziko. I think I probably am on Bitnet. I hadn't actually realised I could be reached through Internet. I access you through Earn-relay so I wonder if you can do the same. If you have difficulty, you might try contacting Hank Stam, who is the editor of Theory and Psychology, and who seems to know a lot more about these things than me. He is on Stam.psyc@uncamult.bitnet.

The only message I have at the moment is to say Hi to Wayne Hershberger (and no, I haven't had any reprint requests either) to say that I am interested in the application of control theory to evolution. I have become interested in evolution recently due to my theory that psychosomatic phenomena evolved during the Paleolithic as a form of group adaptation. (Yes, I usually get that response). The issue of repair of genetic material and destabilization is quite interesting particularly in view of the problem of discontinuities in evolution. If you can point me in the right direction, I would be most grateful.

And finally, it has just stopped raining,

Best wishes to you all

Michael E. Hyland
Bitnet:
P02165@UK.AC.POLY-SOUTH-WEST.PRIME-A

Department of Psychology

Polytechnic South West
Plymouth PL4 8AA
England
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Date: Wed, 17 Oct 90 13:23:38 EDT
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: Nets and threads and needles and knots

REALLY FROM: Dennis <DELPRATO@UM.CC.UMICH.EDU>
Echo Chuck.
==
Date: Wed, 17 Oct 90 23:48:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: Welcome M.H. /Volitional Action

Rick M.:
I haven't had any reprint requests for my book chapter either. Indeed, I've heard of no requests for any chapters, as of yet-- but I am optimistic. I understand Elsevier is still in the process of getting the book "reviewed," before pushing it--but I understand your impatience.

Michael Hyland:
Hi. Glad to hear you're aboard. What postings have you gotten? All, or just the recent ones? I believe Gary has been collecting all our communiques in a file which any one of us can retrieve. If you don't already have these, you might want to check with Gary--there were a number of postings concerning genetics and evolution. Sorry to hear that you have had no reprint requests for your book chapter. I am disappointed that Elsevier has not really advertised the book yet. We can always "advertise" it ourselves by religiously citing it. I am confident the book has staying power--and the reprint requests will come in time.

Tom B., Gary C.:
Thanks, again, for getting some more of our European contingent plugged into the network.

Date: Thu, 18 Oct 90 10:12:07 -0700
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: Position Wanted/West SouthWest & Tom's Speech

Gary -- thanks for sending me the post about the job opening at Butler University for an applied cognitive psychologist. It just so happens that I would like to move back into academia. Your post made me realize that it might be worth it to make this announcement to CSGNET. So here it is:

Richard S. Marken, control theorist extrodinaire, would like a position at a College or University in the West or Southwest US. I'd prefer CA but Arizona, Nev or NM are OK). If any of you know of a position opening in that neighborhood, please send me the info by e-mail or regular mail. I want a position where I can teach some courses and continue my program of research on intentional behavior. I have already sent out a couple of resumes. I think that I might have some chance at places that are looking for cognitive types (I already sent a resume to a couple places looking for cognitive psychologists. I figure its OK because intentions are cognitive, no?).

This is not a big emergency but I got the OK from Linda to start looking into it. I really would love to get back into academia -- Ahh, the good old ivory tower.

Thanks again for the post, Gary.

Chuck Tucker: I love your idea about all the different threads. Who cares if no one follows up on an idea. It's survival of the fittest idea. Besides all these threads are saved so nothing is really wasted

You social psychologists are so sensible.

So here is a new thread!!

I finally received my CSG Newsletter and was moved almost to tears by Tom's presidential address. It was so good I read it aloud to my wife. I think he makes a point that we should all be reminded of and reflect on regularly; we are not dealing with absolute truth but with testable models of truth. This is a point that Jacob Bronowski (sp?) made in "The Ascent of Man". The beauty of science is that its "truths" are tentative; they are always open to test. The worst horrors of humanity occur when people are certain; when they know that they have the "truth". This is the nightmare of religion and ideology. As Tom said so well, this nightmare occurs when people believe that certain "ideas are so beneficial and appealing that their truth and beauty must be evident to everyone". Control theory does appear to be beneficial and appealing. But that is not the test of its truth. Control theory is, as Tom said, "just an idea", but a "true" one as long as it stands up to continuous, rigorous and fair testing. It is it's ability to explain a phenomenon -- purposive behavior -- that gives it its value; not its intrinsic beauty (and it does, indeed, have intrinsic beauty as well).

Tom is again right when he says that modelers -- the people who are actually testing the control theory idea -- have run into places where the model does not seem to work. I have had this experience in my work on hierarchical control. Phenomena that seemed to require an explanation in terms of hierarchical control actually did not. It was not so much that the basic idea of control theory was wrong-- but a mistake in how I saw the model being mapped into behavior. The correct model (not the "true" one, but the one that worked) was a non-hierarchical model that controlled a different variable than I had originally guessed.

I want to add something to what Tom said. I believe that there is a misconception about what it means when a model does fail a test. People who look at models in the same way as they look at religious ideas think the model is either TRUE or it is not. The model is seen as "testable" but what is being tested, according to these types, is its TRUTH. Thus, when the model fails (as, I believe, the passive, Darwinian evolutionary model fails) then the conclusion is that the model is FALSE and a radical alternative is accepted (such as creationism). The fallacy here is related to what Tom pointed out in his talk; we don't test the TRUTH of a model; we test its explanatory power. Tom is right; all models are false. That's a good way to start. Testing does not evaluate TRUTH; it evaluates how well the model explains what we experience. Some models ARE BETTER than others; the ones that explain the phenomena and survive the tests. The models that fail in this regard can be considered WRONG: but they are not necessarily useless. I'd say that Alchemical models in chemistry can now be considered WRONG; moreover, they are also not useful compared to the atomic model. The Newtonian model of the universe is also demonstrably WRONG -- but, since it is still useful, I think that it is less WRONG than the Alchemical model. Input-output models of behavior are demonstrably WRONG and, I think, useless in the same way as the Alchemical model(to the extent that that is even a model). This is because the input-output model is actively misleading -- in ways I won't go into here because they have been documented rather fully in Bill's books and just about everything written by members of CSG.

While control theory is not TRUE, it is currently less WRONG than input-output type models of behavior. Eventually, there will probably be a better model to replace control theory, but I can guarantee one thing, that model will not be an input-output model. It will also be a model that can behave purposively, just like a control model. At the end of his 1973 book, Powers himself acknowledged that the details of the model he described may be wrong (some of these details are already being tested and the model augmented) but he said (and I confidently concur) that he would be surprised if the basic organizing principle of purposive systems -- that they are organized around the control of perception-- turned out to be completly off base. Again, in some distant future there may be some super-model that goes beyond control theory in some fundemental way (because failure of some tests of the control model demanded a new approach) but it is almost certainly not going to be a model that says that perception guides behavior.

There is one other little point I would like to make. Although models should not be considered TRUE, even when they have passed all tests to date, they can be considered our best shot at understanding some aspect of our experience. People do care about models because they are part of our system level understanding of our experience. We should not be dogmatic about them and enforce belief in them; that is the job of religion and ideology. But we do care about them. The understanding of human nature that I get from the control model is important to me; it makes me feel satisfied and enobled. It is important to me to show why this model is a better one than input-output models. It is important to me to try to show other people the fundementally different perspective on "how people work" that we get from the control model. But I just think of the control model as a model -- a tentative step toward trying to understand the cause of one aspect of my experience -- the experience I have of other people and myself. But I understand that the model is tentative; it is an approach; it is testable and I expect revisions. Control theory is an extremely satisfying model but we must always remind ourselves (as Tom did in his speech) that it is a model, not revealed TRUTH.

On a different, but related topic: I am going to give a talk in about a month on the value of theory in the practice of human engineering. I wrote a brief article about this once. One of the topics I would like to discuss is "The difference between descriptive and working models of behavior". Does anyone have a nice, succinct way of describing the difference and explaining why descriptive models are stupid. The reason I would like to find a simple way to explain this is because, to the extent that models are used in human engineering (human factors) they are descriptive models. That is, they model a person doing some task in terms of a bunch of boxes that break down the task into components that take some time to do with some probability of error. So the model describes, say, typing behavior but the model can't type.

Any suggestions or comments on this would be greatly appreciated.

Best wishes

Rick
****

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

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Date: Fri, 19 Oct 90 08:27:35 -0500
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: log9009

get csg-l log9009

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

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Date: Fri, 19 Oct 90 09:06:12 -0500
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: Getting the Log

The other message you received from me today shows what happens when a message that should be sent to LISTSERV@VMD.CSO.UIUC.EDU is instead stupidly sent to CSG-L@VMD.CSO.UIUC.EDU. Since I was trying to have the log sent to me of all September traffic, I should have sent the message to the Listserver and NOT for posting on CSG-L. I can assure you that this is a very embarrassing error to make, especially when you are the list manager!

By sending the command GET CSG-L LOG9009 as text in a message to LISTSERV@VMD.CSO.UIUC.EDU you can get all postings for September and this seems to work fine. However, you should be able to also get all October postings (so far) by sending GET CSG-L LOG9010 but when I do this I only get the first message of October. I know the full October file is there since when I use a TELL command from a Bitnet machine it get the full file. I will try to figure out what is wrong here so that any CSGnet subscribers can get these log files easily.

Since we have had lots of traffice and the log files are getting quite long, I will probably switch in November to weekly logs. This will change the filenaming convention somewhat so that the first week of November will be CSG-L LOG9011A, the second week CSG-L LOG9011B, etc. An index of all available files can also be obtrained by sending the command INDEX CSG-L to LISTSERV@VMD.CSO.UIUC.EDU.

On another, but related, matter, Greg Williams has recently written (901015):

"At our recent modeling conference, Bill P., Tom B., Bill W., and I talked about writing up some of the CSGNET exchanges (edited as appropriate, with the aid of the authors) in a newsletter. I'd be willing to do it (typeset, even!) if nobody else wants to, assuming that I could be supplied with files off-line on a continuing basis until (I guess) next June [when Greg expects his own private phone line]. At any rate, I'd love to keep seeing what's happening on the network, even If I can't participate directly. A "CSGNET Digest" with amended and expanded comments by net workers might be welcomed by several individuals. What do yo think?"

I think it's a great idea if (to repeat one of Bill P.'s favorite phrases) I don't have to do it. I can easily supply Greg with the files. This would be a great way for all those who cannot get access to CSGnet to keep up to date on what's going on among those who are on the network. Reactions from others are encouraged and will forwarded on to Greg.--Gary

P.S. From a Bitnet machine you don't need to send mail to the listserver for files. You can simply type TELL LISTSERV AT UIUCVMD GET CSG-L LOG9009 (for example).

Gary A. Cziko   Telephone:
217/333-4382
Associate Professor   FAX: 217/333-5847
  of Educational Psychology
Bureau of Educational Research   Internet: g-cziko@uiuc.edu
1310 S. 6th Street-Room 230   Bitnet: cziko@uiucvmd
Champaign, Illinois 61820-6990
USA
===
Date: Fri, 19 Oct 90 10:31:58 EDT
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: Re: Getting the Log
In-Reply-To: Message of Fri, 19 Oct 90 09:06:12 -0500 from <g-cziko@UIUC.EDU>

I think my error was to use the Bitnet rather than the Internet address; ignore previous message on log.

Chuck
===
Date: Fri, 19 Oct 90 10:05:11 -0500
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: Control Theory and Evolution

Michael Hyland:

I was very pleased to see your interest in applying control theory to evolution. Evolution is also a major interest of mine, or rather what I call Universal Selection Theory which posits that all increases of fit of one system to another must be due to the mechanisms of blind variation and selective retention. (reorganization within control theory is just such an example). I have been very much influenced by Donald T. Campbell's evolutionary epistemology (or what he now prefers to call general selection theory) and it was he who originally told me about Powers and and his work.

I don't have time now to get to much into this, but I will forward to you previous messages from CSGnet which had to do with evolution.

I'm looking forward to some intersting exchanges with you.--Gary

Gary A. Cziko Telephone: 217/333-4382
Associate Professor FAX: 217/333-5847
of Educational Psychology
Bureau of Educational Research Internet: g-cziko@uiuc.edu
1310 S. 6th Street-Room 230 Bitnet: cziko@uiucvmd
Champaign, Illinois 61820-6990
USA
==
Date: Fri, 19 Oct 90 08:38:31 -0700
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: CSGNet

Gary -- I also think it would be great if Greg Williams created an edited version of the CSGNet posts. Maybe there are funds in the CSGroup budget to pay some of his expenses for this -- ie. postage, time? What do you think Tom?

Also, Gary, if you send posts on evolution to Hyland could you send the same package to me. I have deleted all my old local files and I didn't download any of the early posts on evolution. Thanks.

Tom (and whomever else): On rereading my post about your speech I realized there was a point I wanted to make and I didn't make it clearly so let me try again. The point is simply that, because models themselves are controlled perceptions (or a part thereof) they can be expected to be taken VERY seriously by the people who hold to them. And people would be expected to try to defend the model against disturbances. Thus, I think control theory would predict that it would be very hard to do what we expect control theorists (and any other theorist to do) and that is be willing to submit the model to tests (disturbances) and abandon the model when those disturbances do not have their expected effects -- the model predictions fail.

I guess that is why I was saying that it important for us to keep the point you made in your address in our minds at all times. Our own theory says that it should be difficult, very difficult, to act like scientists and abandon cherished ideas (system concepts, principles?) when the evidence is against them. Being a good scientist means a willingness to change references for very high order perceptions when the evidence says that this should be done. I think this means those scientists either have to be willing to reorganize system concepts (which has got to be unpleasant -- and probably the reason people don't give up their religious beliefs readily) or they have to be presumed to have a level of control systems that can adjust system concepts in order to control those higher order variables. Maybe there is a "master system concept" that some people have that makes it possible to change other system concepts, if necessary, without feeling that by doing so they have lost their sense of meaning and value in life.

I think this discussion is related to my earlier discussion of why psychologists have a problem accepting control theory. The "gut level" understanding is, as I said, a system or principle level reference regarding "how people work" (perhaps I should have called it "cerebral level"?). These psychologists would have a hell of a hard time changing this reference since it is likely that they have no system that can change it; the only alternative is reorganization and that can be painful.

So that is why I think your point about the TRUTH of modeling is so important. Our own theory predicts that it should be difficult to do what you correctly said that a scientist must do -- be willing to abandon ideas based on evidence. I can tell you that I know from my own personal experience how difficult it is to operate according to this principle. I have often had cherished models of the way certain behavioral processes SHOULD work. It was not easy to abandon those models in the face of evidence -- there was even the temptation to fudge the evidence (I never did it but, still, I can understand, sort of, those who do -- it ain't right but it is predicted by control theory).

Hasta Luego

Rick M

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

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Date: Fri, 19 Oct 90 14:12:36 -0500
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: Commercial Access to CSGnet

I just found out (why am I always the last to know?) that Compuserve (a commercial telecommunications service that anyone with a computer and modem can subsribe to) has a link to Internet. That means that we via CSGnet (and privately via Internet) can communicate with anyone on Compuserve. It may not be cheap on the Compuserve end (I think it's something like $8/hour plus about $2 for the phone link via a local Telnet number), but it does make us accessible to REAL people (i.e., people who don't live in ivory towers or work for hi-tech industry). There may even be cheaper services available for getting into Internet such as the new IBM/Sears service called Prodigy which I have heard charges a flat $10/month (although I don't know if they provide Internet service yet).

This is how the compuserve link works. To send a message via Internet to compuserve address 123,456, you mail to 123.456@compuserve.com. Note that the comma has been replaced by a period. Compuserve users should say internet:user@domain (eg, internet:csg-l@vmd.cso.uiuc.edu). I can also add Compuserve address to CSGnet.

If anybody knows of other publicly available, commercial links to Internet, please let me know. Also, if you have contact with non-university CSG members who would like to join CSGnet, please let them know of the

Compuserve link. It would be great to have people like Ed Ford and Greg Williams with us.--Gary

Gary A. Cziko   Telephone:
217/333-4382
Associate Professor   FAX: 217/333-5847
  of Educational Psychology
Bureau of Educational Research   Internet: g-cziko@uiuc.edu
1310 S. 6th Street-Room 230   Bitnet: cziko@uiucvmd
Champaign, Illinois 61820-6990
USA
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Date: Fri, 19 Oct 90 14:45:54 -0500
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: Changing Paradigms

I just got through showing parts of Powers' demo1 to a colleague of mind, a behaviorist type.

He was intrigued by the compensatory tracking task, but commented that the demo cheats in computing the correlation between the position of the cursor ("stimulus") and the POSITION of the handle (mouse). The response, he noted repeatedly, is not the POSITION of the handle but rather the direction and velocity of the handle movements. Also, he noted that a time-lag needs to introduced to the correlations to allow for the latency of the response to the stimulus.

He believes that he could take the data of the handle and the controlled cursor and with appropriate statistical analyses show a high correlation between cursor and response, the S-R link using derivatives, time-lags etc.

He did admit that if there was perfect control that he could not show a S-R link since there would be no changing stimulus, but even control theorists would have to admit that is no such thing as perfect control.

Can anyone provide me with a good argument for why his S-R analysis venture can not work? Perhaps it has already been tried. I'm not saying that if it did work it control theory would be in trouble, but this type of reasoning can stand in the way of understanding behavior and I would like some good arguments against it.--Gary

P.S. Christian missionaries have the Bible. Control theorists have Powers' demos 1 and 2?

Gary A. Cziko   Telephone:
217/333-4382
Associate Professor   FAX: 217/333-5847
  of Educational Psychology
Bureau of Educational Research   Internet: g-cziko@uiuc.edu
1310 S. 6th Street-Room 230   Bitnet: cziko@uiucvmd
Champaign, Illinois 61820-6990
USA
==
Date: Fri, 19 Oct 90 14:27:38 -0700
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: S-R Tracking

Dear Gary:

Thanks for the evolution posts.

Also, thanks for the great post about the behaviorist. That's how I got started on this full tilt -- I showed the compensatory tracking task to a political scientist friend and he also said that the cursor must be the stimulus for the response. In fact, anybody would come to the same conclusion as your behaviorist friend -- and they would be right, the cursor is a stimulus for the response (in a sense, it is the deviation of cursor from reference but that can be ignored for now). But the cursor is also a response!! that's what people forget. The cursor is just one component in a causal loop. That's why s-r analysis leads to funny results. Powers demo just shows that if you think of the cursor as the stimulus things come out weird -- like a very low correlation between stimulus and response and a high correlation between (invisible) disturbance and response.

Your behaviorist can go through all kinds of contortions but he will never be able to get a better correlation between stimulus (any measure) and response (any measure) than between disturbance and output (these "any measures" include derivatives, second derivatives and time lags. Actually, he can get a good stimulus-response correlation if he uses, as the stimulus measure, the integral of the error -- difference between cursor and target. I forget the math of why this works -- but when you do this you are obviously incorporating the reference signal into your calculations-- and objectifying it as the "target").

I tried to develop a demo of the failure of the s-r analysis of compensatory tracking that would eliminate the kinds of hypotheses that your behaviorist came up with. The experiment is reported in a cute little paper by me:

R. Marken (1980) The cause of control movements in a tracking task,Perceptual and Motor Skills, 51, 755-758.

All I did was play the same disturbance twicein a compensatory tracking task. The correlation between the responses to the disturbance on the two occasions was, of course, on the order of .99. The correlation between the cursor movements on the two occasions rarely exceeded .2. Thus, everything about the responses was the same but everything about the stimuli was different. So how can different stimuli reliably cause the same response? They don't. There is a causal LOOP, not a CHAIN.

Incidentally, the integral of the stimulus correlates with the response because the value of the integral includes past values of the response. You are basically correlating the response with itself. But you are also integrating in effects of the disturbance; the "stimulus" at any instant is the result of both the disturbance and the response although when control is good it is mostly the result of the response, I suppose. Nevertheless, I think if you do the math you will find (I may be wrong here) that the correlation between the integral of the error and the response will never be as high as the coreelation between the disturbance and the response. You calculus types can solve this for me if you like. Or we could find out empirically.

There are many ways to try to show that the s-r approach to tracking is wrong. But ultimately, the only way is through modeling. Have the behaviorist build a stimulus-response model of tracking. He or she will then see that the only way to make it work is to take into

account the r to s feedback effects; they must be negative with respect
to an internal offset value -- the reference signal; ie the system
must have a PURPOSE.

Best Regards

Rick
****

Richard S. Marken   USMail: 10459 Holman Ave
The Aerospace Corporation   Los Angeles, CA 90024
Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)
==
Date: Sat, 20 Oct 90 13:00:00 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: David McCord/Psych <MCCORD@WCUVAX1.BITNET>
Subject: RE: Position Wanted/West SouthWest & Tom's Speech

Rick,

Sitting here on Saturday p.m. with Bill P., reading mail. Glad to hear of
your academic interests.

David M. McCord, Ph.D.   (w) (704) 227-7361
Department of Psychology   (h) (704) 293-5665
Western Carolina University   mccord@wcuvax1   (Bitnet)
Cullowhee, NC   28723   mccord@wcuvax1.wcu.edu (Internet)
==
Date: Sat, 20 Oct 90 13:18:00 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: David McCord/Psych <MCCORD@WCUVAX1.BITNET>
Subject: RE: Changing Paradigms

From Bill P. visiting David McCord:
Gary, your behaviorist friend is quite correct. The best model uses a time-integrator in the output function, so output velocity is proportional to cursor deviation. However, remind him of two facts: (1) It is cursor deviation from a reference position that matters; the subject may keep the cursor in any relation to the target; and (2) the cursor position continuously depends on handle position at the same time that handle position depends on the integral of cursor error. No fair analyzing just part of the control loop: two variables requires two equations. An analysis that takes into account only the forward relationship will lead to inaccurate predictions -- try it and see.
Let us know the outcome -- a behaviorist would be welcome on the net if he's willing to discuss EVERYTHING.
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Date: Sat, 20 Oct 90 14:36:31 CDT
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: Bibliography

It seems that I just sent Greg Williams control theory bibliography to the network. What I had been TRYING to do was to add this file to the listserver so that anyone who wanted it could retrieve it.

This is a rather long file of about 65000 bytes and may take about 15 minutes to download if you are using kermit into your personal computer. If you already have a hard copy of the bibliography from the meeting in Indiana this summer you may just want to discard this one, although it has been revised since then.

Now I need to go back and find out how to add files to the listserver for CSGnet without automatically sending the file to everyone on the list. Maybe Chuck Tucker can help me with this?--Gary

Gary A. Cziko, U of Ill. at Urbana

William (Bill) T. Powers
===
Date: Sat, 20 Oct 90 15:06:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: Rick, Tom, Chuck, Gary, et al

Rick M.: Please, please send me a copy of your vita! I am in the process of trying to persuade my department that we need to replace a recently-deceased behavioristic colleague, an Iowa rat man, with a non-traditional scholar in this traditional area of scientific psychology; that is, someone who will investigate behavior, but particularly that type of behavior which the traditional behaviorists ignored, namely, controlled input. Short of this, the department will likely hire someone with an applied emphasis, rather than a scientific psychologist.

Tom B.: Reading your "presidential address" made me very proud to be a part of CSG!

Greg W.: Your idea about consolidating our postings in print is CAPITAL!

Chuck T.: It seems to me that the wisdom of your observations about the proper nature and number of "threads" leaves us all with nothing more to say on the matter. You have set the tone admirably.

Everyone: I thoroughly enjoyed Rick's cogent remarks (I always look forward to reading his postings; his head is not only full of interesting ideas, it is invariably screwed on straight) regarding Gary's behavioristic friend's conceptualization of tracking. However, I believe Rick's remarks may be too technical to help Gary's friend, at this point. Here are some less technical, although more wordy, thoughts which may be of help to a behaviorist trying to understand control theory when exposed to it for the first time:

Regarding Gary's behavioristic friend's suspicions, I dare say he is thinking about the control loop's error-driven output. He is half right; he needs to realize only that the "stimulus" for these outputs is an ERROR: a difference between where the cursor is and where the tracker intends it to be. This intended position, is the reference signal of the control loop. When control is good, the position of the cursor on the screen is the tracker's doing (i.e., a voluntary response, or what Rick has called, "behavior in the first degree"--I love it!), precisely because errors automatically drive output in a direction which cancels that error (in a way that Gary's friend, perhaps, is already supposing), thereby keeping the cursor at the reference position that the tracker is, necessarily, intending in the first place. Error is the key concept! And, I would guess that Gary's friend realizes this. I suspect that his understanding falters with the not uncommon supposition that errors are akin to objects; that is, things which populate our immediate environments like pencils, pebbles, and pictures. They are not! ERRORS ALWAYS IMPLY A STANDARD. And since the number and variety of potential standards is infinite, a particular standard must be identified before the tracker has anything to track. And, more importantly, this particular standard has to be known to the tracker, in the form of a physiological "set point," "water shed," "solwert," "reference signal," or what have you (he need not be able to verbalize it; but his nervous system can not possibly identify error with out it). The behaviorist's blind spot is the failure to recognize ALL the conditions necessary for error detection, which include the specification of a standard, either implicitly or explicitly. This blind spot developed with Jaques Loeb's analysis of tropisms. Loeb passed this blind spot on to his student, John B. Watson, who passed it on to B. F. Skinner. Recall how Skinner skipped over error detection by resorting to qualitative language in his rejoinder to my comment about a third type of overt behavior: controlled input (see my posting Oct. 15, 1990). I will bet dollars to donuts that Gary's friend is doing the same thing: overlooking some of the essential ingredients of error detection, either by using qualitative language, or by assigning the standard a value of zero (i.e., using the standard as the origin of measurement) and then supposing that a value of zero is the same thing as nothing at all---essentially the same error the ancient greeks were wont to make when they failed to recognize zero as a numerical quantity; think about it.

Talking about Skinner: There is a supremely ironic, even macabre, twist to APA president Stanley Graham's final tribute to Skinner's memory in the October issue of The APA Monitor: "B. F. Skinner [his ordeal of addressing the APA convention on the occasion of his receipt of an APA Presidential Award--he was weak and dying of leukemia at the time] had given us one last lesson in...[the] dignity and triumph of the human will." "Dignity of the will?" What an unthinking, or unkind, cut! I can almost hear Skinner crying out from the grave, "et tu Stanley?" We critics, at least, give Skinner the honor of understanding him.

Warmest regards, Wayne

Wayne A. Hershberger
Department of Psychology
Northern Illinois University
DeKalb, Illinois 60115
Phone: Office (815) 753 7097; Home (815) 758 3747
Bitnet address: TJ0WAH1@NIU
===
Date: Sat, 20 Oct 90 11:42:30 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested

From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: THREADS

Michael Hyland: Welcome to the net. From what I have seen during the past few days, your self-introduction struck chords in several of our other members.

Rick: Thanks, for your remarks about my "address." I kept it to one page in Ed's format for the newsletter, so I could not develop many of the ideas. It is nice to see you pick up the threads and say exactly what I would have said, had there been more space. And that extends all the way to citing Jacob Bronowski's _Ascent of Man_ -- the episode on "Knowledge or Certainty." That is the most powerful commentary on differences between science and pure faith I have encountered -- and the final scene, in which he scoops up a handful of mud from the mass grave at Auschwitz, is devastating. Many members of his family were in that grave, victims of perverted "science" that was used to justify a "perfecting" of the species.

His penultimate paragraph, in the text of that program, is a masterpiece. "Science is a very human form of knowledge. We are always at the brink of the known, we always feel forward for what is to be hoped. Every judgment in science stands at the edge of error, and is personal. Science is a tribute to what we can know although we are fallible. In the end the words were said by Oliver Cromwell:'I beseech you, in the bowels of Christ, think it possible you may be mistaken.'"

IT IS SO HARD TO DO, when the theory you work with IS elegant and beautiful and powerful! All the more reason to be on guard against zealotry __ and against seeking equivalents of holy texts (sorry Gary, I can't think of the Demos as the Bible, but as damned good demos.)

GARY and Rick: The comments of your behaviorist colleague are common. Bill and I try to address them in our manuscript that was rejected. (We are re-doing it for another submission -- CST types can do what pilots do in wartime, only in reverse: we can paint a crashed plane on our fuselage for every time we are shot down!) Rick already addressed one of the major problems in thinking of the cursor as the stimulus -- it is also the "response" -- simultaneously -- in every moment. In a simple tracking task, another problem with the cursor-as-stimulus idea emerges, IF the participant simply decides to keep the cursor in a different relationship with the target, or to ignore the target all together and move the cursor to trace "pretty pictures" on the screen. In those cases, the reference signal is different than it was in the original runs -- where one might conclude that the cursor is the stimulus and the handle movements are responses. And I am not smart enough to figure out how a cursor, as stimulus, can make that happen. If your behaviorist friend can do that, I will give him full credit for a major accomplishment!

And Rick's comment on modeling is crucial. If your friend knows that his explanation works, ask him to demonstrate that fact. If he does not, then his argument can be dismissed. (Oh, his demonstration must be constrained: it must produce the same results as the CST model, in the same time-frame as the CST model (which means in "real time" -- the time of a run by a person). All weekend on the Cray does not count. And his model must PREDICT what will happen when a random disturbance, that he has never seen, affects the cursor in a subsequent run: this constraint assures that no tinkering with the model is used to make it fit the data and that it is a GENERATIVE model, rather

than a post hoc DESCRIPTIVE model.
If your friend is serious about his claims, he should be willing, and able, to do all of those things and more. We do them as a matter of course in our CST modeling.
And on that, I must remind myself of my address, and of Bronowski's eloquent plea!

Tom Bourbon <TBourbon@SFAustin.BITNet>

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Date: Sat, 20 Oct 90 16:21:27 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: TRACKING

Gary,
The more I think about your friend, the behaviorist, the more I am intrigued by something. Like your friend, nearly every behavioral type who sees the results of CST modeling of tracking SAYS (but does not prove, via modeling) that the cursor, or the cursor-target distance, or change in the cursor-target distance, or ... , could be the STIMULUS and handle movement the RESPONSE. What intrigues me is the fact that practically every variety of behaviorist renounced the S-R model as implausible, or as too mentalistic (because it often leads to a hypothesized reflex arc as an internal cause). Nearly any time someone goes into print and refers to behaviorism as "S-R" psychology, a host of behaviorists jump down the authors throat, all of them asserting that the "reflexological" S-R model is not taken seriously by any major behaviorist.
Now THAT is an interesting turn of events.
Would you do with your friend what I will do, henceforth, with everyone who self-professes a behaviorist lifestyle, yet invokes the S-R, reflexological model to explain tracking data? Simply ask them if the reflexological model is indeed the causal model that undergirds all of behavioristic theory. My bet is that everyone will deny that prospect. If so, ask them why it applies in the specific case of tracking, but in no other. It is time to take this discussion to their turf, rather than assuming we must always defend our position. The inconsistency, and vacuity, of the behavioral accounts of S-R causality have gone on long enough.

Tom Bourbon <TBourbon@SFAustin.BITNet>

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Date: Sun, 21 Oct 90 13:02:56 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: S-R ERROR

Wayne: I am not at all certain that Gary's friend realizes that error, not change in the cursor, is the "stimulus" for handle movements in a tracking task. Since my last posting, in which I suggested that Gary ask his friend to do the modeling needed to test his S-R ideas, I have been working on such a model myself.
Any such purely empirical test is inconclusive, especially when the modeler must try to imagine what the protagonist means by remarks about casual relationships, but so far I have been unable to come up with a handle=cursor, or change-in-handle=change-in-cursor model

that works: all of my attempts fail in the same way -- the handle] movements immediately become essentially uncorrelated with the target, as they must, because the handle is now chasing the cursor that it is simultaneously pushing around.

Unless Gary's friend, or any other advocate of a behavioristic "model" says, explicitly, that by a stimulus they mean the difference between the target and the cursor, then they probably mean something else. ASK THEM! My bet is they will not have thought things through to that level.

If they DO mean that the "stimulus" is the difference between target and cursor, AND IF THEY recognize that the difference that matters is specified by the subject, then they are not really behaviorists at all -- not by any of the generally accepted definitions of behaviorism.

By the way, in my simulations, there is never a chance to do a meaningful prediction of what will happen when the cursors is also affected by a disturbance -- the S-R model fails when it is merely asked to replicate the data from a simple undisturbed run.

THE ADVOCATES OF S-R MODELS SHOULD BE DOING THIS WORK!

Tom Bourbon <TBourbon@SFAustin.BITNet>
===
Date: Sun, 21 Oct 90 14:42:04 -0500
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: Shifting Paradigms

Thanks so much to Bill P., Rick, Wayne, David M., and Tom (did I leave someone out) for your well thought-out arguments for my behaviorist friend's attempts to understand the tracking task.

He went through the demos with me just Friday and he will have a stack of printouts of your responses on his desk Monday morning! I don't see how he cannot be impressed with both the contents of your replies and of their promptness. I'll let you know what his reaction is, or even better, I'll get him on the network so he can tell you himself.

Control theory plus CSGnet is quite a powerful combination!--Gary

Gary A. Cziko Telephone: 217/333-4382
Associate Professor FAX: 217/333-5847
of Educational Psychology
Bureau of Educational Research Internet: g-cziko@uiuc.edu
1310 S. 6th Street-Room 230 Bitnet: cziko@uiucvmd
Champaign, Illinois 61820-6990
USA
===
Date: Mon, 22 Oct 90 03:11:59 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: MORE S-R MODELING

Gary,

And for good measure, give him this! (I'm not sure the net is such a good idea -- instead of sleeping, I want to keep after the S-R model.)

I have gone more systematically through several versions of a model in which "handle = some version of cursor, or of cursor-target, or of target-cursor." I get some really neat effects, but none resembles real tracking behavior. (For those interested in "chaos," when I set "handle = target - cursor," I get some really nice structured chaos, with a periodicity that depends on the lag times I use to make the handle a function of the position of the cursor at earlier times.)

In my earlier posting, I said I didn't get to the conditions in which a disturbance also affected the cursor. That was only because I inadvertantly was starting the cursor moving before the handle kicked in. When "handle = cursor," nothing happens, because there is no reason for the model to start behaving. When "handle = inverse of cursor, or target - cursor," there is behavior, but it is an interesting kind of oscillation back and forth of both the handle and cursor. And neither cursor of handle even approximates the path of the target.

A number of conditions lead to positive feedback and everything quickly blows up.

As I mentioned earlier, a purely empirical exercise like this is not conclusive, but I have much more confidence than before today that there are few, if any, viable S-R models lurking out there. It would be nice if advocates of those models would try some modeling. If they DID come up with something that worked, we would have a lot to talk about!

Tom Bourbon <TBourbon@SFAustin.BITNet>

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Date: Mon, 22 Oct 90 09:33:11 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Barry A. Edelstein" <U21B4@WVNVM.BITNET>
Subject: csg-list

I would appreciate having my name added to your csg-list. Thanks!

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Date: Mon, 22 Oct 90 14:19:34 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Behaviorist Model

Back home again after a most pleasant stay with David McCord, his students, and his family. Many thanks, David and Melanie, for letting me play Grampa. The first accomplishment after getting home was to download the voluminous mail, discard my reader files in my mailbox, and then delete the results in my computer while trying to figure out how to get XYWrite to handle the 130K file. I suspect it was a funny character in the text of the bibliography, but anyway -- thanks, Gary, for rescuing me. Great Sysop.

Tom, Rick, Wayne, et. al., and O.B.F. (Our Behaviorist Friend or Friends):

I have time now to consider the interesting comments of OBF at greater length. OBF noted correctly that the "stimulus" is related most closely to the time rate of change of handle position. That was pretty sharp. So the handle position is the time-integral of the stimulus. The next step, as several of you noted, is to get specific about the definition of the stimulus: is it cursor, target minus cursor, target alone, or what? Analysis will show that target minus cursor shows the highest correlation with handle velocity. Calculating the correlation between handle velocity and that stimulus will then reveal the slope of the regression line and the intercept. The intercept is the reference level (which we assume is determined by an internal reference SIGNAL, but that isn't necessary for those who eschew models of the interior of the system). The slope of the regression line combines input and output sensitivity into a single measure of system gain. OBF wants to put in a transport lag, too: fine -- just make it an adjustable delay to be determined from the data. So we have a system equation that goes

h(t) = k*integral[T(t-tau) - c(t - tau) - (T* - c*)], where
h = handle, c = cursor, T = target, d = disturbance, t & tau = time.

Now, as all the CST people noted, we must take into account the fact that cursor position depends on handle position (and in the complete case, on an independent time-varying disturbance, d). If c = f(h,d), we can substitute that expression for c in the behaving-system equation above and get the total system equation, eliminating c. This tells us immediately that c is not an independent variable: only the target component of the stimulus is independent of the system's behavior. In fact we can express both c and h as functions of the parameters and the two independent variables, T (target) and d (disturbance of cursor).

We can solve the equations by numerical integration, given the measured slope and intercept, and given the time functions of the target movement and the independent disturbance applied to the cursor. This leads to the familiar model that CSTers know and love. By the way, the best delay-time (tau) found by matching the model to behavior is zero, or at least not more than 1/30 second.

Suppose however, OBF, that we leave out the second part of this analysis and just use the behaving-system equation. Because the effective stimulus is the difference between cursor and target positions, we must know both the cursor position and the target position as functions of time to compute the value of the stimulus as a function of time. If we're just trying to reproduce the data from a previous run,

we can do this: there is a record of both target and cursor behavior as functions of time. We subtract one from the other, integrate the result using the measured offset and slope (and delay), and thus compute the behavior of the output variable as a function of time. How closely will the result imitate the actual behavior?

The first problem we find is a sensitivity to initial conditions. Because we're using an integration in the output of the system, we have to supply an initial value of the output. This has to be the first observed value if the rest of the output waveform isn't to be offset from the actual one. There is no way to predict what that initial value should be, so we just have to use the data.

The second problem is that integrations are very sensitive to small measurement errors in the variable being integrated. Any systematic offset whatsoever will eventually lead the output of the computation to deviate farther and farther from that of the real system. Any random errors of measurement will lead to a random walk away from the right value (slowed by the integration process, but inevitable nonetheless). We are talking about hypersensitivity to initial conditions -- one of the phenomena of Chaos, as Gary has noted. We can guess, therefore, that the imitation of the straight-through model will not be perfect, at least over long periods of time.

The control-system model is not sensitive to initial conditions. You can pick any starting value of h, reasonable or unreasonable, and the model's handle behavior will quickly converge to the same waveform as the real behavior. The control-system model will continue to match the real behavior for runs of any length (provided that the system offset or reference level doesn't change -- both models have that problem). Negative feedback prevents the drift due to integration error, and corrects errors of initial conditions.

The worst problem with the straight-through model comes when you try to use the parameters determined from one experimental run to predict the behavior in a second run using different patterns of target movement and disturbance with waveforms unknown to the modeler. Now there is no way to know what initial value to use for the handle position, and there is no way to know what the cursor position will be as time goes on (or initially) so it can be subtracted from the target position to yield the effective stimulus for the next change of output. The next cursor position can't be calculated because the disturbance value isn't known until cursor position and handle position are known. There simply isn't any way to set up the model so it can do a run. The critical data aren't available until the behavior, the target movement, and the disturbance change that produce them have already taken place. Therefore the straight-through model, while perhaps reasonable as a description, is not GENERATIVE. It can be used to describe behavior after the fact, but not to predict it.

The straight-through model, because it uses only one equation to fit the behavior of two variables, leaves the system underdetermined. It can be made to work after the fact by putting in values that can be found only after the data have been taken. It works, therefore, only under exactly the conditions used in generating the data, and does not generalize to

new conditions.

The basic problem is trying to account for two dependent variables using only one equation, a basic modeling mistake you can see illustrated by turning to practically any page in JEAB.

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD   (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

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Date: Mon, 22 Oct 90 14:16:35 -0700
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: OpenLoop Control

Gary and OFB

I just read Bill's latest note on the problems with an S-R appraoch to tracking. It made me realize that I have another study that illustrates the problem in the same way that Bill suggested illustrating it with the model -- that it, see what happens if you look at the response to the "simulus" with the connection from response to stimulus broken. Bill notes that the model would show chaotic effects. I did exactly this study with people instead of a model (unfortunately, I didn't try to compare the people's performance to that of the model). Anyway, I reported my results at the 1981 meeting of the Western Psych Assn. I have a write up which I will post if I can find the Mac file of the paper. If I can't find the file I'll have my sec'y retype it and I'll post it if there is sufficient interest. It's actually a very good demo for a behaviorist, and it doesn't require much mathematical understanding. I should have published the thing -- maybe I will someday.

Very simply, what I did was record the cursur movements during the 1st 15 seconds or so of a compensatory tracking task. When the time was "ripe" (so that there would be a smooth transition) I disconnected the control handle from the cursor and simply replayed the cursor movements from the earlier part of the experiment. There was no break at the point where this "open loop" segment began. I tested at least ten people in the study (I reported results for only five or so) and none noticde the change from regular to "open loop" tracking. So, during the "open loop" phase they were going along as though they had to move the handle to do the tracking when, in fact, they didn't. The s-r view would be that the handle movements during the openloop phase should be the same as those during the closed loop phase; same stimulus yields same response (within the limits of "response variability"). In fact, the handle movements during the openloop phase were consistently NOT like those during the closed loop phase. I measured this in terms of how well handle movements keep (or would have kept) the cursor on target. In the closed loop case handle movements always kept the cursor within about 4 pixels of the target. In the open loop case the deviation (had the handle had an effect) would have been 8 or 9 pixels. For conventional psychology fans, the diffenence was significalt at the .00000001 or so level. Tracking was ALWAYS worse in the open loop condiiton. The handle in the openloop case does move somewhat like it did in the closed loop case; but as time goes on during the open loop session the handle movements get more and more different than they were in the closed loop case.

It's a fun experiment but it does require some deception, so it is hard to use yourself as a subject (my criterion for a good experiment). You can always test to see if you are "in control" by giving a little extra push to see if the cursor responds as expected. An experienced tracker (like me) can tell when things go open loop by the "feel" of the stick. Some of the subjects did notice that the handle got "smushy" at some points but only one (a PhD friend of mine) ever noticed that they were open loop for several seconds at a time.

If I find the file I'll post it (it's only about six pages). I can fax the data to you.

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)
===
Date: Mon, 22 Oct 90 17:38:03 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: origins of life

On the evolution-thread: Origins of life.

Some of you have heard this, but not all. Rick Marken is on the same track.

Sometimes you find new answers by standing an old question on its head. The old question here is "What causes mutations?" Upside-down, this question reads "How can DNA replication be so extraordinarily precise?" We know one answer -- repair enzymes -- but the question can also be asked about an earlier stage of life, back when molecules were hardly able to replicate at all.

In a soup with an energy input and some kind of complex molecules, there will be constant interactions between formation processes and dissociation processes. Formation of molecules alters the substrate, affecting the formation of more molecules. There is feedback. Let's think just about the aspects of molecule formation on which accuracy of replication depends. Not being a biochemist I will have to indulge in arm-waving here, but I'm told by a real biochemist that this isn't just a fantasy:

The molecules that are formed can have three types of effect on the reactions that form more molecules: positive feedback, no feedback, or negative feedback. If feedback is positive, a small change in the formation reactions will lead to larger changes and immediate failure of replication. If there is no feedback, nothing interesting happens. If the feedback is negative, however, any small change in the formation process leads to changes in the formed molecules that act, via the substrate, against the change, tending to limit the amount of change and hence to lead to formation of molecules more similar to the original than if there were no negative feedback. Remember, we're only interested in effects that bear on accuracy of replication -- not on achievement of any particular form of molecule.

As soon as negative feedback appears, the population of molecules with the property of affecting their own formation through negative feedback must enormously increase, at the expense of other kinds. Replication becomes more stable.

Now carry this on for a while, and you'll have molecules that strongly affect the process of forming similar molecules, in a way that resists external disturbances of the replication process. Always the molecules with the greatest negative feedback influence on variables critical to accurate replication win. Sooner or later, however, the limit will be reached where ordinary chemical reactions can't resist increasing disturbances. Then either the molecules go extinct, or a new process with a larger loop gain appears: catalyzed reactions. Enzymes. As soon as enzymes appear, possibly as spinoffs of the process of replication, those that increase the gain of negative feedback loops will greatly improve the stability of replication, because smaller errors will lead to larger disturbance-opposing effects. Now much larger disturbances will be required to make any important difference in the process of replication. The dominant population will consist of molecules that produce amplifying enzymes.

Fast forward. Now we have cells. These cells -- microorganisms -- have developed the next stage in stabilizing replication. They now have sensors that can detect environmental and internal variables on which stability of replication depends. When environmental changes tend to alter these variables, the molecules now begin to undergo spontaneous, random, BUT SMALL changes. The rate at which these changes occur depends now on the difference between the "correct" state of the critical

environmental variables (specified in DNA) and their actual state. We have a reorganizing system with sensors and reference signals pertaining to replication-critical variables. As the error grows, the rate of spontaneous mutation increases. As the error becomes smaller (if it does), the rate decreases, preserving the new form. We now merge with Rick Marken's concepts: we have INTERNAL selection processes that alter not the direction but the rate of mutation, just like E. coli swimming more or less up a gradient of attractants with no means of steering. Unfavorable changes, error-increasing changes, lead to mutations right away; favorable changes delay the next mutation.

And so on until you get to us. Of course in the background there is still Darwin's Hammer, squashing the total failures and leaving behind only the successes. But now we can account more reasonably for the successes, for the fine-tuning of evolution, by introducing some tools more capable of delicate application than a hammer is. A species doesn't have to go extinct if it mutates the wrong way; it mutates again right away. This greatly increases the odds of maintaining stable replication in a changing environment (changing, in part, because of the presence of other replicating systems).

There are limits, of course. As the environment changes over geological time, the disturbances get larger and larger. Up to a point, the adjustments of the molecules get larger and larger in opposition. When no further adjustment is possible, the next increase in pressure is passed through as if no control existed -- passed through, remember, to alter the process of replication, which is all we are concerned with. When that happens, naturally, variations in replication appear -- and in an instant the whole thing blows up, because the crux of control is to maintain stable replication. So we have punctuated equilibrium, explained not by looking for causes of mutation but by asking how such incredible stability became possible in the first place.

Living systems don't care WHAT they are. They only care THAT they are.

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

==

Date: Mon, 22 Oct 90 17:51:59 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Rick Experiment

Rick --

Great experiment. Here's a twist: do one run to match a model to the participant. Then do another run, with the model running at the same time as the person (recording everything as usual for both subject and model). At "propitious moments" (when the two cursor positions are identical), switch which cursor is being displayed, the subject's or the model's. This way you aren't replaying an irrelevant cursor. With a difficult disturbance I'll bet that hardly anyone will have time to figure out that control went open loop, and the drift should be even more obvious.

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

==

Date: Tue, 23 Oct 90 10:53:16 +0100
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <arti9!chen@VUB.VUB.AC.BE>

Subject: Re: Rick, Tom, Chuck, Gary, et al

It seems that I should read the "presidential address" of Tom B.
Can anyone send me this?
Thanks.

Chen
==
Date: Tue, 23 Oct 90 09:53:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: edelston

Barry Edelston:
Welcome to the CSG network. Glad to see you follow my suggestion to join in. We customarily invite new members to introduce themselves and their particular interests in living control systems. Please fill us in.

Warm regards, Wayne

Wayne A. Hershberger   Work: (815) 753-7097
Department of Psychology   Home: (815) 758-3747
Northern Illinois University
DeKalb IL 60115   Bitnet: tj0wah1@niu

==
Date: Tue, 23 Oct 90 10:18:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: please pardon me barry

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Warm regards, Wayne

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

Barry Edelstein:
No sooner did I send my welcome than I noticed I had misspelled your name. Some welcome! Sorry. I'll try again.
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Warm regards, Wayne

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

==

Date: Tue, 23 Oct 90 11:14:41 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Bill Powers by way of Gary A. Cziko g-cziko@uiuc.edu" <FREE0536@UIUCVMD.BITNET>
Subject: origins of life

Don: I know you wanted to get away from the deluge of email generated by the Control Systems Group, but I couldn't resist sending you this bit by your (and now our) friend Bill Powers on evolution.

Might not the history of science also be understandable as type of control system? Perhaps we can see the protective auxiliary assumptions that Lakatos talks about as a way of protecting or controlling the hard core of a theory. However, eventually, if the disturbances (in the form of observations incompatible with the hard core plus auxiliary assumptions) become strong enough, blind mutations of the theory system are produced until a better one is selected. This might handle the so-called, so-perceived "naive falsificationism" of Popper.

Kindest regards, Gary.
==

On the evolution-thread: Origins of life.

Some of you have heard this, but not all. Rick Marken is on the same track.

Sometimes you find new answers by standing an old question on its head. The old question here is "What causes mutations?" Upside-down, this question reads "How can DNA replication be so extraordinarily precise?" We know one answer -- repair enzymes -- but the question can also be asked about an earlier stage of life, back when molecules were hardly able to replicate at all.

In a soup with an energy input and some kind of complex molecules, there will be constant interactions between formation processes and dissociation processes. Formation of molecules alters the substrate, affecting the formation of more molecules. There is feedback. Let's think just about the aspects of molecule formation on which accuracy of replication depends. Not being a biochemist I will have to indulge in arm-waving here, but I'm told by a real biochemist that this isn't just a fantasy:

The molecules that are formed can have three types of effect on the reactions that form more molecules: positive feedback, no feedback, or negative feedback. If feedback is positive, a small change in the formation reactions will lead to larger changes and immediate failure of replication. If there is no feedback, nothing interesting happens. If the feedback is negative, however, any small change in the formation process leads to changes in the formed molecules that act, via the substrate, against the change, tending to limit the amount of change and hence to lead to formation of molecules more similar to the original than if there were no negative feedback. Remember, we're only interested in effects that bear on accuracy of replication -- not on achievement of any particular form of molecule.

As soon as negative feedback appears, the population of molecules with the property of affecting their own formation through negative feedback must enormously increase, at the expense of other kinds. Replication becomes more stable.

Now carry this on for a while, and you'll have molecules that strongly affect the process of forming similar molecules, in a way that resists external disturbances of the replication process. Always the molecules with the greatest negative feedback influence on variables critical to accurate replication win. Sooner or later, however, the limit will be reached where ordinary chemical reactions can't resist increasing disturbances. Then either the molecules go extinct, or a new process with a larger loop gain appears: catalyzed reactions. Enzymes. As soon as enzymes appear, possibly as spinoffs of the process of replication, those that increase the gain of negative feedback loops will greatly improve the stability of replication, because smaller errors will lead to larger disturbance-opposing effects. Now much larger disturbances will be required to make any important difference in the process of replication. The dominant population will consist of molecules that produce amplifying enzymes.

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(BITNET) FREE0536@UIUCVMD   (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

===

Date: Tue, 23 Oct 90 11:51:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: "Where am I?'

Bill P.:
The neat experiment you suggested to Rick, in which the the behavior of the cursor is controlled alternately by the human and by the model reminds me of a delightfull(!) essay by Daniel Dennet of Boston College, entitled "Where am I?" It appears in Dennet's book, "Brainstorms," and in "The Minds I," edited by Dennet and Douglas Hoffsteder(sp?). I know you know of Dennet. Are you familiar with this essay? It is as ammusing as it is insightful. It probably should be required reading for all psychologists who speak of the "self."

Warm regards, Wayne

Wayne A. Hershberger Work: (815) 753-7097
Department of Psychology Home: (815) 758-3747
Northern Illinois University
DeKalb IL 60115 Bitnet: tj0wah1@niu
==
Date: Wed, 24 Oct 90 08:42:15 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <arti6!chen@VUB.UUCP>
Subject: Re: origins of life

Bill P., Rick M. and Gary C. etc.:
After reading your posts about evolution and the origin of life, I think maybe you are interested in the following book:

%A E. Jantsch
%T The Self-Organizing Universe
%I Pergamon
%C Oxford
%D 1980

This is a very interesting book. It uses self-organization to explain the whole universe (from the big-bang to the life etc, everything included.). The main idea comes from I. Prigogine (a Nobel Prize laureate from the Free Univ. of Brussels). Prigogine uses symmetry-breaking (order through fluctuation) to explain the formation of dissipative structures. It seems to me that the control theory is useful to explain the life in a small scope (such as the control of movements). But in a large scope (such as evolution or the formation of new structures), self-organization paradigm is more suitable. In an evolutionary system (far from equilibrium) the important thing is not to control input or output, but the co-evolution of the organism and the environment.

Chung-Chih Chen
Artificial Intelligence Laboratory
(Building K, 4th Floor)
Free University of Brussels
Pleinlaan 2
1050 Brussels, BELGIUM
(email: chen@arti.vub.ac.be)
==
Date: Wed, 24 Oct 90 08:32:24 EDT
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>

October 23, 1990

Dear CSGNET:

I have been tied up lately reading examinations where I ask students about cybernetic control theory and just keeping up with my classes. I have read all of the postings through October 22 and am still very excited and pleased that Gary started up this network. All of the postings (threads) have been useful to me in various ways but I hope everyone realizes that the knowledge displayed on this net far exceeds what you can find in the best "institutions" of higher learning in this world and certaintly greater than I have found on any network. This is a way of thanking everyone for their contributions and quite indirectly reminding those who wish to work in these great "institutions" of higher learning that you should make certain that this net is available so you can continue to learn and teach.

I wish to make a few comments on Tom's address as well as others' postings regarding it. I found that Tom's remarks hit the target with me but my references are quite different from Tom's and others. For me what his address reminds me of is the pragmatic approach of John Dewey, George Mead, Arthur F. Bentley and Charles S. Pierce. The citations I would give are Dewey's THE QUEST FOR CERTAINTY, 1929 and Mead's "A Pragmatic Theory of Truth" (1929). Both put forth the position that science is a means to solve problems rather than the pursuit of ultimate truth. My friend Bob Stewart hands out a list of statements to his students stating his pragmatic view which has these statements:

> Scientific facts and theories do NOT describe reality.
>
> No scientist in any discipline, including physics, has discovered, or can discover, any law or principle of nature, or has or can gain knowledge of reality.
>
> Discovering the laws of social life is NOT possible, or even sensible.
>
> Scientists in all disciplines provide ways of solving problems people are having.

These statements can be best understood in a pragmatic or control theory mode. I think that we should remember these statements (and others like Tom's, Rick's and all of those who have commented on these statements) when we deal with our problems and questions about human activity. This leads me to a comment on Rick's question regarding what he should do in answer to the questions about descriptive and working models.

I think that Rick would agree that models are not smart or stupid, true or untrue but rather useful for particular purposes. The question, it seems to me, is what is the purpose of the model and does it accomplish its stated purpose. If the descriptive model is supposed to tell one how an organism or system or process works but fails to do its job then it is not the type of model that is useful; one must develop another model that will do what you intend for it. I have used various terms for different types of models, such as: metaphor, analogy, statistical, replication, working, operational and cybernetic. Each one, at least as I describe them does something quite different and depending on what I think the audience understands (e.g., sociology 101 students versus sociology 500 students) I contrast a model that tells one how a system work with one which claims to do that but upon examination fails on a number of counts. This seems to work for me and I avoid the polemical position of having to claim the one is "good" while the other is "bad". I suggest that whatever terms you use for the models that the approach of asking about their purpose is not only consistent with a pragmatic approach but is quite consistent with the cybernetic control model. I hope that you (or whomever uses this tactic) will report back to us so we can all learn how we can better present this very important matter to others.

I think it would be very useful for Greg Williams or someone to make a sample or summary of our remarks available to other interested parties. We must remember that all we say here is public (even though I have difficulty getting a log of our postings) so there is no copywrite problem but everyone would have to understand that the "editor" can take or leave any statements that he/she wishes unless we ask in advance not to have any of our particular statements repeated. It would still be the choice of the "editor" to honor any request. I don't mean to get legal here but I don't think it would be helpful to the net if anyone got offended by such an activity. I think that our purpose here is to first develop our ideas on certain questions and issues and second to communicate those to others who are not on the net (it is possible for anyone to get on the net). I hope that these remarks have not offended anyone.

In closing for now I want to tell y'all that I will be in New York City this weekend and if anyone wants me to contact anyone please let me know and I will try.

CHUCK TUCKER UNIVERSITY OF SOUTH CAROLINA COLUMBIA SC N050024 AT UNIVSCVM
===
Date: Wed, 24 Oct 90 08:36:41 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: RETURN <FREE0536@UIUCVMD.BITNET>
Subject: Self-Organizing Systems

Chung-Chih Chen -- glad to hear from you.

I have a feeling that you'll be hearing from others on the subject of the "small scope" of control theory, so I'll confine my comments to "symmetry-breaking order through fluctuation." I guess my argument concerning Prigogine is the same one I have concerning Schroedinger's Cat -- a lot of detail is left out, and it's the details than ultimately make or break an explanation. Those, and the unspoken assumptions that always underlie arguments from general principles.

For non-physicists and other amateurs like me: Erwin Schroedinger set up a thought experiment in which the fate of a cat in a sealed box depends on the decay of an atomic particle. When the particle decays it is detected; the apparatus could then, for example, release a hammer that swings down and breaks a container of cyanide gas. What is the state of the cat at any given time? "Quantum realists" have decided that the cat is in fact both alive and dead until someone opens the box and looks -- then its quantum state collapses into one of the alternatives.

Penrose (The Emperor's New Mind) put a second observer inside the box, to show that an internal observer and an external one may or may not experience a different reality. This suggests that we should ask about some details.

WHEN does the quantum state collapse? When the cat staggers about and collapses (does the observer have to be human? Intelligent?)? When the image of its collapse falls on the observer's retina? When the visual information crosses the sensory boundary? When the signals arising there are recognized in the midbrain of the observer as a sick cat? When the movements begin to suggest to higher centers that the cat is expiring? When the observer creates a string of thought-symbols that say "the cat is dead?" And what if the observer thinks "The cat is sleeping" -- prior to further tests? Exactly WHEN is the cat "dead?"

I think I have a lot of questions like that about Prigogine's offering, Nobel Prize or not. His general principle seems convincing enough -- more complex structures are possible when random fluctuations make a new stable state reachable (provided there's enough energy input to support it). But he's trying to make this process work without any selection criteria other than chance. My proposal loads the dice by saying that in a living system the occurrence of negative feedback in the relationship between system and environment strongly affects the RATE of fluctuations, so that non-negative-feedback relationships are quickly disposed of by immediate repetition of fluctuations, and negative-feedback organizations are preserved by putting off the next fluctuation. The fluctuations are randomly directed, but not randomly timed. I think that through eagerness to show that there is no supernatural guidance of evolution, many scientists overlook some perfectly physical means of guidance that could work a heck of a lot better than chance.

Prigogine could doubtless show that this design principle, too, would fall under his general self-organizing principles. But the principles would never have led to this design. Physicists, I think, tend to believe that principles have some organizing power of their own just because they're true. But that is like saying that a bridge obeys the laws of conservation of mass and energy; that's true, but it won't lead to a design for a bridge that doesn't collapse when you step on it. The Big Picture, says Mary, does not provide generative models. The details matter.

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD   (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

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Date: Wed, 24 Oct 90 09:13:08 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Self-Org:Corrections

SECOND TRY: KERMIT OR SOMEONE DROPPED LOTS OF CHARACTERS
Chung-Chih Chen -- glad to hear from you.

I have a feeling that you'll be hearing from others on the subject of the "small scope" of control theory, so I'll confine my comments to "symmetry-breaking order through fluctuation." I guess my argument concerning Prigogine is the same one I have concerning Schroedinger's Cat -- a lot of detail is left out, and it's the details than ultimately make or break an explanation. Those, and the unspoken assumptions that always underlie arguments from general principles.

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(BITNET) FREE0536@UIUCVMD   (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

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Date: Wed, 24 Oct 90 11:11:37 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Technical Note

EVERYONE WHO DOES OR MIGHT DO REAL-TIME EXPERIMENTS

I quote from a recent letter from Real Time Devices, 820 N. University Drive, P.O. Box 906, State College, Pennsylvania 16804 USA, 814/234-6864:

"As per your [phone] request, enclosed please find two each, AD712AQ Analogue Devices Op Amps. We normally sell them as spare parts at $3.60 each, but since two pieces doesn't make a minimum order, I've enclosed them to you at no charge."

I blew out these op amps on my Analogue-To-Digital board, and called to purchase a replacement.

The PC/AT-compatible AD200 (which I bought for $250 two years ago) now has three timers and 24 digital I/O pins as well as the original 4 channels of 12-bit analogue inputs (8 KHz throughput, 1/4 that for 4 channels). It now sells for $235. A terminal-board and cable (XB40) is $64. Programming is extremely simple.

The top-of-the-line ADA2000-2xx(8 microsecond conversion, 5 or 10

volt input uni- or bi-directional, two 12-bit analogue outputs, software-controlled gain, 8 differential or 16 single ended 12-bit analogue inputs, 16 unbuffered and 24 buffered digital I/O bits, 3 programmable counters, PC/XT/AT compatible) sells for $589. There are many models between the extremes.
I don't think you can do much better than this, in several departments. Ask for their catalogue.

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

==
Date: Wed, 24 Oct 90 15:57:01 EDT
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: My copy had some words deleted so I am sending it again.

October 23, 1990

Dear CSGNET:

I have been tied up lately reading examinations where I ask students about cybernetic control theory and just keeping up with my classes. I have read all of the postings through October 22 and am still very excited and pleased that Gary started up this network. All of the postings (threads) have been useful to me in various ways but I hope everyone realizes that the knowledge displayed on this net far exceeds what you can find in the best "institutions" of higher learning in this world and certaintly greater than I have found on any network. This is a way of thanking everyone for their contributions and quite indirectly reminding those who wish to work in these great "institutions" of higher learning that you should make certain that this net is available so you can continue to learn and teach.

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>
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>
> Scientists in all disciplines provide ways of solving problems people are having.

These statements can be best understood in a pragmatic or control theory mode. I think that we should remember these statements (and others like Tom's, Rick's and all of those who have commented on these statements) when we deal with our problems and questions about human activity. This leads me to a comment on Rick's question regarding what he should do in answer to the questions about descriptive and working models.

I think that Rick would agree that models are not smart or stupid, true or untrue but rather useful for particular purposes. The question, it seems to me, is what is the purpose of the model and does it accomplish its stated purpose. If the descriptive model is supposed to tell one how an organism or system or process works but fails to do its job then it is not the type of model that is useful; one must develop another model that will do what you intend for it. I have used various terms for different types of models, such as: metaphor, analogy, statistical, replication, working, operational and cybernetic. Each one, at least as I describe them does something quite different and depending on what I think the audience understands (e.g., sociology 101 students versus sociology 500 students) I contrast a model that tells one how a system work with one which claims to do that but upon examination fails on a number of counts. This seems to work for me and I avoid the polemical position of having to claim the one is "good" while the other is "bad". I suggest that whatever terms you use for the models that the approach of asking about their purpose is not only consistent with a pragmatic approach but is quite consistent with the cybernetic control model. I hope that you (or whomever uses this tactic) will report back to us so we can all learn how we can better present this very important matter to others.

I think it would be very useful for Greg Williams or someone to make a sample or summary of our remarks available to other interested parties. We must remember that all we say here is public (even though I have difficulty getting a log of our postings) so there is no copywrite problem but everyone would have to understand that the "editor" can take or leave any statements that he/she wishes unless we ask in advance not to have any of our particular statements repeated. It would still be the choice of the "editor" to honor any request. I don't mean to get legal here but I don't think it would be helpful to the net if anyone got offended by such an activity. I think that our purpose here is to first develop our ideas on certain questions and issues and second to communicate those to others who are not on the net (it is possible for anyone to get on the net). I hope that these remarks have not offended anyone.

In closing for now I want to tell y'all that I will be in New York City this weekend and if anyone wants me to contact anyone please let me know and I will try.

CHUCK TUCKER   UNIVERSITY OF SOUTH CAROLINA COLUMBIA SC   N050024 AT UNIVSCVM

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Date: Thu, 25 Oct 90 09:32:13 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%arti6@VUB.VUB.AC.BE>
Subject: the role of fluctuations

Dear Bill,

You said:

>   I think I have a lot of questions like that about Prigogine's
>offering, Nobel Prize or not. His general principle seems convincing
>enough -- more complex structures are possible when random fluctuations
>make a new stable state reachable (provided there's enough energy input
>to support it). But he's trying to make this process work without any
>>selection criteria other than chance. My proposal loads the dice by

>>saying that in a living system the occurrence of negative feedback in
>>the relationship between system and environment strongly affects the
>>RATE of fluctuations, so that non-negative-feedback relationships are
>>quickly disposed of by immediate repetition of fluctuations, and
>>negative-feedback organizations are preserved by putting off the next
>>fluctuation. The fluctuations are randomly directed, but not randomly
>>timed. I think that through eagerness to show that there is no
>supernatural guidance of evolution, many scientists overlook some
>perfectly physical means of guidance that could work a heck of a lot
>better than chance.

If I don't misunderstand, you regard the fluctuations as a source which will destroy the stable state. So you use NEGATIVE feedback to maintain an organism. But for Prigogine, the fluctuations are the source to create new organisms. So he uses POSITIVE feedback to amplify the fluctuations to reach new states (this is the internal self-amplification, see p.44 of The Self-Organizing Universe.).
So your selection criterion is the negative feedback.
But his is the positive feedback.
I wonder which one will create new organisms?

The other point is that evolution is not just random selection. Darwinian evolution is that life adapts to the environment one-sidedly. But the self-organizing evolution is the CO-EVOLUTION of the life and the environment. Life creates its suitable environment and adapts to it at the same time.

Chung-Chih Chen
Artificial Intelligence Laboratory
(Building K, 4th Floor)
Free University of Brussels
Pleinlaan 2
1050 Brussels, BELGIUM
(email: chen@arti.vub.ac.be)

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Date: Thu, 25 Oct 90 13:04:43 -0700
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: trendy science/models

To Chung-Chih Chen: I think the reason control theorists tend to get testy when they read something like

>of dissipative structures. It seems to me that the control theory is
>useful to explain the life in a small scope (such as the control of
>movements). But in a large scope (such as evolution or the formation of
>new structures), self-organization paradigm is more suitable.

is because they have heard very similar things under less friendly circumstances. I have heard this sort of statement many times -- not with respect to "evolution or the formation of structures" but rather about "more complex behavior like thinking etc". What I hear is that "control theory may be able to handle the simple stuff like tracking or moving fingers but it can't explain what the big boys are dealing with -- the real heavy duty cognitive stuff like planning and understanding and whatever. For that you need dissipative structures or connectionist theories or whatever the current TRENDY idea is. It is just heartbreaking to hear this stuff.

I am reading (really, skimming since there's not much meat to it) THE IMPROBABLE MACHINE by Jeremy Campbell who seems to have gone from homage to the last trendy group of cognitive psychologists -- the information processing/von Neuman mind model types in GRAMMATICAL MAN -- to the new one, the connectionists. It is heartbreaking because these folks are missing the whole point of the exercise; the reason that any of this "cognition" is done at all; they are trying desperately to ignore its purpose. So we get a purposeless cognitivism that is only superficially based on actual observation.

On the other hand, there is Bill Powers' model of mind based on control theory. This model explains everything that these trendy models (really, formalisms) claim to explain, and it does so in the context of a coherent overall model of organismic behavior in general. It is heartbreaking to see cleverness (of the dissipative structure types, connectionists, etc) mistaken for wisdom and to see the real wisdom go unnoticed by mainstream scientists-- particularly those who could use their cleverness to expand the control theory to new realms. Ah well.

There are all kinds of other things I want to fume about but I'm off on a business trip soon and don't have time to write. Let me try one quickies:

To Chuck Tucker: I think I am a bit less pragmatic in my attitude toward models than is Dewey (maybe not). I do think there is a TRUTH; I just don't think we'll ever get there. I think models are useful to the extent that they seem to take us closer to the truth. This means that they explain our observations better than any other story that we've been able to come up with. The danger comes from imagining that a currently excellent model is THE truth. It is a tentative truth; but a good model is more than just useful (in terms of explaining the data or in terms of helping people). I think it also gives us a new understanding of things -- one that influences how we interact with (our experience of) the world (though not necessarily in a pragmatic way).

I know this is fairly vague and I'm willing to be swayed but I think, for better or worse, successful models change our picture of "reality". This change can be useful -- it can get you more food and better computers - but it can also lead to problems. It is now pretty well accepted, based on chemical and physical models-- that the world has been around for several billion years and that people were not its first(or its most successful -- in terms of longevity) inhabitants.
Many people who claim that their rules of conduct are based on words supposedly inscribed on a tablet a few thousand years ago find this model a real problem -- it is not useful to them at all. I think getting closer to the truth through modeling is awfully satisfying but its usefulness (except in terms of this personal satisfaction) can be quite elusive.

Maybe we can get into a more coherent dialog about this next week; it's interesting to me for several reasons, not the least of which is my interest in figuing out how to tell people why I think my work on the model is so damned useful (and I do think it is).

Hasta Luego

Rick M.

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

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Date: Sat, 27 Oct 90 14:16:26 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Transport lag

I guess it pays to think again about Truths, as Tom Bourbon said in his CSG Presidential Address. My Truth was that transport lags in compensatory tracking experiments were too short to measure. Think again.

I set up a new analysis of compensatory tracking data, with the output function, as usual, being a leaky integrator: the output function is $h := h + (g*e - h)/s$, where g is steady-state gain, s is fraction of change allowed on one iteration (so dt/s is the time constant with dt = 1/framerate), and e is the error signal. That gave the basic model, assuming no perceptual lag.

Then I ran a two-loop solution to find the best perceptual lag. The outer loop put a transport lag in the perception that ranged from 0 to 15 70ths of a second (the frame time on a VGA screen). For each delay I ran the model with a perceptual function of $p := p + (c - p)/t$, where p is perceptual signal, c is cursor position relative to measured reference level, and t is the perceptual slowing factor. I started t at 1 on each iteration of the outer loop, and increased it until the RMS error stopped falling and rose again, saving the value of k for the minimum RMS condition. Each time a new best minimum in RMS error, model - person (handle), was found, I saved the parameters and the model's record of handle positions in "best-of-the-best" variables. At the end, the best parameters were:

Perceptual integral lag: 0.000 sec.
>>>>>>> Perceptual transport lag: 0.157 sec.<<<<<<<<<<<
RMS error, model-person, no lags: 111 raw units (max handle = 2048)
RMS error, model-person, with lags: 59 raw units
Correlation, model:person, no lags: 0.9828
Correlation, model:person, with lags: 0.9887

You real mathematicians out there can just stop snickering. This is how I did it and it works, and if you have a better way send it to me IMMEDIATELY. This way is slow.

By rights I should now go back to the first computation of the basic model, put in the transport lag, and iterate the whole thing until I get the same values each time. Later. For now, I tender an apology to all those who believed me when I said that transport lags don't appear in continuous tracking (at least not greater than 1/30 second). The best model involves a leaky integrating output function and a transport lag that is easily measured and quite stable. OBF was quite right to raise a red flag about the lack of a reaction time. I'm glad he brought it up, because who knows how long I might have gone on spouting the Truth.

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

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Date: Sat, 27 Oct 90 15:01:06 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Evolution and Prigogone/Chung-Chih Chen

Dear Chung-Chih Chen

Which is first & last name? If you get to call me Bill (like everyone else) then I get to call you Chung-Chih -- or is it Chen?

I don't have any quarrel with Prigogine's proposition about how organisms get out of one stable state and move toward a new one. If he wants to propose that there's positive feedback, OK with me. Eventually, of course, he's going to have to find out if that's really how it works, but in this area we're all guessing a lot.

My question concerned why there is a stable state to get out of. A physicist tends to think of stable states as a marble in a bowl -- a minimum-energy or minimum-something configuration. But I'm proposing that at least in more advanced organisms (say, single cells), the stable state is NOT a minimum-energy state, even a local minimum, but is actively maintained by feedback processes -- a marble on top of a ball, held there by an error-detecting and correcting system. All that has to happen to get out of THAT kind of stable state is for the control system to lose control -- every direction is then down. The farther you deviate from the top of the ball, the more disturbance there is. As long as the gain of the control system is greater than the downward slope, the ball remains more or less on top. But a disturbance larger than the system can handle will start the positive feedback process (falling off).

Once you're out of a stable state, the question is then what the next stable state will be. My claim is that once organisms evolve enough to have a reorganizing system, they can institute random changes that produce new states -- but that the random walk is strongly biased by the fact that if the new state has results that are not satisfactory to the organism, another random change is immediately generated. If the result lessens the error signal in the genetic control system, the next change is postponed, so the new form persists. Control uses variations in the timing of changes, the changes themselves still being random.

This introduces bumps into the terrain that are not put there by the environment. What the organism considers to be a local maximum (or better, a local minimum of error) might not correspond to any bump created by the environment treating the organism as a passive element. The organism might reach a stable state that, to an onlooker considering only physical relationships, might look as if it's on the side of a hill or in a valley.

In human beings, most of the bumps are specified in the human genome; the environment has very little to say about it any more.

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

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Date: Sun, 28 Oct 90 21:06:58 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: More on Transport Lag Model

From Bill Powers, for modelers and OBF:

In the following: r = reference signal; e = error signal; p = perceptual signal; c = cursor position; h = handle position; g = integrating gain; k = integrator leak factor; tau = transport lag.

The model I'm now using to analyze conpensatory tracking goes as follows:

Output Function:
h = g*int(e) - k*int(h)
This is a leaky integrator: handle position is integral of error signal minus am amount proportional to the handle position (the "leak").

Comparator
e = r - p
Error signal is reference minus perceptual signal. Reference is taken to be the average cursor position.

Input function (with perceptual transport lag)
p = c(t - tau)
Perceptual signal at time t is equal to cursor position tau seconds ago. It doesn't really matter where in the control system you put the transport lag. Gain of input function is 1.

c = h + disturbance
Cursor position equals handle position plus value of disturbance taken from same table used during live run.

Differentiate the output function:

dh/dt = g*e - k*h

or

dh/dt + kh = g*e

DO ALL THE FOLLOWING FOR EACH VALUE OF TAU FROM 0 TO MAX:

We want the value of g for best fit to the experimental data. Make two arrays, Num and Denom, where one element of Num is dh/dt + kh and the corresponding element of Denom is the error signal e, which is [r - c(t - tau)]; both arrays are indexed by t. I use a handy correlation routine to get the slope of the regression line such that Num = g * Denom. The regression slope is the gain, g. You start with a trial value of k such as 0.001.

Next, use the output function equation to get
g*int(e) - h = k*int(h)
Now the array Num is [g*int(e) - h] and Denom is int(h), both indexed by t. Do the correlation; the slope of the regression line is k.

You might want to iterate these calculations two or three times: they converge very rapidly because k has only a minor effect. You actually need to do them only once, as we do not reinitialize g and k when we change tau: they converge fast enough to keep up. If you want to be pure you can iterate until k and g stop changing, before going on.

Now do a run of the model after initializing h:

First, establish an array LAG of reals to hold past values of the cursor position: initialize it to zero. Use real variables wherever possible in the following loop.

Initialize h to the first actual value from the live run.

On each iteration of the loop from i = 0 to ndata:

1. Move the elements of LAG toward the high (delayed) end by one slot.
2. Fill in the 0th element with (h + disturbance[i]) which is the current cursor value, using, of course, the same disturbance used during the real run.
3. p = LAG[tau] (i.e., perception = lagged version of the cursor)
4. e = r - p (if you want to be explicit)
5. h := h + g*e - k*h   (intyegrating output function as above, except discrete)
6. plot h (model handle) and handle[i] (real handle).
7. accumulate squared difference of h - handle[i].
8. Do next iteration

Everything above is done for each value of lag from 0 to (in my case) 15, where one unit of lag corresponds to one frame time, which with my VGA adaptor is 1/70 second. Don't re-initialize k and g after changing tau -- the values won't change much from one tau to the next, so this helps convergence to stay fast.

Results:

Best value of tau: 11/70 second, or 0.157 sec. (same as I reported before). This corresponds to what Powers, Clark, & McFarland found in reaction-time measurements with supposedly second-order control systems, ca. 1956, unpublished. Does that mean anything? Probably not. But it shows that OBF was right. Transport lag must be taken into account. There is no need for a perceptual integral lag.

RMS error between model and real handle positions: 2.53% of peak-to-peak handle excursion for a 29-second run at 70 data points per second (with a 2-second unrecorded lead-in time). That's the best I've ever seen -- even the fine wobbles are accounted for. This was done with a medium-difficulty disturbance, not an easy one.

The actual RMS errors for tau from 0 to 15 70ths of a second, in percent of peak-to-peak handle range, were 4.31, 3.95, 3.89, 3.61, 3.44, 3.30, 3.14, 3.02, 2.83, 2.72, 2.63, ((2.53)), 2.60, 2.82, 3.19, and 3.79. A nice smooth minimum.

Best integration factor g: 0.07134 per frame, or 4.99 inverse seconds.

Best leakage factor k: 0.00130 per frame, for a time constant of 0.09 sec.

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This is intended for CSG people who are already doing quantitative modeling and have been here before. If others request I will go into more detail -- ask questions. If you don't do modeling, don't think you have to learn. It's not everyone's cup of tea. We're all working on the same problems, doing what we happen to do best. I guess I'm saying that you don't HAVE to be interested in this to be a control theorist. This remark is on the clinicians vs modelers

thread.

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD   (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

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Date: Mon, 29 Oct 90 10:35:02 -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: CT and Co-evolution: Closing the Loops

Bill & Chung-Chih:

I have have been following with great interest your discussion about evolution, since this is a major interest of mine as well.

All I want to point out right now is that the notion of co-evolution that Chung-Chih has mentioned in which the evolution of a species influences the environment which influences evolution of the species, etc. seems to me to be the same type of closed loop that control theorists talk about. So in much the same way that control theory moves psychology away from the linear causation idea of environment influencing behavior, so does the notion of co-evolution appear to get away from the same type of one-way causation of classical Darwinism in which the environment selects the organisms (since in a very real since the environment is also the organism). The trouble I have with some writers on co-evolution is that I sometimes get the idea that they use co-evolution to come up with what appear to me to be quite Lamarckian notions about the environment somehow causing the organisms to produce the right types of variations.

In any case, I think it is of considerable interest that both control theorists and co-evolution theorists have independently (?) closed their loops to come up with what they feel are better models of the phenomena of interest.--Gary

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

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Date: Mon, 29 Oct 90 11:14:03 -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: Lags and conflicts

It seems to be the time of year for making observations that call into question some of our complacent assumptions about how human control systems operate. Bill Powers discovered that there apparently is a significant transport lag operating in our tracking experiments. I think I have discovered that controlling a control system is different than controlling an equivalent physical system. Both of these findings, if confirmed, will require changes to the control model.

(Incidentally, Bill says that the lag was suggeted by the OFB. I don't recall seeing a post regarding OFB's ideas about transport lag. Did I miss something? Are my posts getting out? I need some feedback).

Bill's time lag requires some change in the model of control but it certainly doesn't require changing the basic concept of the control system. I will be trying the new version of the model (when I get a chance) in my two-dimensional tracking experiments. In those experiments, I added a sudden, constant disturbance to one dimension of movement. The subjects seemed to start responding to this disturbance virtually immediately (within the 30 msec sampling period). A 157 msec transport delay was not at all evident in the data traces but maybe I'm not looking at it right. I'll see if the tranport lag model works better -- it should. Fortunately, the existence of such a delay causes no problem for my conclusions from that study -- which was that subjects respond to a one dimensional disturbance only in that dimension. The existence of a transport lag actually makes a lot of sense; after all, the perceptual information must be transported from the eyes to the muscles via neurons that carry electrical impulses at a pretty slow rate. So, finding the transport lag actually brings the control model a little closer to the physiological reality.

My findings about conflict are really weird. The study was done just for fun; the goal was simply to show that conflict with another control system is just like pushing against an inanimate disturbance. I set up a compensatory tracking task where the disturbance is generated by the responses of another control system. The subject is to keep the cursor aligned with a target. The control system is trying to keep the same cursor in positions that vary around the target. That is, the control system has a variable reference for the position of the cursor. This reference varies slowly and randomly with a mean value equal to the target position. From the control system's point of view, the handle movements made by the subject are disturbances to the cursor; the subject's handle movements tend to push the cursor away from where the control system wants it. So the control system generates its own outputs to counter these disturbances. These outputs, called m(t), are disturbances to cursor position from the point of view of the subject. The position of the cursor is determined by

$$c(t) = h(t) + m(t)$$

where c(t) and h(t) are the position of the cursor and subject's handle over time. The subject is able to generate values of h(t) which keep c(t) at the target value -- that is, the subject can control the cursor. Of course, by doing so the subject is preventing the control system from keeping the cursor where it wants. The subject is successful because the control system has fairly low gain-- it's not good at getting what it wants. But its gain is high enough to generate values of m(t) that must be resisted by the subject.

OK, if you follow to here, this is what I did next. I stored the values of m(t) that were generated during a 1000 cycle run (about 1 minute). Then I replayed them as the disturbance in the same tracking task. I measured the subject's performance in terms of average squared deviation of the cursor from the target. Obviously, the subject should do just the same (approx.) when m(t) is replayed as when it was generated "live" by the conflicting control system. But noooooo! The subject does WORSE with the replayed m(t) -- usually by a factor of 2 (depending on the gain of the conflicting control system and the speed of variation of its reference). This is incredible: There is absolutely no basis for expecting such a result from the control model. In fact, I ran a simple version of the control model (instead of the subject) against the conflict generated m(t) and the replay of m(t) and, of course, the RMS error for the model was exactly the same in both cases. After all, m(t) is just a waveform and the model should respond the same to the same waveform, and it does. (Incidently, these results, should they hold up, are not good news for s-r or connectionist or any other models that I can think of).

What is going on here??? Well, the result must have to do with the fact that, in the conflict case, m(t) is, to some extent, being "generated" by the subject. The amount of "push back" being generated by the control system at each instant depends, to some extent, on the amount of push being exerted by the subject. So the subject has "control" over the disturbance to some extent in a conflict situation. The same disturbance just "happens" when it is replayed (this might be the basis for the slogan "shit happens"). Right now there is nothing in the control model that can take account of this difference; the control model just controls and there is no basis for responding differently to a partially self generated as to a completely independent disturbance. If anyone has any idea about how the model might be changed to account for this difference then please let me know.

Of course, my results are preliminary; there may be some weird artifact. But I have checked this out many times (and tested it against the model) and it seems to be a real phenomenon. If it is real then it has some rather remarkable implications. To me, it suggests that it is easier to deal with other control systems, even when in conflict, than to deal with the independent effects of the inanimate world. That is, the effects on variables you are controlling can be delt with better if those effects are caused by living systems rather than non-living systems. Maybe this is one reason why people seem to be able to get along with other people as well as they do even when they don't really understand these people or treat them as little more than "objects".

I would appreciate it if someone out there would let me know if they understand what I am talking about. Thanks.

Rick M.

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Richard S. Marken   USMail: 10459 Holman Ave
The Aerospace Corporation   Los Angeles, CA 90024
Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)
===
Date: Mon, 29 Oct 90 11:58:00 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: POWERS_D@CUBLDR.COLORADO.EDU
Subject: Greetings

Greetings to CSG! My name is Denny Powers. I am a senior mechanical engineering undergrad at the University of Colorado at Boulder, and Bill Powers' son. I have little formal knowledge of control theory, except what I have absorbed from Dad in the last 33 years.

I am very interested in learning how to apply Bill's ideas to the design of control systems. I am taking a hellish course next semester (that I took an incomplete in last semester) in control systems. I hope that learning the "old" way to design control systems will not get in the way of learning the new ideas that are discussed on this network.

I am looking forward to observing and participating in the discussions on CSGnet.

Sincerely,
Denny Powers

home phone: (303) 530-2058
internet address: powers_d@cubldr.colorado.edu

home address: 4491 Wellington Rd.
Boulder, CO 80301

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Date: Mon, 29 Oct 90 15:44:34 -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: Robotics Ads

The following is an ad for a book and video tape on movement control and animation. I copied it from the "Robotics" newgroup that I get on my network. I am posting it for several reasons:

1) I think the book and tapes might be interesting to those who are on the CSGNet and don't have access to the robotics group.

2) I am wondering about the "morality" of this since the publisher is getting free access to a network that is paid for by the users.

3) If those who understand these things better than I deem that this is moral then, for heaven's sake, why don't we advertise the CSG Books and the programs through this medium -- to CSGNet member and to possibly interested newsgroups like the "RObotics" and "AI" groups.

I think this ad might be a bit immoral(unless they paid for it in some way) but, if I were rich enough, I'd buy it. Maybe CSG could buy stuff like this for a library; CSG members could have access to items in the library -- they would just have to pay for postage to get them for a couple weeks. What do you think Tom?

Here's the ad:

+++

Morgan Kaufmann Publishers announces a new title in its Series in Computer Graphics and Geometric Modeling, edited by Brian A. Barsky

MAKING THEM MOVE
Mechanics, Control and Animation of Articulated Figures
(Book and Video Package)

Edited by
Norman I. Badler (University of Pennsylvania)
Brian A. Barsky (University of California at Berkeley) and
David Zeltzer (Media Lab, MIT)

Current computer graphics hardware and software make it possible to synthesize near photo-realistic images, but the simulation of natural-looking motion of articulated figures remains a difficult and challenging task. Skillfully rendered animation of humans, animals and robots can delight and move us, but simulating their realistic motion holds great promise for many other applications as well, including ergonomic engineering design, clinical diagnosis of pathological movements, rehabilitation therapy, and biomechanics.

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VIDEOTAPE

This videotape contains selected animation sequences illustrating techniques discussed in the book. The total running time is approximately one hour. An appendix in the book entitled Video Notes includes commentary on some of the animations.

Selections:

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Norman Badler and others (Univ. of Pennsylvania): Strength-Guided Motion and Task Animation From Natural Language

Tom Calvert and others (Simon Fraser University): Compose and Goal-Directed Dynamic Animation of Human Walking

Rodney Brooks and others (MIT): Genghis: A Six-Legged Walking Robot

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Nadia Magnenat-Thalmann (University of Geneva) and David Thalmann (Swiss Federal Institute of Technology): Galaxy

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Email: morgan@unix.sri.com

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Richard S. Marken USMail: 10459 Holman Ave
The Aerospace Corporation Los Angeles, CA 90024
Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)
===
Date: Tue, 30 Oct 90 02:19:58 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Lag & Conflict

Bill and Rick,
Is this the electronic Institute?
As soon as I can shift gears from projects underway, I will try the revised model, with a transport lag. I agree with Rick

that the change will bring the model more nearly in line with what we know happens in the nervous system.

Rick, I'm not sure what you are finding in your experiments with the model trying to keep a cursor elsewhere than where a person tries to keep it. Have you tried cranking up the gain on the model? Or on both models, when you run them against each other? From my experiments in which a person runs along with a model, instead of another person, I find something different from what you describe, and it may be because my models run with high gain -- they control well.

Recently, we have been running a series of experiments that use exactly the same conditions described in my chapter in Wayne's book and in my ABS article, except that we allow the person using the left control handle to adopt any one of several possible goals, while the one using the right handle tries to do exactly what the people did in the two publications, which is simple pursuit tracking. In the "straight" conditions, the person on the left also does regular tracking: in one task, that person inadvertently interferes with the cursor controlled by the other person; in the other task, the two handles each interfere with the cursor controlled by the other person. No sweat -- everybody tracks like a champ.

One of the new options is for the person on the left to try to control the cursor controlled by the person on the right. If Left tries to keep the right cursor in the same position relative to the right target as does the person on right, all is fine -- with both people trying to make the same thing happen, the cursor sticks to the target like glaze to an apple.

If the left person sets a goal of seeing the handle of the other person move in a pattern chosen by Left, the results depend on the gain adopted by Left and on the speed and range of the pattern Left tries to impose on Right. (This is a tracking version of the old "rubber band" demo.) So long as Left doesn't press Right too hard, both people achieve their goals in the task -- Right successfully keeps the right cursor even with the right target, and Left sees Right move her or his hand more or less in the pattern Left wants to see.

But if Left sets a high gain, or adopts a violent path, the whole thing blows up, immediately: neither can succeed.

The other obvious condition is for Left to try to keep the right cursor at a different position relative to the right target than does Right. This condition sounds the most similar to the one you described. If Left tries very hard at all (i.e., sets a high gain), forget it -- instant chaos.

And exactly the same things happen, for each of the conditions I just described, when the model takes the place of Left. Only if I back way off on the gain for the model do I begin to see results as benign as those you describe

If you try running with higher gains, you might see less difference between the model and the people. If you try that, let me now the results.

Salud.

Tom Bourbon <TBourbon@SFAustin.BITNet>

===

Date: Tue, 30 Oct 90 08:43:52 -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: Conflict/efference copy

Tom

You are right --

> The other obvious condition is for Left to try to keep
>the right cursor at a different position relative to the right
>target than does Right. This condition sounds the most similar
>to the one you described. If Left tries very hard at all (i.e.,
>sets a high gain), forget it -- instant chaos.

The thing is, in my case Right is a LOW GAIN control system with a slowly varying reference. Right's outputs are the disturbance to Left's (the human subject's) efforts to keep the cursor in a fixed position. I called Right's outputs (over time) m(t) -- for model output at time t. Left can control the cursor because Right's gain is very low. If Right's gain is high and Right's ability to generate output values is unlimited then, indeed, Left cannot control the cursor; left quickly runs out of handle range to work with.

The thing that is weird is that if you generate m(t) and REPLAY it then Left controls more POORLY (measured by RMS deviation of cursor from target) than when m(t) was originally generated. The EXACT, SAME WAVEFORM, m(t), is the disturbance in a tracking task on two seperate occasions. The only difference is that, on the first occasion, m(t) was generated "in real time" by a control system (Right) and, on the second occasion, it is just replayed from a stored table. The RMS error on the second occasion, with the SAME DISTURBANCE, is always larger BY A FACTOR OF TWO!!! This is not a small statistical effect. It happens every time and the effect is large.

I think the person who should be happiest about this result is Wayne Hershberger. I have always tried to give Wayne a hard time about his belief in the importance of "efference copies". But it seems like these results cry out for some kind of knowledge about what you intend to do entering into the control process. In the first case, where the disturbance is generated by the control system (in concert with the subject) the subject can know, to some extent, how much disturbance there will be at any instance by knowing how much push he or she intends to exert at each instant.

Maybe the new version of the model, with the perceptual input being the difference between error and perception, will handle the kind of phenomenon I have discovered (again, if it turns out to be something other than a stupid artifact -- but I'm pretty sure it's real).

Thanks for your comments. Keep those cards and letters coming in.

Best regards

RSM

****

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

213 474-0313 (evening)

==

Date: Tue, 30 Oct 90 15:45:00 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: Tom, Bill, Rick, et al.

To Tom Bourbon: Say hi to Frank LaManita for me. Glad to hear he is working with you. I only wish more of my undergraduates were as fortunate.

To Bill Powers: Your note to Chung-Chih Chen about two different types of stability, minimum energy and minimum error, was a delightfully lucid lesson.

To Rick Marken: A couple of observations:

First, a caveat regarding the topic of Tom Bourbon's presidential address. When speaking about a quest for truth I try to avoid using such expressions as "closer to the truth" because such language presumes not merely an existence of THE TRUTH but also our KNOWLEDGE OF the truth, for how else can we assess whether we are getting "closer," or not? In other words, a correspondence theory of truth begs the epistemological question, presupposing that we already know what we claim to be trying to discover. Plato, who championed a correspondence theory of truth, realized that this implies that we (as intellects) must be vested with THE TRUTH in the form of innate ideas (forgotten at birth) , and that all true learning is but the remembering of these innate ideas triggered by those empirical observations which are sufficiently similar to the truth to remind us of what we've always known. Plato's nativistic rationalism is interesting to contemplate but it is decidedly anti-scientific, as he took pains to point out in his picturesque allegory of the cave.

The empiricistic quest for truth that we call modern science does not appear to use a correspondence theory of truth at all. Rather, the hallmarks of scientific truth are parsimony and replicability. That is, scientific explanations (i.e., truth) are simply parsimonious accounts of replicable phenomena. Moritz Schlick (the founder of the Vienna Circle of logical positivism--had he not died young, logical positivism would have been more "logical") observed that these parsimonious accounts are merely "descriptions in terms of LAWS"--or what you, Tom Bourbon, and Bill Powers would call descriptions in terms of a GENERATIVE model, or what Phil Runkle and Bill Powers have in mind when they refer to the research target of the "method of specimens." This scientific sort of truth is a relative, rather than absolute truth. Consequently, such truths are also multiple (e.g., Newtonian as well as quantum physics has its uses, as do laymans' notions of solid matter). The rank ordering of these realities (the layman's, Newton's, quantum theory's) in terms of their truth value is a riddle worthy of consideration, but it is self-defeating to phrase this issue in the words, "which is closer to the truth?" (This is the point Chuck Tucker is emphasizing.) Instead, we should ask, "Which is truer, or most true of all? I submit that the ranking criterion boils down to a question of parsimony. I would define parsimony as the ratio a/b, where a is the number of empirical particulars (phenomena) which can be accounted for by the theory/model, and b is the finite number, or set, of universals comprising the theory/model. For example, the great power or truth value of the table of chemical elements (a set of universals) derives not from the diminutive size of the elements but from the diminutive size of the table!

Item 2: Your finding that a subject performs better when disturbances are yoked to the subject's current rather than pre-recorded output suggests to me that he or she is utilizing what I have previously called endogenous (self-generated) disturbances, which depend upon the ability of the control system to anticipate exogenous disturbances. In the chapter that I wrote for your special edition of ABS, I noted that properly timed endogenous disturbances could aid control. They are what Pavlov called conditional reflexes, and what engineers have called feedforward. The point that I tried to make in my chapter, "Control theory and learning theory," is that such notions as these fit parsimoniously into error-driven control-system models, which are fundamentally feedBACK mechanisms. I believe you are probably correct in supposing that the conditional stimuli" for these endogenous disturbances in your example are corollary discharges (putative efference copies) of the control loop's own error-driven output. Rick, why not play around with the way in which the exogenous disturbance is related to the tracker's output in the first place? What if you interpolate a constant or variable delay?

To CSG : I think it may be appropriate sometimes to refer to each other with full names as I have done here, so that third parties, particularly those who are new to the network or those who read the network mail infrequently, are immediately able to recognize who the conversants are. Also, a conversation among a group of close acquaintances who address each other only on a first name basis may appear to some newcomers as a private exchange upon which they might quite naturally be reluctant to intrude. I would like to minimize such inhibitions.

Warm regards, Wayne

Wayne A. Hershberger Work: (815) 753-7097
Department of Psychology Home: (815) 758-3747
Northern Illinois University
DeKalb IL 60115 Bitnet: tj0wah1@niu

==

Date: Tue, 30 Oct 90 15:46:37 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Efference and D

Rick,

Is your model still running with no transport lag, as are mine? I am wondering if what you see is a result of the immediacy with which one's self-generated disturbance (by way of the model resisting the person's disturbance on it). Although I have not played back any records of earlier runs as disturbances for later runs, often in my two person tasks, when I reach the point where a model takes over from one person, I am struck by the different "feel" when you run against the model. With no delay, the effects of your own actions come back at you in the next screen update -- in 1/30 second, on my systems.

I plan to do two things: one is to play back some old records; the other, introduce some delay. I'll let you know the results -- not that I doubt your accounts, I just want to see for myself.
More later -- I want to get this off before you California folks split!

Tom Bourbon <TBourbon@SFAustin.BITNet>
==
Date: Tue, 30 Oct 90 16:19:41 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Rick's Conflict Discovery

Rick is right. I have replicated the effect. The RMS error when a human is opposed by a low-gain (loop gain = 2) control system is HALF what it is when the recorded output of that control system is used as a disturbance. By playing with the gain and slowing factor in the control system I have so far been able to get a ratio of three to one in RMS error.

In doing the task, I found it distinctly easier to control the cursor when the "live" control system was trying to vary it. With the recorded disturbance (exactly the same -- I checked -- as when the control system was alive) at work, the cursor movements seemed much faster. With the live control system active, the variations were smooth and easy to control. This is fascinating.

I don't have an answer yet -- only a suspicion. The smoothness of the cursor movements with the live control system present suggests that the dynamics of the systems are different when two control systems are coupled. The handle movements of the person, in either case, oppose the output of the control system accurately -- you still get that Rohrschach shape. The wobbles are simply a lot greater when the recorded disturbance is used. So this looks like a change in the damping due to feedback from the human-induced cursor movements, through the other control system, and back to the cursor movements. But this is all a bunch of words. Simulations, or even (gasp) solving the differential equations (if possible) may show the answer.

I can think of a variant that might help us figure this out. Give the other control system a zero reference signal, and apply a disturbance to the jointly-controlled cursor. I think this is equivalent. Want to try that, Rick? Record ALL the variables so we can see their time-relationships. I'll try it too, tomorrow. Tom B., Wayne H., are you getting in on this? Why isn't Ray Pavloski on this net? If Greg Williams had his phone-line we could really bear down.

Important finding, Rick. There may be some practical applications of your discovery, too. Nice work.

Denny -- your message took an hour to get to me.

Chuck Tucker -- I've enjoyed the Cariani-Wolpert argument on Cybnet. Peter should be on out net -- I sent him a message saying so. Also a friend of his named Cliff Joslyn. Sometimes cyberneticists have their heads screwed on almost straight, although it's still a wonder to me how they all manage to dance on the head of that pin.

Herewith the TP 5.5 program I used: you can adapt it, no doubt.....

```
program conflict;
uses dos,crt,graph,mycrt,graftrix,AdContrl,disturb,stats;
```

```
var hp,ep,rp,kp,hm,em,rm,km,c,crms,crms1,crms2: real;
    i,j: integer;
    ch: char;
    cursor,savehm,handle,dist: dataarraytype;

{multiply m1 * m1, 32 bit product; divide by d to give integer result }

{ this is an assembly-language inline macro }

function dmd(m1,m2,d: integer): integer;
inline(
   $59/$5b/$58/      { pop cx, pop ax, pop bx }
   $f7/$eb/          { imul bx }
   $f7/$f9);         { idiv cx }  { result left in ax }

begin
 if not SetAtoD then halt;    { selects which handle device to use }
 getdistdiff(1,1,@dist);   { disturbance 1, difficulty 1 }
 for i := 0 to ndata - 1 do dist[i] := dist[i] div 3;
 selectgraph(0,0);         { detects graphics and sets up size variables }
 setgraphmode(graphmode);
 hp := 0.0; hm := 0.0; c := 0.0;
 km := 1.0; rp := 0.0;
 plotbar(vcenter,7);       { puts one of 16 bars on the screen }
 plotbar(vcenter,9);
  crms := 0.0;
  c := 0.0;
 mousex^ := 0; mousey^ := 0;    { defined in Graphtrix -- I used the mouse }

{ start paying attention here }

 for j := -200 to ndata - 1 do
  begin
   i := abs(j);
   rm := dist[i];
   hm := hm + (km * (rm - c) - hm)/(1+km);
   savehm[i] := round(hm);     { save the model's output in an array }
   crms := crms + c*c;
   handle[i] := adread(1);     { this all-purpose routine uses -- here-- mouse}
   adstart(1);                 { dummy -- not used by mouse }
   hp := handle[i];
   c := savehm[i] + hp;
   plotbar(vcenter - dmd(round(c),vcenter,2048),8);
   retrace;
  end;
 crms1 := sqrt(crms/ndata);
 clearviewport;
 for i := 0 to ndata - 1 do
  begin
   plot(i div 4, vcenter - dmd(handle[i],vcenter,2048),lightgray);
   plot(i div 4, vcenter - dmd(savehm[i],vcenter,2048),white);
   plot(i div 4, vcenter - dmd(handle[i] + savehm[i],vcenter,2048),white);
  end;
  ch := readkey;
  clearviewport;
  plotbar(vcenter,7);
  plotbar(vcenter,9);
```

```
  crms := 0.0;
  c := 0.0;
  mousex^ := 0; mousey^ := 0;
  for j := -200 to ndata - 1 do
   begin

    i := abs(j);
    hm := savehm[i];
    crms := crms + c*c;
    handle[i] := adread(1);
    adstart(1);
    hp := handle[i];
    c := savehm[i] + hp;
    plotbar(vcenter - dmd(round(c),vcenter,2048),8);
    retrace;
   end;
  clearviewport;
  for i := 0 to ndata - 1 do
   begin
    plot(i div 4, vcenter - dmd(handle[i],vcenter,2048),lightgray);
    plot(i div 4, vcenter - dmd(savehm[i],vcenter,2048),white);
    plot(i div 4, vcenter - dmd(handle[i] + savehm[i],vcenter,2048),white);
   end;
 ch := readkey;
 crms2 := sqrt(crms/ndata);
 restorecrtmode;
 closegraph;
 clrscr;
 writeln('first rms = ',crms1:8:1);
 writeln('second rms = ',crms2:8:1);
 ch := readkey;
end.
```

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD   (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

===
Date: Tue, 30 Oct 90 09:04:32 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%arti6@VUB.VUB.AC.BE>

My machine had been down for several days. I am very glad to read the emails from CSG-L again.

Bill P.: Chung-Chih is first name, but 'Chen' is easier to pronounce, so people call me 'Chen'. I just read that Denny is your son. I would like to know your relationship with William T. Powers.

Rick M.: Can you tell me the detailed paper or book of Bill's model of mind using control theory. I am very interested.

Chung-Chih Chen
Artificial Intelligence Laboratory
(Building K, 4th Floor)
Free University of Brussels
Pleinlaan 2
1050 Brussels, BELGIUM
(email: chen@arti.vub.ac.be)

==
Date: Tue, 30 Oct 90 21:07:12 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Parsimony

Note on Wayne Hershberger's comments about parsimony...

There's another way to think about parsimony (which by itself is just an aesthetic judgement). The way I think of it is to ask "How much effort does it take to make this theory be true?" When you're far off the track, it takes a LOT of effort. That's because the theory implies a model and the model keeps behaving differently from what actually happens. To make a wrong model behave right, you have to keep adjusting things, making excuses, waving your arms, re-renormalizing, and so on. You have to browbeat people into believing your model because when they try to use it they feel the effort and start asking questions.

When I finally gave up my prejudice that said transport lags weren't important in tracking behavior, nearly all those ugly little bumps where the model didn't wiggle like the real person went away -- and the model got simpler. I got the feeling that the model had just settled into a closer fit with the way things actually work. That's not just an aesthetic judgement. It implies that there is a truth to be found -- or at least approximated, in some of its dimensions, so closely that it's hardly worth the bother to try getting any closer. Maybe we can't reach Truth itself, but we can sure extrapolate and see that we're not far from it -- once in a while.

If, as the new revision implies, the hierarchy works in terms of a hierarchy of models (I'm far from the first to suggest that), then parsimony takes on an interesting evolutionary meaning. The brain works best when its models need the least tinkering to keep them working. To me, that implies a continual approach to a fit of the models with what they're about, the zero-effort state. Truth is like a reference signal: you hardly ever see the actual reference state, but from the behavior of the system when there's an error, you can fill in the arrows that all point toward the state of zero error.

Good to see everyone getting their backs into Rick Marken's conundrum.

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

==
Date: Wed, 31 Oct 90 08:32:39 -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: Transport Lags, etc.

Wayne Hershberger:

I like the idea of using full names, at least the first time in a posting to let the newcomers know who the players are.

Rick Marken:

Yes, I think I know what you're talking about even though I'm far from being a quantitative modeler. Very interesting and exciting, your finding about conflicting control systems being easier to handle than conflicting inanimate objects.

Concerning the tranport logs, I don't recall posting anything from our Old Behaviorist Friend (OBF) about this, but he did mention them to me, i.e., reaction time must be taken into account. How Bill Powers knew about this without my mentioning it is just another mystery.

Bill Powers:

With all your excitement about the transport lag discovery, you now have to make sure we can understand that this does not imply S-R-S-R... chaining, as in the TOTE (Test-Operate-Test-Exit) model. I believe that I know the difference (at least I hope so) but it seems a good idea to make sure there is no misunderstanding.

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

==

Date: Wed, 31 Oct 90 10:08:34 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Partial Solution, Marken Effect

Got a partial answer to Rick Marken's puzzle, anyway:

Let:
c1 = cursor, live control; km = model gain; hm = model handle; rm = model ref
c2 = cursor, no control ; kp = pers. gain; hp = pers. handle; rp = pers. ref
Note: c,h,r are time-functions, but no dynamics here;

FIRST CASE: LIVE CONTROL SYSTEM

Basic equations:
(1) $hm = km*(rm - c1)$ (model output = model gain times model error signal)
(2) $hp = kp*(rp - c1)$ (ditto for person)
(3) $c1 = hm + hp$ (cursor set by sum of both outputs, hm and hp)

Substitute from (1) and (2) into (3);

$$(4)\quad c1 = \frac{km*rm + kp*rp}{1 + km + kp}$$

Substitute from (4) for c1 in (1)

$$hm = \frac{km*rm*(km + kp) - kp*rp}{1 + km + kp}, \text{ or}$$

$$(6)\quad hm = c1*(km + kp) - \frac{kp*rp}{1 + km + kp}$$

The value of hm is recorded and serves as a disturbance d in the second case:

SECOND CASE: STORED DISTURBANCE

(7) hp = kp*(rp - c2) (person's control system, output = gain * error)

(8) c2 = hp + d (d is disturbance from Case 1)

Substitute c2 from (8) into (7)

(9) c2 = kp*rp - kp*c2 + d, or

$$(10)\quad c2 = \frac{kp*rp + d}{1 + kp}$$

Substitute d from (6) into (10):

$$(11)\quad c2 = \frac{kp*rp + c1*(km + kp) - kp*rp/(1 + km + kp)}{1 + kp}, \text{ or}$$

$$(12)\quad c2 = \frac{kp*rp*(km+kp)/(1 + km + kp) + c1 * (km + kp)}{1 + kp}$$

Now is a great time to recognize that the subject's reference signal is zero (keep cursor aligned with target at zero position). With rp = 0 ---

$$(13)\quad c2 = \frac{c1*(km + kp)}{1 + kp}, \text{ or FINALLY,}$$

$$(14)\quad c2/c1 = \frac{km + kp}{1 + kp}$$

That was a lot of work for a result that really doesn't give the whole answer. At least it shows that c2 is not the same as c1. But with km = 2 (low loop gain, as in the case I tried), we don't get a ratio close to 2 unless kp is very small, like 0.1 or so. As we know that people have loop gains on the order of 30 upward in compensatory tracking, we clearly don't have the answer yet.

However. This model doesn't have any dynamics in it. Maybe somebody who's more used to solving differential equations or manipulating LaPlace transforms can give us the answer. The model to use is then

h = k* int(r - c), with c = hm + hp as before. You could also add a transport lag.

My simple-minded analysis, based about 80% on arm-waving, goes like this:

For a system that doesn't respond to error instantly, the effective gain rises with time, starting with zero. That is, if you fed square-wave disturbances into the system and measured the incremental output/error ratio (partial derivative) immediately after each transition, you would get an apparent gain of close to zero. The longer you delay the samples after the transitions, the larger the gain looks, until at some sampling delay a few tenths of a second after the transitions, the gain would look very high.

In that case, the AVERAGE apparent person-gain kp is lowest for a rapidlyfluctuating disturbance. Therefore I predict (I haven't tried this yet, honest) that the Marken Effect will get smaller and smaller as the cutoff frequency of disturbances gets lower and lower. What else? Oh yes, if you make the disturbance easier by making its amplitude smaller (leaving the frequency cutoff the same), there should be no change in the Marken Effect. Also, when the effect is being seen, you can make it larger by increasing km, the loop gain of the conflicting control system (up to the point where the control system starts to win the conflict).

We still don't have the elegant solution, but at least we have some guesses to test.

Somehow I DON'T think that this phenomenon is due to "efference copies" or complex computations about how much effort is being put out. Revision or no revision, that model would be a bit too complex for my taste -- at least until we're forced to adopt it.

Best regards to all --- Bill Powers

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD   (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

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Date: Wed, 31 Oct 90 09:56:45 -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: Powers' Model/Marken Effect/Truth

Chen:

Bill Powers' model of mind is described in two books
1) Behavior: The control of perception. It is published by
Aldine de Gruyter (1973). This book is a classic and,
in my judgment, the most important and brilliant work
on psychology/life sciences written in the last 2000+ years.
It is necessary for your library-- worth every penny and more.

2) Living control systems. It is published by CSG Press (1989)
Just ask for it on the net and I'm sure someone can send you
a copy -- it is about $17.00(US). It is a collection of some
of Bill's published papers. It is also a must for your library.

....

Bill: The work on the "Marken effect"(I like it, I like it) is
now proceeding at a pace that I cannot presently keep up with. I will
look over your explanation of it this evening (Halloween--yipes). I
skimmed it and I agree that it would be nice not to need "efference
copies" and stuff like that. I want it to be simple. But you're
the best modeler so I bet you get the explanation first; I'm happy
as long as it's called the Marken effect(of course I have an ego).
It is a very interesting phenomenon and I will keep working on it
when I get a chance but Aerospace work is getting a bit heavy again
and I also want to try the lag model in my 2-D tracking experiment and
I have to prepare for a talk in November. It all seems so frantic. But
keep me posted on what you find.

Bill and Wayne-on TRUTH

I was going to post a reply to Wayne that was much like what Bill said.
Actually, I was going to compare the search for truth to e. coli's search for its favorite piece of shit. We may not have a good idea what TRUTH is but we know when we are getting closer to it. Like e. coli, when we are not getting closer all we know how to do is "change" but that may not make things any better; but if it doesn't we just change again.
I like Bill's description of it in terms of the effort involved in getting our models to "fit" our experience. The less the effort, the closer we are to the TRUTH. I think this idea of a Truth that we can only "feel" in terms of our efforts to understand our experience is very similar to what Pirsig was talking about in Zen and the Art of Motorcycle Maintenance when he was ruminating about Quality and how he had this sense that he could tell when it was there and when it wasn't; is Quality (TRUTH) "out there" or is it "in here"? Pirsig asks. I think the control theory answer is "both"; TRUTH (Quality) is approached when what is "in here" (the reference) matches something else "in here" (my experience), the latter being, presumably a result of what is "out there".

By the way, Chen, Pirsig's book is gook supplementary reading for control theorists.

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: Wed, 31 Oct 90 12:12:10 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>

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Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

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Date: Wed, 31 Oct 90 13:00:14 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>

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(BITNET) FREE0536@UIUCVMD (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

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Date: Wed, 31 Oct 90 13:14:56 CST

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>

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Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

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Date: Wed, 31 Oct 90 14:49:54 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: S-R-S/TRUTH/INTERACTIONS

Gary Cziko: I don't think there is a need to worry about the S-R-S-... problem. A transport lag does not alter the continuous circular actions of the model. Everything works the same way, but with a little lag between the time when a "stimulus" first reaches the sensor and when the output of the system begins to change. From then on, it is a continuous system, not a a discrete one.
Wayne Hershberger,Rick Marken,Bill Powers: Concerning "truth" in science, I seem to fall somewhere between the positions you guys articulated, but a bit closer to Wayne. If we assume we are coming closer to TRUTH, we adopt the Platonic concept of Forms, and I cannot imagine how we might check the veracity of our _belief_ that we are closer to them. At the samee time, I agree that we judge the truthfulness of our understanding in terms of our "conviction," or belief that it is good and true -- but at that point, we do not differ from one whose personal belief and the sense of satisfaction that accompanies it is the arbiter of its truthfullness. That we attempt to bolster our conviction by establishing empirical tests certainly can lead to differences between science and theology, but not necessarily so -- consider the empirical confirmation of faith that occurs when one exorcises the demon one believes is responsible for what someone else might call a transitory seizure, as in epilepsy.
   Various: The interaction effect replicates here, as well -- three out of three ("proof of its truthfulness?"). Like Bill Powers, I am reluctant to appeal to "efference copies" as an explanation. (Wayne Hershberger, didn't you conclude, in your analysis of efference-copy theory, that it is AFFERENCE COPY, not efference copy, that is necessary? Or did I miss the point of your thorough discussion?) In my two-person, or two-hand, or one-model-one-person tasks, I frequently observe that when both controllers, be they the two hands of one person, one hand on each of two people, or a model and a person, try to keep the same cursor in the same place, the task is easier for them than when they perform alone. (I have not checked to see if they do better when interacting with a played-back record than with another control system, but I can't imagine how to do that, because I need both systems to try to do the same thing.) When the two try to keep the same cursor inn two different places, the new (Marken) effect occurs.
   In my tasks, I have interpreted the results (off the top of my head, with no formal analysis) as resulting from the fact that an immediate inverse consequence of one's actions comes back

in the form of an influence on the controlled variable (cursor position). The result is that one's movements to control the cursor are "damped" by the need to counteract the inverse consequences of those very actions. (Participants often describe a feeling of "heaviness" in the control handle -- what I believe they are sensing is the result of "trying to move both ways at once.") When you run against an inanimate disturbance, there is no immediate "feeding back" of your own actions as part of the disturbance you must oppose.

All of this is off the top -- Bill Powers seems to have made a good start at analyzing things more rigorously.

Tom Bourbon <TBourbon@SFAustin.BITNet>

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Date: Wed, 31 Oct 90 21:10:48 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: Fundamental Unit

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

Sciences require fundamental units of analysis. As yet, psychological (behavioral) science has no agreed upon unit. It has had the reflex arc, S-R, S-O-R, Skinner's three-term contingency (Discriminative stim: Response-Reinforcer), and whatever cognitive psychology offers (I assume Input->Processing->Output). I suggest that CST has the potential to supply a new fundamental unit, one suitable to the full complexities of psychological behavior. In this way, control system theorists and researchers are not merely working on some esoteric "theoretical perspective" that is offered as an alternative to current "theories and systems." Rather, CST is moving to the behavioral equivalent of the atom.

For the first time, we have in front of us a unit that is not static--it behaves, and this behavior is quantifiable. I think here of Shimp's call for "models that behave" (JEAB, 1989). Furthermore, just as there is not an atom, there is not one behavioral control system. This already seems to be becoming clear.

The above proposal seems to go back to Dewey's reflex circuit. Dewey didn't offer a new theory. He took an initial step toward a fundamental behavioral unit to replace the reflex arc (the basic "model" behind all subsequent attempts to define a unit). Dewey's paper is much cited (most cited in Psychol. Rev. up to 1943). Yet what impact did this paper actually have? Not a whole lot. Why? Because like Kantor's later R<--->S in-a-field unit, it was still too abstract. Researchers couldn't do anything with it. Dewey and Kantor were ahead of their times but not so far ahead that they could get around to making their models behave.

I recall somewhere where Bill Powers said that a day will come when control system theorists will no longer be necessary. This will be because control theory is simply ingrained in the discipline. Perhaps this will come about when the control system is taken to be the "underlying" unit of all behavioral events.

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