CSG_9011

Date: Tue, 6 Nov 90 13:31:38 -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: Method

To Warren Thorngate:
From Rick Marken:

It was nice to hear your comments about control theory research. I'm sure you will get a number of responses. What I say may overlap with others but I think the issues you raise are worth redundant attention.

About your first point re: "tracking tasks" and the John Maynard Smith apartment shower analogue.

It turns out that Smith's apartment situation can be handled by control theory quite nicely -- I have done it and virtually the exact analogue of Smith's situation has been modeled by Tom Bourbon. I don't see Smith's example as an indication of the limitation of the tracking task in control research; indeed, Smith's example is a tracking task. Each resident is using a handle (or handles) to adjust a variable (water temp) to a reference condition. There are many disturbances but Smith is most interested in the disturbance created by the effects of one person's water use on the temp of the others' water. But this is no problem; My spreadsheet model shows how several variables can be controlled relative to different references simultaneously -- even when the outputs of one or more system affect the inputs to one or more other system. Tom Bourbon's studies of social integration (reported in Wayne Hershberger's book and my American Behavioral Science issue) show how control theory handles Smith's "problem" just like people do -- there can be momentary inconvenience but people (and the model) adapt in order to get their input variables to acceptable reference levels. The modeling done on this topic has not yet included lags (in the model or the environment) but, given Bill's work on the lag model it looks like the addition of this level of fidelity will be easy and will materially improve the fit of model to data (which is pretty impressive since Tom's model already accounts for 99.9% of the variance).

So control theory is already well on the way to handling "social dynamics". You might also like to look into the work of Clark McPhail and Chuck Tucker on modeling social imitation and interaction in large groups. They don't even use tracking experiments -- they just watch people out there in the "real world". And the people out there are doing just what they do in the lab -- controlling perceptions relative to internally specified references for those perceptions.

I think control theorists (of the Powersian persuation anyway) are doing some non-tracking type studies. It is true that most of our control theory studies look a lot like variations of compensatory tracking -- but those variations can be quite magical and important. One goal of control research is to try to discovered the types of variables that people can control. A lot of those variables can be displayed on a computer console -- but many can't and so we must get into the field. There have been "non-tracking" studies of self-concept, for example, that are quite good. Some indication of the range of research done by control theorists can be gotten by looking at some articles in the Sept/Oct issue of American Behavioral Scientist (see, in particular the Plooij article) and at Phil Runkel's

new book "Casting nets and testing specimens" from Praegar.

I used to worry about the fact that most of my research is based on "tracking type" studies. But just a few months ago I realized "what's the problem?". After all, a fellow named Skinner came to rather cosmic conclusions about human nature based on ONE type of research paradigm -- the operant conditioning experiment -- which is really just a type of tracking task where a variable (eg. food rate, picture focus, click occurance/non-occurrance, whatever) is maintained in a fixed or variable reference state by doing something with the hand, paw, or beak. Freud came to similarly cosmic conclusions just by asking people questions. And cognitive psychology was built largely on reaction time studies. So if a narrow research paradigm is a shortcoming, it is a shortcoming that control theory shares with other theories that have been taken very seriously.

Thanks for the stimulating comments.

Hasta Luego

Rick M.

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Richard S. Marken USMail: 10459 Holman Ave
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213 336-6214 (day)
213 474-0313 (evening)

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Date: Tue, 6 Nov 90 20:17:43 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Mailing list for Cybernetics and Systems

Greetings from Binghamton. I'm a student of Systems Theory here, and run a similar mailing list on general Cybernetics and Systems Theory. Blurb below, please join!

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

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ANNOUNCING FORMATION OF A MAILING LIST FOR SYSTEMS AND CYBERNETICS

An electronic mailing list dedicated to Systems Science and Cybernetics is currently in operation on the SUNY-Binghamton computer system. The list is commited to discussing a general understanding of the evolution of complex, multi-level systems like organisms, minds, and societies as informational entities containing possibly circular processes. Specific subjects include Complex Systems Theory, Self-Organizing Systems Theory, Dynamic Systems Theory, Artificial Intelligence, Network Theory, Semiotics, fractal geometry, Fuzzy Set Theory, Recursive Theory, computer simulation, Information Theory, and more.

The purposes of the list include: 1) facilitating discussion among those

working in or just interested in the general fields of Systems and Cybernetics; 2) providing a means of communicating to the general research community about the work that Systems Scientists and Cyberneticians do; 3) housing a repository of electronic files for general distribution concerning Systems and Cybernetics; and 4) providing a central, public directory of working Systems Scientists and Cyberneticians. The mailing list can store or transmit notes and messages, technical papers, references, calls for papers, computer programs, and pictures and diagrams.

The list is coordinated by members of the Systems Science department of the Watson School at SUNY-Binghamton, and is affiliated with the International Society for the Systems Sciences (ISSS) and the American Society for Cybernetics (ASC). The list is open to everyone, and we currently have over three hundred members from America, Canada, and Europe. Our
subscribers are from both academia and industry, and while many are active researchers, others are just "listening in". We share in an exciting, ongoing, multi-way conversation about many aspects of Systems and Cybernetics. Different levels and kinds of knowledge and experience are represented.

We invite all to join the discussion. To subscribe, you need a computer account with access to one of the international networks (e.g. BITNET, USENET, ARPANET, INTERNET, CSNET). Send a file containing only the line: 'SUB CYBSYS-L Your Full Name' to the list server at the address LISTSERV@BINGVMB.BITNET.

Once subscribed, please post a message to the list itself at the address CYBSYS-L@BINGVMB.BITNET. In the message, include your name, affiliation, and a brief description of your work and/or interest in the fields of Systems and Cybernetics.

List moderator: CYBSYS@BINGVAXU.CC.BINGHAMTON.EDU

Author: Cliff Joslyn, CJOSLYN@BINGVAXU.CC.BINGHAMTON.EDU

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Date: Tue, 6 Nov 90 14:40:10 +0100
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti9@VUB.VUB.AC.BE>
Subject: hierarchy

Bill Powers:

I just borrowed your book "Behavior: The control of perception." I am trying to understand your hierarchy of control. I think the idea of using hierarchy to control a system has a very long history.
But recently maybe this idea is more explored. For example, in robotics the subsumption architecture developed in MIT has been used successfully to control mobile robots. In this architecture higher layers can subsume the functions of lower layers. So it's very robust. See the following paper:

%A R. A. Brooks
%T A Robust Layered Control System for a Mobile Robot
%J IEEE Journal of Robotics and Automation
%V RA-2
%N 1
%P 14-23
%D March 1986

On the theoretical side, you can try the following book (from the Brussels school of Prigogine):

%A J. S. Nicolis
%T Dynamics of Hierarchical Systems
%I Springer
%C Berlin
%D 1986

I have read many critics on neural nets in CSG-L, for example, "neural nets are just S-R models," ... etc. Neural networks have been successfully applied to pattern recognition. Can the control theory explain pattern recognition in our brain?

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Date: Wed, 7 Nov 90 08:46:20 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: Hello Cliff, Beyond tracking, Hierarchy

Welcome, Cliff Joslyn! You'll find CSGnet less organized than your CYBsys network, but perhaps some subjects will be found here that aren't redundant. By the way, have you noticed that there seem to be a lot of nibbles out of that biscuit lately?

Warren Thorngate, good to hear from you at last. It's hard to avoid the "we have it and they don't" talk, because the biggest problem with getting control theory accepted is the conviction in the mainstream that THEY have it and WE don't (a conviction spoken loudly, often, and with no shrinking from hubris). If all we had to do was develop control theory for psychology and explain it, life would have been easier (starting about 40 years ago). We've had to study the other side's views and pick them apart, first to be sure we aren't missing something, and second to learn how to meet arguments against control theory, which still abound.

To add to Rick Marken's comments: I agree with Warren that we have to go beyond tracking experiments. All the interesting stuff goes on at the higher levels of organization. But when you come down to it, no matter what variable you're controlling, the behavior that controls it is always very simple: push, pull, twist, or squeeze. Behavior isn't very interesting; what's interesting are the EFFECTS of behavior, which range from the position of a limb to a position in the stock market. To understand how these effects are controlled, we have to understand how they are perceived, and how a hierarchy of control can be built that bridges the gap between ppts and these complex perceptual variables. It's tempting to jump right to the higher levels, but I think that as we learn more about the nature of intermediate levels in the hierarchy, the problems at higher levels will change their appearance and turn into different problems.

Chen: Yes, hierarchy is an old idea. But as you get familiar with the model in my book, I think you'll see that the control hierarchy I propose is quite different from hierarchies that others have proposed. Most hierarchies are based on some organizing principle like size: molecule, organelle, cell, organ, organism, community, society, life... .One very common notion is that the ELEMENTS of the hierarchy, the things being organized, remain a collection of elementary variables, while some observer classifies and reclassifies them into sets of larger and larger size. You draw a lot of little circles around sets of the variables, then larger

circles around the little circles, and so on.

In my hierarchy, higher systems are physically distinct from lower ones and do not work with the same input variables. Instead, the input functions of higher systems receive copies of the signals in the input functions of lower systems, and apply some typical transformation to them to create new signals that represent a different TYPE of variable (so that a collection of configuration signals is re-perceived in terms of derivatives, or a collection of objects/transitions/events is re-perceived as the state of a relationship). Furthermore, every level in my hierarchy is also a level of control: there are comparators and output functions at every level. The output functions act not on the external world, but by adjusting the reference signals for systems of the next lower level. The SAME systems from which copies of input signals come.

One last difference from other hierarchical models (like Brooks'). I've tried to use neurological information as much as possible, and to define

levels that seem possible to find in ordinary human experience. Many other hierarchical approaches are more like ad-hoc inventions, organizations put together to achieve some immediate purpose without the constraint of achieving it the way a living system does. I'm not basically interested in robotics, although it can be fun. I'm interested in how human beings and other organisms work. For me, the constraint is always to figure out how the real system achieves a given behavior, not just to find ANY way of achieving it.

I don't deny that we can learn useful principles from making ad-hoc models, principles that will help us understand the real system. So I'm not saying that people who do it a different way are wasting their time. I just have to stick with what I know how to do.

Best --- Bill

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Date: Wed, 7 Nov 90 10:19:52 +0100
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti9@VUB.VUB.AC.BE>
Subject: intelligence and evolution

Bill Powers:

Very clear analysis about the question of evolution and intelligence! Although you didn't give the definitive answer, it seems that you agree that evolution is intelligent (in biological systems). But my question goes deeper, is the evolution of the physical systems intelligent? It seems that the control processes you talked about can be applied to physical systems. For example, in a star system, planets moves around the star, they maintain their orbits resisting the small disturbances. In a certain sense, we can even say that the physical force is purposive, directed. Under such force, an object has to go from here to there.

This is my idea that there is intelligence in physics. Is this a funny idea?

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Date: Wed, 7 Nov 90 22:17:54 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: Y role in CSG

BILL POWERS' ROLE IN CSG RESEARCH: PRESENT AND FUTURE.
At the present moment I have these projects going on:

1. A model of tracking behavior involving the stretch and tendon reflexes, with visual tracking of a target (the Little Man). I'm presently merging several programs so that parameters of all control systems can be set while the Little Man is active on the screen, for optimization. This model will also have nonlinear muscle-spring and force-velocity relationships in it, taken from the best literature (Stark et. al.). It will be possible to set up starting and ending positions of the target and to leave traces of the fingertip trajectory, for comparison with data on rapid arm-movement from the literature.This work is being done with Greg Williams, who is taking the results of the Little Man model and putting them into the Desire simulation language for more precise calculations.

2. A program for the measurement of visual motion illusions, for Pat Alfano's doctoral research. Pat works with Dick Robertson and teaches at Northeastern Illinois University. The program is under test now, and will probably require refinements before being used for actual research. The principle is "control of perception:" after a moving image is seen by a subject for an adjustable period of time, the motion ceases and the subject uses a control handle to keep the movement apparently at zero. The amount of "correction" of velocity shows the magnitude of the illusion as it decays. It will probably be necessary to include a training task in the program, because subjects must be very good at correcting tiny changes in velocity in order to produce data that can be fitted to an exponential decay with reasonable uncertainties.

3. A program for presenting multiple tasks to a subject using more than one control handle, for a doctoral candidate's research under David McCord (Western Carolina University). Subjects will be trained to do concurrent control of different variables (in a way similar to the tasks devised by Ray Pavsloki of Indiana University of Pennsylvania). After the dual tasks are learned, the characteristics of one task will be altered, by reversals or by adding dynamic effects such as simulated mass or transport delays between handle and controlled variable. This presumably will attract attention to one task. The proposal is to measure parameters of both control systems, before and during the resolution of the problem. The basic research problem involves two questions: can the effects of reorganization be measured while it is going on, and what is the effect of diverting conscious attention from one control task to another?

4. The "Crowd" program, for Clark McPhail (U. of Illinois at Champaign-Urbana) and Chuck Tucker (University of South Carolina). They have already presented version 2 to the annual meeting of the American Sociological Society (if that's what it's called). Now the project is to add many new adjustable features to the model, including the rudiments of an ability to negotiate goals within sub-groups of the crowd. The model allows each individual to move toward a destination or another person, avoiding collisions. In its present state it illustrates a number of observed crowd-movement phenomena, emergent from some simple control-system properties of individuals and without

any overriding "mass" laws of action. The goal of this research is to make the models of individuals more comprehensive, to see what new crowd phenomena will emerge from this collection of up to 255 active individuals. One new feature of the program will be the ability to specify conditions and record measures of mass behavior in a way that will allow setting up field experiments with live subjects for compari son with the model's behavior.

5. Simulation of operant conditioning. CSGers who attended the meeting three or four years ago saw the first attempt at this project: a bird that pecked on a key to get rewards, with a live block diagram showing the presumed two-level control system at work (one level controlling perception of mean food intake, the next level adjusting the gain of the first to control cost-benefit ratio). This model has been fit to data from Staddon (collected from the literature) for animals tested over an unusually wide range of schedules. The ultimate aim is to pro- duce a model that exhibits all the major phenomena observed during the operant conditioning of animals in Skinner boxes: your basic simulated rat or bird. In gathering data about behavior for this experiment I have found out all kinds of strange and wonderful things about the way data are taken: for example, the rate of bar-pressing behavior is measured under conditions where the animal may spend significant periods of time away from the lever, doing something else (while the mean behavior rate declines like your average speed on a trip while you're stopped for lunch). William Timberlake and Gary Lucas, with whom I've corresponded a bit, may come up with better ways of taking data, so the model will have to be adjusted accordingly. I'm not doing this one for anybody but me.

There's more, but I think the basis of my next comments will be clear:

I guess I'm approaching overload. The problem is that I seem to be the only one among CSG modelers who is working out programs for non-programmers to use in their research. I don't mind thinking up ways to do things, and even coming up with a practical programming approach for doing them, but there's just so much detail work in making a program easily usable that I'm getting a little discouraged about the tasks I've taken on. I really need some help. There are others in the CSG who can do programming; I hope they will volunteer to help out with specific tasks in the future. Also, the people who want to use the modeling approach in their own research ought to be learning how to do it themselves, as much as possible. Those who just can't do it, of course, should feel free to ask for what they need. But there are computing courses available, and there's nothing about programming that is beyond an unprejudiced person. I'm still perfectly willing to help get things started and help troubleshoot, but friends and Romans, I'm only one person, and now that I've retired I'd like to retire once in a while and not feel so obligated. I know, it's my own fault for not knowing how to say "no," but I'm trying now to do something about that.

It would be lovely if we could use the net to discuss research problems people are having, and for the programmers to trade notes on how to approach these problems, and parcel them out to VARIOUS individuals for complete development. If there are newcomers on the net who have program- ming skills, this would be a great way to learn the nuts and bolts of the CSG approach to modeling behavior. I suggest that people who want programs

written (and/or research ideas to go with them) submit their requests to the Net, and that we programmers discuss the problems and come up with solutions -- and then wait for a hand to be raised, when someone breaks down and says "OK, I'll do the program." I suggest that we stick to Turbo Pascal or C -- I'm willing to switch to C if the consensus goes that way. Whichever way we go, we can at least send source code back and forth, so whoever takes on a task won't be completely alone in it. If we switch to

C there will be groans from several sources, but I have to admit that for all its ugliness, C is fast and it will transport in source form to many machines that don't have good Pascal compilers. I don't think we should plunge into OOP stuff; it's structured wrong for control hierarchies anyway (top-down, logic-tree instead of bottom-up perceptual tree).

So who's willing to help take out the garbage?

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
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Date: Wed, 7 Nov 90 23:03:59 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: Chen's question on intelligence

Old Chen --

It's easier for me to talk about control than about intelligence -- after all, psychologists define intelligence as the score on a Stanford-Binet intelligence test, which would be hard to administer to a planet.

Back in the 1940s, there was a debate between cyberneticists and a philosopher during which numerous examples of goal-seeking were brought up, among them a compass seeking North, and a pendulum seeking the straight-down position. Your example of planets seeking equilibrium after a perturbation (say, away from a LaGrangian point) is of the same nature. I took me a while to figure out why these are NOT examples of control.

To take the pendulum:

First, the condition of the pendulum is always determined by the energy inserted from external sources -- disturbances. The net deviation from the resting point is caused entirely by the perturbation, and the process of returning to equilibrium is powered by that same externally-injected energy (even if the system is dissipative). In short, everything than happens after the perturbation is powered by the cause of the perturbation and not by the pendulum itself.

Second, if you calculate the loop gain of a pendulum as a feedback system, it is always equal to or less than 1. The effect of a variation in position, reflected back to the next variation in position, is equal to the initial variation (or less than it if there is dissipation). On the other hand, the loop gain of a real control system is always greater than (negative) 1. Taking impedances and dissipation into account, the POWER gain is always greater than 1, and is usually, in good control systems, in the hundreds or thousands (or even millions). Thus the control system, in order to seek a goal-state, must draw on nonspecific energy sources -- that is, energy sources that are not related to the energy injected by an externally-originated perturbation. The control system is a (thermodynamically) open system.

To be a control system, a pendulum would have to swing from the shaft of a motor, and there would have to be a low-energy sensor detecting the position of the pendulum. This position as sensed would have to be compared with a reference-position, and the error would have to be AMPLIFIED to operate the motor. Then the pendulum could come to rest at ANY position, not just straight down, and it would resist perturbations far more actively than the passive pendulum alone would do. Of course the amplification that provides loop gain would have to draw on energy from a battery or other source of power that is independent of the perturbation.

I think you can see that the same consideration would apply to a planet. So if what we think of as intelligence is really just an advanced form of control, we won't find intelligence in any kind of natural system but a control system. I don't know of any examples of natural control systems that aren't alive.

Have I answered your question or avoided it?

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Date: Thu, 8 Nov 90 08:42:26 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Clarke G by way of Gary A. Cziko g-cziko@uiuc.edu" <clarg@ESSEX.AC.UK>
Subject: control theory list

I would like to be put on the mailing list for this group.
My interest stems from hearing Powers compared to Bateson whom I admire greatly.
Graham Clarke
University of Essex
Englang
clarg@uk.ac.essex

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Note from Gary Cziko:

What a shame that Greg Williams cannot get on the network. Who would be better able to compare Powers and Bateson?

I have put Graham Clarke on the network. But watch out for Internet addresses from the UK. For some reason they do it backwards. From North America (and perhaps from the rest of the world as well), this address must be stated as clarg@essex.ac.uk and not as clarg@uk.ac.essex as he gives above.

I do not know why the British do it backwards. Maybe it has something to do with driving on the wrong side of the road.

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Date: Thu, 8 Nov 90 14:41:02 -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: CSG Network News

Dear CSG-L Networkers:

Since there has recently been quite a few new people added to the Control Systems Group List (CSG-L) recently, I thought it would be appropriate to provide an update on the network.

CSG-L was established in September 1990 by Gary Cziko (the list "owner") at the University of Illinois at Urbana-Champaign in order to facilitate communication and discussion among individuals interested in applying the concepts of control theory to animal and human behavior. All individuals with access to Internet or Bitnet electronic mail who would like to take part in this dialogue are welcome to subscribe to CSG-L at no cost. (Note that in addition to the access to Internet and Bitnet provided by universities, research institutions and commercial establishments, access may also be obtained via publicly available commercial telecommunications services, such as CompuServe in the U.S.) As of November 8, 1990 we had 30 people on the network (see appended list).

Here are some tips on using CSG-L and the LISTSERV which "maintains" the list.

1. Messages to be sent to all subscribers to CSG-L should be addressed to CSG-L@VMD.CSO.UIUC.EDU (Internet) or CSG-L@UIUCVMD (Bitnet). Try this out, if you haven't already done so, by sending a personal introduction to the network.

2. Anyone may subscribe to CSG-L by sending a mail message to LISTSERV@VMD.CSO.UIUC.EDU (Internet) or LISTSERV@UIUCVMD (Bitnet). The message should be in the following form:

SUBSCRIBE CSG-L FULLNAME

For example, I to add myself to CSG-L, I would send a message to LISTSERV@VMD.CSO.UIUC.EDU with the following message:

SUBSCRIBE CZIKO Gary: U of Illinois at Urbana

Note that I do not need to add my email address since LISTSERV will take this from the return address of the message.

3. All mail sent to CSGnet is archived in log files. These files may be obtained by sending messages to LISTSERV@VMD.CSO.UIUC.EDU (Internet) or LISTSERV@UIUCVMD (Bitnet) of the following form:

GET CSG-L LOG9009 (for the September, 1990 log)
GET CSG-L LOG9010 (for the October, 1990 log)

From November 1990 on, log files are created for each week so that CSG-L LOG9011A is for the first seven days of November 1990, CSG-L LOG9011B is for the second seven days, etc. The September and October log files will also be broken into weekly files some time soon.

Browsing through these log files is an excellent way to catch up on what has been going on. Note, however, that there is a limit as how much information you can request from the LISTSERV. If you request the September 1990 log, you will likely have to wait about 12 hours before the LISTSERV will honor your request for the October log.

4. A list of all files available from CSG-L via LISTSERV can be obtained by sending to LISTSERV the message:

INDEX CSG-L

I plan to be adding in the near future a number of other files, including a comprehensive bibliograpy on control theory compiled by Greg Williams.

5. A list of all CSGnet individuals can be had by sending the LISTSERV the following command:

REVIEW CSG-L

This will generate a list similar to the one appended to this message.

6. Those on Bitnet-connected machines operating under CMS can use the TELL command instead of mail to send commands to LISTSERV. From the command line within CMS, simply precede all commands with TELL LISTSERV AT UIUCVMD (e.g., TELL LISTSERV AT UIUCVMD REVIEW CSG-L).

While access to CSGnet is free, I wish to encourage all those interested in control theory to become an active member of the Control Systems Group. Members receive our quarterly newsletter containing articles of interest as

well as information about our annual conference. There are also plans to make
available to members a regular edited volume of CSGnet discussions. Our next conference will be held over three days sometime during the first two weeks of
October 1991 near Eugene, Oregon.

To become a member of CSG, send a check for $25 made out to the Control Systems
Group to Mary Powers, 1138 Whitfield Rd., Northbrook, IL 60062 USA.

If you are a new participant on CSG-L you are requested to send the network a brief personal introduction.

We look forward to your participation in our discussions.

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CSG-L Subscribers as of 8 October 1990

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LO0745@ALBNYVMS	OETJEN-GERDES Lynne A., Albany, NY
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Date: Thu, 8 Nov 90 16:04:11 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Introduction

According to the informational post, I'm supposed to submit an introductory note.

I'm a doctoral candidate in the Systems Science department of SUNY-Binghamton, studying general information science under George Klir. I'm writing a dissertation on 1) empirically-based semantics for possibility distributions, fuzzy sets, and general evidence measures; 2)

information measures of possibilistic processes; and 3) implementation of C++ classes for modeling stochastic, fuzzy and possibilistic objects and processes.

I'm also a professional software engineer, and hope to parly (3) above into an R&D position in CAST (Computer-Aided Systems Theory) systems development. I also teach C here in Binghamton.

My overall intellectual interest is in Systems Science (General Systems Theory) and Cybernetics. I moderate the CYBSYS-L list, and write and study on the natural philosophy of evolving systems. I am an editor and founder of Principia Cybernetica, which is a project in the collaborative development of a complete cybernetic philosophy. The project will use email and hypertext markup languages to construct and publish a dynamically evolving network of philosophical work. Our ideas are similar to Powers in that we describe the fundamental processes of general evolution as the construction of loose hierarchical levels of control.

I had known of Powers' work for a while, but had the great pleasure of meeting him this summer at the Gordon Research Conference (ostensibly) Not About Cybernetics. We've also enjoyed a correspondence about the use of chaotic dynamics in William Freeman's neurological theories. Should we continue that here, Bill?

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

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Date: Thu, 8 Nov 90 14:44:52 -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: Powers/Marken Aiding, Etc

Latest on the "Powers/Marken effect":

A practical application of control theory. I discovered that the response to exactly the same disturbance can be quite different depending on whether the disturbance is or is not the result of action by another control system. Control is much better (by a factor of 2 or more in terms of RMS error) if the disturbance is generated by a control system. Bill Powers showed through modeling that this is expected of any control system operating with a transport lag. Bill suggested that this might have practical applications -- the facilitating effect of the "competing" control system could be added to the outputs of a person to make control better. Indeed, I found that this is the case -- in spades. I can get improvements in tracking of 200 to 500 % by adding a low gain control system into the loop in a compensatory tracking task. Thus, the Powers/Marken effect can be used to improve control.

This approach to improving control has some advantages over previous attempts to improve control. First, "Powers/Marken aiding" is based on a true representation of the state of the variables being controlled; other approaches--like quickening and predictive displays-- give a false representation of the state of the variables that the person is supposed to be controlling. This is related to the second point -- "Powers/Marken aiding" makes sense -- while other aiding schemes are best replaced by control systems that can do the job better because the reference condition is known. In "Powers/Marken aiding" the reference can be changed intelligently by the human controller -- a good reason to have a human in the loop-- and the aiding still works.

When I get a chance I will try to do more quantitative investigations of this aiding system. This may be my first chance to make a practical contribution to what is currently my professional affiliation (Human factors engineering) through the application of principles derived from my theoretical interest (control theory).

Other news: It looks like my paper on coordination ("Degrees of freedom in behavior") will be published in the journal Psychological Science. It has taken me nearly three years to get this thing published. It basically shows that motor program and dissipative structure approaches to coordination (the currently hot approaches to motor control) are unnecessary and probably wrong. So it's been tough to get a fair shake from reviewers. The editor of Psychological Science, W.K. Estes, was instumental in getting my paper into print. I would like to salute him for being a fair and honorable scientist.

Bill P. : I'd love to help you take out the garbage but I have too many other chores already (whine, whine). Actually, my work load might decrease substantially in the near future -- they just announced that there will be a 6% reduction in force at my company. I think I'm safe but who knows. Academia is looking better and better.

I think your problems with workload are another excellent indication of the need for a Control Systems Institute. Whatever became of that guy in Oregon who was going to be the fund-raiser. If I could make a regular living at it I would love to spend hours at the institute developing my projects and helping you program yours. Maybe we could even get some staff to help with the nuts and bolts stuff -- that's what you need, some staff.

Anyway, I have been trying to write a book for the last three years and that is what I think I should keep working on. But, if I really do get canned, maybe I will start devoting my efforts to the creation of an Institute of Living Control Systems.

Best Regards

Rick M.

****

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213 474-0313 (evening)

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Date: Thu, 8 Nov 90 13:22:33 +0100
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti9@VUB.VUB.AC.BE>
Subject: evolution of the universe

Bill Powers:

While calling me 'old Chen', don't misunderstand that I am very old (I was born in the year when Kennedy was elected as the President of US.). I think you are right that a star system is not intelligent. But I am talking about the evolution of the universe (in physics). According to the inflationary model of the universe, the universe may come from nothing. If the universe can evolve from nothing to the current complex structures, to provide an environment for the intelligent systems to survive, can we say the physical evolution is not intelligent? Anyway, the intelligent systems come from physical systems.

It's difficult for me to believe that intelligence emerges suddenly from the biological systems. I believe that intelligence is inherent in the universe.

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Date: Fri, 9 Nov 90 09:41:17 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: Evolution, Intelligence, Misc.

Gary Cziko --

No bet. It's a clever argument, but it doesn't apply to my model of evolution (my proposal for a model, that is -- I haven't programmed it yet and if anyone else tries it before I do I won't complain). Let's see if I can shape up what I mean a little more clearly.

The kind of system I'm proposing pays essentially no attention to the forms or functions produced by a mutation. What is monitored is an EFFECT of the change, an effect on a critical variable inside the system itself. For example, the variable might be the amount of energy consumed by repair enzymes, which would rise if a lot of mistakes are being found. That's just an illustration; I don't know what the critical variables might be at the genome-control-system level.

Furthermore, when there is such an error in a critical variable, the reorganizing effect does NOT directly correct it. It simply increases the rate of RANDOM mutation. If the error gets bigger yet, the next mutation comes even sooner. If the error gets smaller the next mutation is postponed. There's no guarantee that the error will ever get smaller.

The organism might be deluded about what reference-states of critical variables are important. In that case, old-fashioned natural selection will do the editing. The organisms that are left will be those controlling meaningful critical variables with respect to the right reference levels by the method of variable-rate mutation.

So we end up with evolutionary PROGRESS (toward better control) without evolutionary DIRECTION (toward any particular design).

Chung-Chich Chen -- I used "Old Chen" because I guessed that it might be more respectful than "Little Chen." Anyway, EVERYONE is younger than me.

The problem with saying that the universe is intelligent is that this doesn't mean any more than saying it is X. What is X? It is whatever is needed to make things come out the way they did. If you want to say that the physical universe is X, first you have to say what X is, so we can compare the properties of X with the properties of the universe and decide for ourselves that they are the same. The great difficulty is in finding a way to determine whether X is present OTHER THAN citing the effects we propose that it must have. Proving that something exists by pointing to its effects leaves a lot of room for alternative proposals.

Some time I want to talk with you about my definitions of levels of control, and see if they mean anything when translated into Chinese.

Rick Marken --

Yes, I had "quickening" in mind; it was tried back around 1947 and didn't work very well. It would be nice if your simple expedient did the job.

I hope you do get back into academia, so you can sit around all day with nothing to do but think about control theory, like all these other guys on the net.

Great about the paper's being accepted. Estes did reply to me and

indicated that he makes up his own mind -- referees just give him their "reactions." He said that his interaction with you was normal for papers that eventually get published. I wrote him back and said I was completely satisfied and that I would recommend that other control theorists submit papers to PS. I hereby do so. They will be fairly judged.

Cliff Joslyn --
Sounds like pretty heady stuff that you're into. A lot of us here are modelers and experimentalists. Are there any applications of your ideas

that could bear on control-system modeling? Or are we still working too far down toward the basement to link up with your ideas?

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
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Date: Fri, 9 Nov 90 10:09:40 +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: Intelligence, Evolution & Suckers

Gary Cziko:

Good example to show that something very improbable will happen if there were many, many occurrences (like The Blind Watchmaker by R. Dawkins). I think many cons and pros have been expressed in the following paper and its Commentary (although they are for biological evolution, many points can be applied to physical evolution too.).

%A J. Schull
%T Are Species Intelligent?
%J Behavioral and Brain Sciences
%V 13
%P 63-108
%I Cambridge University Press
%D 1990

Schull concluded that it's not a yes or no question. Although I can't prove it right now, my basic belief is that the essence of intelligence is inherent in the universe. People may think I am just a nuts. But who knows? In the future maybe you will see I am right!

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Date: Sun, 11 Nov 90 10:47:51 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti1@VUB.VUB.AC.BE>
Subject: why the universe is intelligent

Gary Cziko, Bill Powers:

Yesterday night suddenly I got the reason why the universe is intelligent:

(1) The definition of intelligence:

I don't want to give a precise definition because it's not important. I just say something is intelligent if it shows more and more intelligent behaviors (a circular definition?), such as the human being. I think everybody knows what I mean.

(2) How you achieve intelligence is not important:
Gary's example shows that you use some method similar to 'random walk without direction' to reach a target does not mean it's intelligent. I feel such implication is not justified for the universe. Because the universe can execute many, many random walks IN PARALLEL, it can reach some target with a large probability although each random walk has only very small probability to reach the target. For example, suppose the probability that a planet can produce life is 1/P, where P is a very large number. But suppose the universe can create NP planets, where N is a large number more than 1. Then we can see that the probability that there will be life in the universe is nearly 1, which means it must happen.

(3) Why the universe is intelligent:
Firstly, the universe shows more and more intelligent behaviors (from physical systems to biological systems).
Secondly, the universe uses 'MASSIVELY PARALLEL random walks' to achieve its purposes. This method of intelligence may be very different from how a human being achieves his intelligence.

I am very excited with my argument that the universe is intelligent. Any counter-arguments are very welcome.

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Date: Mon, 12 Nov 90 08:25:19 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: Chen and Intelligence

Chen --
Quick note before I go on a trip until the 15th ---

I don't think that massively parallel stupidity generates anything but massive stupidity. In defense of my argument, I point to current events in the Middle East. If that's not enough, I indicate the phenomenon known as Government. I rest my case.

No,I don't; that was a joke.

Massively parallel random walks explain intelligence, but only if the probability of success of each parallel path is large enough. Remember that if we say that life evolved from zero, there is only about half a billion years to a billion years (after the Earth cooled enough) to get from nothing to molecules organized into single cells. Now the units of parallel processing are planets, not molecules, and each planet has only (let's say) one or two billion years to get the job done before it turns into something like Venus or Mars.

It would be pleasant to back up the argument with something more efficient than just a random walk.

In one sense I agree with you: the POTENTIAL for intelligence is inherent in the physical universe. My claim is that the principle of negative feedback, which is inherent in the set of all possible closed-loop

relationships, makes the random walk immensely more efficient than it would be if no such principle could operate. I think it makes life not only probable, but inevitable when a planet spends enough time at a temperature of 0 plus or minus 30 C (given, of course, carbon chemistry, which I take to be universal). The probability of life is simply the probability that Earth-like physical conditions will appear on a given planet.
  I offer this definition of intelligence: the capacity to control. This capacity can grow over time without changing its essential character. It's hard to think of any other feature of human organization that would still mean anything in the form it would have to take in a self-replicating molecule.

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Date: Mon, 12 Nov 90 13:22:56 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: Address of Dave Goldstein

REALLY FROM Dennis <DELPRATO@UM.CC.UMICH.EDU>
Can someone supply with the address and phone no.(s)
of Dave Goldstein?
Dennis Delprato

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Date: Mon, 12 Nov 90 13:16:56 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: C538435@UMCVMB.BITNET

subscribe csg-l MISTRY SANJAY

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Date: Tue, 13 Nov 90 08:03:19 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <arti1!chen@VUB.UUCP>
Subject: Re: Chen and Intelligence

Bill Powers:

I have no intention to use only random walk to explain life. That would be stupid. The important thing is the principle of massive parallelism, which will, without any doubt, greatly increase the probability of success. Imagine how many parallel events can be executed in the scale of the universe or a planet.

I think it would be fair to say that both the positive and the negative feedbacks are necessary in the evolution of the universe. Positive feedback is used to create new organisms (as proposed by Prigogine). Negative feedback is used to maintain stable organisms (as proposed by you).

As for your definition of intelligence: the capacity to control, I agree. Without some control mechanism, it's difficult to imagine that the universe just evolved from nothing to now using only 4 basic forces. I like cosmology. But I don't like the current picture of the origin of the universe explained by physicists. There should be something more.

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Date: Tue, 13 Nov 90 08:32:33 -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: Positive Feedback & Evolution

Chung-Chich Chen, Bill Powers, Tom Bourbon, others:

The issue of the role of positive feedback in evolution is an interesting one.

My first real knowledge about evolution came from reading _The Blind Watchmaker_ by Richard Dawkins. Dawkins argues that the complexity of species can be at least partly explained by positive feedback in the form of "evolutionary arms races." As the cheetah gets faster so does the gazelle who then puts presssure on the cheetahs to get faster still, etc. Also the pressure on trees to grow taller to get more sunlight. As a species gets taller it puts pressure on others to get taller also, etc. There are also some quite amazing things that have happened concerning parasite-host evolution; and of course there is sexual selection (the peacock's tail).

Last August in Indiana I discussed this briefly with Tom Bourbon who maintained that this is not positive feedback. He didn't quite convince me. Instead, it seems to me that organisms must use negative feedback to maintain control, but that at another level (e.g., phylogenetically) there are changes taking place that seem driven at least partly by positive feedback. But then I think that perhaps even the cheetah-gazelle story can be seen as negative feedback. The cheetah needs to keep (over evolutionary time) a certain amount of gazelle meat in its stomach and does whatever it needs to do this, including evolving faster legs. This would see what looks like positive feedback as re-organization.

Since Bill Powers is away for a few days, I guess it's up to Tom Bourbon to straighten me (and Chung-Chih Chen) out about this.--Gary

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Date: Tue, 13 Nov 90 08:54:24 -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: Books, Powersian Control

Gary--

I'm glad to see that I'm missed. Actually, I think I just posted something on Thursday or Friday (I don't post on the weekend for electronic reasons -- I have no modem at home). It has been tough to post because 1) I am trying to prepare for a talk I am giving this weekend and 2) because everyone around here is waiting to see if they will have a job next Monday. I've never been in this position before -- where there was a cutback in the workforce -- but it's like being in "It came from outer space" or "Alien". You don't know who might be the next to "get it". Not good for worker morale.

I have not tried to get a list of good control theory publications. But I received a copy of Phil Runkel's book last week and it is EXCELLENT. I highy

recommend it. For those who want a copy it's:

Runkel, P.J.(1990) Casting nets and testing specimens. New York:Praeger.

Everyone should also have one (or more) copies of:

Powers, W. T. (1989) Living control systems. CSG Press, Route 1, Box 302, Gravel Switch, KY 40328

From the same publisher order a copy (or several) of:

Robertson, R. and Powers, W.T. (1990) Introduction to modern psychology.

Of course, I also recommend the Control Theory issue of the journal American Behavioral Scientist. That's Volume 34, No 1, Sept/Oct.
Order a copy from:
Sage Publications, P.O. Box 5084, Newbury Park, CA 91359-9924

I was (am) going to start a scintillating thread on how Powersian control theory differs from other uses of control theory in the behavioral sciences. I think this is an important topic because Powers is certainly not the first to have applied the tools of engineering control theory to psychology. For example, I am currently reading a book by Tom Sheridan called "Man-machine systems", MIT Press, 1974. This book is filled with models of human performance
based on control theory. Moreover, it looks real serious -- the mathematics would cross your eyes. So it seems a bit impudent for a fellow like me, who couldn't do a Laplace transform to save my life, to say that these guys have it all wrong -- or, at least wrong enough to miss the most important point of control theory. But, indeed, they do miss the point. I don't have time to get into this now but I would welcome any comments from people on the net with respect to this topic; what do you think of some of the literature on manual control, for example. I know that Tom Bourbon did note that some physiologists are applying control theory from a Powersian perspective. How about some examples. How about some discussion about the essential differences between Powersian and standard applications of control theory in the behavioral sciences. How do we readily distinguish our approach from theirs? For those who already know the answer, I refer you to Figure 9.1 on page 178 of Sheridan's book. That figure says it all; the triumph of S-R thinking over the minds of people who should know better.

I'll try to post again soon. Best wishes to everyone.

Regards

Rick M.

****

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Date: Tue, 13 Nov 90 23:08:00 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: David's address

Dennis Delprato:

David Goldstein's address is 801 Edgemoor Road, Cherry Hill, NJ 08034.

Regards, Wayne

==========

Date: Wed, 14 Nov 90 09:08:00 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: Volitional Action: Conatiion and Control

Gary:

"Volitional Action: Conation and Control" is the title of the book that I edited. It is Volume 62 in North-Holland's "Advances in Psychology" series. The citation is:

Hershberger, W. A. (Ed.). (1989). "Volitional Action: Conation and Control." Amsterdam: Elsevier/North-Holland.

The ISBN number is 0 444 88318 5

The publisher's address is:

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1014 AG Amsterdam
The Netherlands

The preface and the table of contents of "Volitional Action: Conation and Control" are reprinted below:

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PREFACE
("Volitional Action: Conation and Control")

During the last several decades the behavioral sciences have been undergoing what is arguably a Kuhnian scientific revolution, with radical behaviorism giving way to considerations of cognition and conation. Although cognition is perhaps the more familiar of these two terms, conation (concerning the inclination to act purposively) is equally a hallmark of the times. Indeed, the past few years has seen a resurgence of interest in the psychology and physiology of volition that is unparalleled in this century. Not since William James published his "Principles of Psychology" in 1890 has so much careful attention been devoted to a

consideration of the will.

The present book comprises a significant sample, or distillation, of the observations, both rational and empirical, of individuals from diverse disciplines who are contributing to the present renaissance in conation. The book was designed to serve a threefold purpose: (a) to consolidate the gains of the various scholars, relatively isolated in their respective disciplines, (b) to foster and help focus future research on conation and self-control, and (c) to provide practitioners in applied psychology with a broad-based tutorial.

William James noted that there are two fundamental things to be understood about voluntary action: First, volitional actions, being desired and intended beforehand, are done with full prevision; that is, they are preceded by anticipatory images defining what those actions are to be. Secondly, these anticipatory images are representations of the intended sensory consequences of the necessary muscular innervation and not representations of the muscular innervation itself. (James' putative image is not to be confused with von Holst and Mittelstaedt's efference copy.)

The chapters in this book have been authored by individuals with something further to contribute to our understanding of one or both of James' observations. For example, some authors have been investigating the neurological signals which precede voluntary movements (e.g., Georgopoulos; and, Kornhuber, Deecke, Lang, Lang, & Kornhuber) whereas others (e.g., MacKay & Crammond) have been concerned primarily with the sensory feedback from the effectors involved in such movements. And still others, such as those with systems approaches (e.g., Bullock & Grossberg) are concerned with both aspects.

The theoretical flavor of the book is largely cybernetic or control theoretic. That is, most of the authors are committed to the proposition that voluntary actions are intentional, self-controlled inputs or sensations (including, in some cases, the sensed corollary discharges of efference), just as James implied. The principal champion of this notion today is William Powers (see Chapters 2 & 13), who used the idea as the title of his influential 1973 book, "Behavior: the Control of Perception."

William James also noted that the sensory consequences which define a particular voluntary action may be resident or remote. Sensations arising from muscle spindles are resident sensations; those arising from exteroceptors are remote. A person driving an automobile, for example, is controlling the remote visual consequences of his or her effector activity. The driver is also controlling his or her destination, another remote sensory consequence. Some of the authors, particularly those with a psychological or sociological perspective (e.g., Hyland) are concerned primarily with the control of remote sensory consequences, whereas others, particularly those with a physiological perspective (e.g., Pavloski), focus more upon resident sensory effects. This, of course, is as it should be. The two perspectives are complementary.

Volition is a phenomenon of immense practical as well as theoretical significance, and several chapters (e.g., Lord & Kernan) address the applied aspect. Professional psychology is in need of a broader scientific foundation than that provided by 20th century behaviorism. Conative science is a veritable cornerstone for such a new scientific foundation. I believe practitioners will find the

observations in this book (even the esoteric ones) uncommonly stimulating, informative, and professionally relevant.

The chapters are grouped according to the methodological approach of the author(s) into 5 sections: theoretical, neurophysiological, mathematical, psychological, and practical, in that order. Within each section the chapters are ordered alphabetically, by author.

Wayne A. Hershberger
DeKalb, Illinois
June 1989
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Richard J. Robertson

APPLIED PERSPECTIVE

20. A Paradigm Shift in Behavior Therapy: From External Control to Self-Control
Dennis J. Delprato

21. Fostering Self-Control: Comments of a Counselor
Edward E. Ford

22. Control Theory Applied to Stress Management
David M. Goldstein

23. Application of Control Theory to Work Settings
Robert G. Lord and Mary C. Kernan

24. Effective Personnel Management: An Application of Control Theory
James Soldani

25. The Giffen Effect: A Control Theory Resolution of an Economic Paradox
William D. Williams

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The price is 235 Dutch Guilders, about $110--it depends on the current exchange rate. It is most convenient to use a charge card (e.g., Visa, or Master Card) and you also get the best exchange rate that way.

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Thanks, Gary, for passing on this information.

Warmest 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

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Date: Wed, 14 Nov 90 15:01:52 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mark-olson@UXA.CSO.UIUC.EDU
Subject: hello, muscle testing,etc

To all CSG subscribers,
Hello, I think that its about time I introduce myself since I've been listening in on your conversations for the last month. My name is Mark Olson. I am a graduate student in Educational Psychology at the Univ of Illinois. Most importantly, I'm very excited about the ideas that are being discussed on this network.

Now for a question...is anyone out there familiar with "muscle testing?" It is a procedure used by many people for a variety of purposes. Chiropractors use it to determine where various neural pathways are being blocked. Various practitioners use it to determine what things a person is allergic to. The procedure is as follows: Person A stands with his arm extended out from him, parallel with the ground. Person B applies a firm, consistent resistance downward on person A's arm. Person A resists and the arm remains in place, just as we would expect. However, place an object which person A is (unknowingly) allergic to and upon retesting, the arm drops. I have heard a number of reasons for why this occurs and none are very satisfying--not until today did I realize that its a case for control theorists. Any ideas?

Concerning the idea that the Universe is some sort of control system...John Gribben, in his book Cosmic Coincidences stated in chapter 1 some of the various unique qualities of the Universe. One of them is extreme "flatness." This means that the Universe seems to be "right at" the critical density above which it would eventually recollapse upon itself and below which it would continue to expand. For various reasons which I could explain if anyone is interested, life in the Universe is entirely more probable given the flatness which we now observe. The actual density of the Universe now is within one in 10^15 parts of the critical density. This means that at the moment of the Big Bang, it was within one part in 10^60 of the critical density. True, if it were otherwise we wouldn't be here talking about it but the specificity of that number is so great that I would have no problem believing that in some way, shape, or form, this constant was CONTROLLED for (i.e. I could find myself agreeing with Chen that intelligence is inherent in Reality) It's all post hoc, but I thought you would find it interesting.

--Mark Olson
mark-olson@uxa.cso.uiuc.edu

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Date: Wed, 14 Nov 90 15:47:36 -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: Intelligence in the Universe

Mark Olsen:

I just had an interesting talk with biologist Jay Mittenthal about the second part of your question.

How did the universe "know" how fast to expand so that it would neither collapse upon itself nor expand so fast that no heavy matter could form? I would answer this by asking how many Big Bangs occurred before one occurred which, by chance, happened to result in a universe which could evolve into what we see today.

This reminds me again of Dennett's betting hoax. After the fact it may look like there was some intelligence in the system when they really was none.

I'm sure Chen will have something to say to this.

--Gary

P.S. What happened to all the interesting talk of "real" control theory issues, i.e., using control to model the behavior of living organisms. We have had quite a few new subscribers lately. They may well get the idea from recent discussion that evolution and cosmology are the main concerns of control theory! What about Bill Powers' revision of the model? The Marken effect? Jordan and Hershberger's eye movement studies?

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: Wed, 14 Nov 90 14:47:54 -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: Ultimate Tissues

Gary--

I would love to get some discussion going again on "living control systems". These big cosmological issues rather give me a headache. I'm as interested as anyone in the "What it's all about" cosmic stuff. But I'd really much rather spend my time trying to understand stuff that I might have an outside chance of understanding in my life-time. I pretty much gave up thinking about stuff like "what's the universe inside of" and "what's god inside of" and "what's inside of the smallest thing" when I was five or six. It's fun but cosmology just ain't my thing.

I actually think there is a point here that I want to mention. One of the reasons that people DON'T get interested in control theory is that it does not seem to be addressing the massively deep, cosmic questions of mentality that other theories seem to address. I think the popularity of "neural nets" and AI has to do with the cosmic nature of the language that they use -- they are trying to understand the NATURE OF INTELLIGENCE or how the brain evolved to be able to understand the NATURE OF REALITY. You know how they talk.

Control theorists are trying to understand purposeful behavior. It is a phenomenon that is easy to observe, possible to test, mathematically

tractable and intrinsically human. But there is an astoundingly deep concept in the apparently simple control theory explanation of purpose -- and that is that behavior is the control of perception. As I said in my very first posting, that idea is as deep and profound as any that has been proposed in any other science. I don't know why that idea has not caused shivers of excitment throughout the world of the life sciences. I think its just an idea that really needs to be understood; savored, whatever. But it just doesn't seem to sell in "profound" circles.

Anyway, to me, the beauty of how control systems work and the accuracy with which they explain purposeful behavior is a good enough high for me.

I am, as I have mentioned before, giving a presentation on "Psychological theory and human factors engineering" this Saturday. I will give a demo of the "Marken effect" in manual tracking. I will use it to show that control theory can have some practical benefits. Moreover, I will use it to show that an S-R approach to control (which is what manual tracking types have used)
leads to effort to design better displays (stimuli) rather than looking at the person as the controller. Actually, I will argue that all current models of human nature are based on what I call the "Lineal causal model". The influence of this model is so profound that it has even led people who understand control theory to interpret the theory in S-R terms when applied to human behavior. It is amazing. Therefore, for those who think that control theory is dull because it is old hat -- forget it. There is plenty of excitement left because the engineering psychologists never realized at all that they had a revolution by the tail.

Anyway, this talk (and work) and everything is taking up my time but I had to try to get a post off to try to get some of us out of the clouds so that we can look at the wonders and miracles that are right here in our very own cars, homes and offices -- ourselves.

PS. Thanks, Wayne, for giving the ordering info on your book. I didn't mention it myself because I thought it might be a bit too expensive for those on the net looking for books for their own library.

Best 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: Thu, 15 Nov 90 07:18:00 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: Marry Accident

Hello again. I'm back from a very nice visit with Hugh Gibbons (more on that later, but much later) that didn't end very nicely. I was checking in to come home from Manchester, NH, when I got a message that Mary had been in an auto accident. I went directly from O'Hare field to Glenbrook Hospital in Northbrook, where I found that she had a broken forearm, a shattered kneecap, and was in surgery having her spleen removed. I am assured that she will recover all right, but right now (this happened

yesterday afternoon, Wednesday) that day looks a long way away to me, and I'm sure even longer to her. I'll be going to visit her in a few minutes, so I can't comment on all the mail that was waiting this morning.

I know that all her friends will be thinking about her, and she will be cheered by knowing that, too.

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

Date: Thu, 15 Nov 90 09:25:13 -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: New Subscribers

There have been quite a few new subcribers to CSG-L who have not yet introduced themselves. Here is at least a partial list those for whom I cannot recall introductions.

LO0745@ALBNYVMS OETJEN-GERDES Lynne A., Albany, NY
poolla@CONTROL.CSL.UIUC.EDU POOLLA Kameshwar U Ill-Urbana
BARKANA@DUPR.OCS.DREXEL.EDU BARK-KANA Izhak
clarg@ESSEX.AC.UK CLARKE Graham: U Essex, UK
grino@IC.UPC.ES Robert Grino (IC)
EK3@MEMSTVX1 Ed Koshland
mcnamara@MGI.COM MACNAMARA Curt: Mgmt. Graphics, Mpls. MN
JEB1@MSSTATE Gene Boggess
HALEY@SDNET HALEY Bill
coleman@SSURF.UCSD.EDU COLEMAN Brian: U Cal San Diego
C538435@UMCVMB MISTRY Sanjay
remerm@VMD.CSO.UIUC.EDU MCAULEY Edward: U. Ill.-Urbana @uiucvmd
mark@WACSVAX.CS.UWA.OZ.AU NELSON Mark: U of W Australia

Please let us know your research interests and how you learned about control theory and CSG-L.

We are waiting to hear from you!.

Reminder: A full list of all subscribers to CSG-L can be obtained by sending a message to LISTSERV@UIUCVMD (bitnet) or LISTSERV@VMD.CSO.UIUC.EDU with the following command as the message:

REVIEW CSG-L

--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: Thu, 15 Nov 90 10:51:41 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

From: Jay Mittenthal <mitten@UX1.CSO.UIUC.EDU>
Subject: entry to CSG

Hello, this is a test before I send a self-introduction, to be sure this mailing address works for me. Do you get this? Jay Mittenthal

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Date: Thu, 15 Nov 90 09:33:55 -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: Got it

Jay Mittenthal

Yes, Got it

Rick M.

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Date: Thu, 15 Nov 90 12:54:58 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: intro/inquiry

I too will crawl out of the woodwork and say hello. My name is Joel Judd and I too am a graduate student in the department of Educational Psychology at the U. of Illinois, Champaign.

Gary Cziko introduced me to Control Theory but reading the mail I have to say I feel like I'm getting a worn spot on top of my head where so many comments continue to pass over.

My interests are second language teaching and learning, so my interests in CT are from the perspective of what it might have to say about language learning in general, and bilingualism, in particular. Any available material along those lines would be appreciated.

Gary's recent P.S. comment asking about Jordan and Herschberger reminded me to ask them about their recent work on eye movements. Gary described a paper dealing with the possibility that we mentally shift our perspective in preparation for eye movements? I would like to find out details, especially since I am sitting in on a seminar by George McConkie this semester dealing with eye movements and eye tracking.

Joel

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Date: Thu, 15 Nov 90 14:29:13 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Jay Mittenthal <mitten@UX1.CSO.UIUC.EDU>
Subject: self-introduction for Jay Mittenthal

I'm a biologist in the Department of Cell and Structural Biology at the

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Date: Thu, 15 Nov 90 15:40:47 -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: Random Notes

Here are some random thoughts on a thursday:

Tom -- Always nice to hear from you. You are right, of course. Positive feedback is probably only good for one thing -- amplification. It certainly doesn't produce control. By the way, what are you up to these days. Anything new on the meeting? Newsletter? Publications?

Wayne and Scott - Haven't heard from you in a while. How about a report on your
eye movement stuff. I think your study would be a good place to start doing some evaluation of Bill's revised model -- where p(n)=f(r(n-1)+e(n-1)). Your data is largely responsible for motivating this new model. How about some discussion of what it implies -- when higher order perception is a function of what the system intends to perceive (r(n-1)) and the lower order error in perception (e(n-1)). There must be some interesting dynamic considerations in such a model -- the intended perception must be lagging slightly behind the lower order perceptual error -- so the higher order perception p(n) is what was intended combined with what is the current discrepancy. Your results imply that there is a lag between intended and actual perception, right? Have you guys done any modeling. I think this would be very helpful.

On another substantive note that might be related to the new model even more than to the old -- how do we put attention into the model. This came up at the CSG meeting in August. Ray Pavloski has been having subjects control two different variables simultaneously. Of course, at first, they are worst at one or the other when doing both than they are at either one alone -- a well know finding in psychology and a big motivator of all those studies of attention in Cognitive psychology. Well, there is nothing in the control model, as it sits, that says why doing two things at the same time should be any harder than doing just one or the other. Any thoughts on this?

Bill Powers - I am really sorry to hear about Mary's accident. I hope she feels better soon. Please give her my very best wishes.

Love

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: Thu, 15 Nov 90 21:37:48 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: Comments on recent comments

21:00 on Thursday, Nov. 15. Was it really just yesterday? Mary's sleeping and looks a lot better. She's on enough goofy juice to stay comfortable. They've discovered control theory: she has a button she can press to administer one milligram of stuff through the IV (max rate, once every 6 minutes). No anxiety about attracting someone's attention 3 hours and 45 minutes into a dose of once-every-four-hours dope.

She won't be home for 1 to 2 weeks. She's at Glenbrook Hospital, Northbrook, IL 60062.

A few comments.

Yes, I agree with Rick and Tom: cosmology and all are fun, but we do have business to transact. It all gets a little too loose for me, too.

Mark Olson: I wonder where Gribben got that 1 part in 10^15 approach to "flatness." Astronomers are still looking for the "missing mass" needed to account for galactic rotation; it's something like 90 percent of the estimated mass of the universe. Is he saying that someone knows EXACTLY how much is missing? Or that someone knows the Hubble constant to one part in 10^15? Wow. Sounds like either a garble or bullshit. There's a lot of both going around. Why do you believe that report about the allergy thing? Have you tried it?

Various commentators: I think we have a good case for saying that organisms work by negative feedback. In a collection of organisms mixed in with a passive environment, you can have relationships AMONG THE ELEMENTS of any sort: negative feedback, positive feedback, or no feedback. If you know that some elements of a "social" system are control systems, and understand the properties of the environment, then the kind of "social" feedback that exists isn't a matter of opinion. Solve the equations and see what kind it is. It could be any kind in any particular case. As to positive feedback and evolution: sure, maybe. But where's the test? (The same question applies to my guesses).

Jay Mittenthal: Biologists, for some unfathomable reason, seem to hate the idea of reference levels. This may come from thinking of a single isolated "homeostatic" control system, and from failing to realize that reference levels can depend on other variables (for example, the outputs of superordinate systems). The biological argument seems to go like this:

You have a two-variable system, x and y. Y depends on x according to

$$y = -a(x - x0) \quad \text{(for example)},$$

and x depends on y according to

$$x = by + c$$

Thus we have two lines that intersect to give the solution-state. The reference level x0 "falls out" of these equations (it isn't at the intersection point).

Right. The loop gain is the product -ab. The disturbance is c. If -ab is much more negative than -1, and a is much larger than b, we call the result a control system (the first equation). If x0, the reference level, depends on some other variable, the path through which the dependence is mediated is called the "reference signal", and it becomes the means of telling the control system how much input x to experience. X is protected against variations in c to the extent that the loop gain is large. The system comes to equilibrium with x not at, but very near, the reference level x0. So what's the problem? I think the problem is spelled S-T-R-A-W-M-A-N. No control theorist in our bunch has ever said there is "a fixed reference level sitting somewhere in the body." Or anything even close to that.

Control systems make one variable depend on another in a way that isn't governed by the obvious physical/chemical laws. It seems to me that this should be an interesting notion to a biologist.

Good night, all.
Bill P.

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

Date: Fri, 16 Nov 90 11:56:14 U
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Mark Nelson <mark@CS.UWA.OZ.AU>
Subject: An intro from OZ

Hi! I'm Mark Nelson and am currently preparing my PhD thesis, titled "A Control Architecture Towards Intelligent Behaviour". I have been working on a navigational control system for a mobile robot (computer simulated) that enables the robot to exhibit a navigational behaviour which may be observed to simulate some overt aspects of navigational intelligence.

The main results of this thesis are:

- the model of control architecture (influenced by Bill Powers work),

- the methodology for incremental behavioural development (influenced by Rodney Brooks "Subsumption Architecture"),

- and as a consequence of the above, a capability has been developed that enables the system to regulate the complexity of its control in response to constraints imposed by the environment. That is, for certain situations simpler control algorithms may be used to generate responses that are more likely to satisfy the constraints caused by stationary obstacles and/or moving objects.

I have read "Behavior: The Control of Perception" by Bill Powers and found it very interesting and most enlightening. So, when I read about this mailing list in CYBSYS-L I was interested to find out about the type of topics that are being discussed.

Late last year I tried to buy a copy of "Behavior: ..." but the campus bookshop here told me that the book was no longer in print. Bill Powers, are there any plans for a new edition or for the book going back into print?

Mark Nelson
PhD student
Thesis: "A Control Architecture Towards Intelligent Behaviour"

Department of Computer Science, CSNet: mark@cs.uwa.oz.au
University of Western Australia, ARPA: mark%cs.uwa.oz.au@uunet.uu.net
Mounts Bay Rd, UUCP: ..!uunet!munnari!wacsvax!mark
Nedlands, Western Australia, 6009.
PHONE: (09) 380 2305 OVERSEAS: +61 9 380 2305

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Date: Thu, 15 Nov 90 23:18:06 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: OZ

Mark Nelson,
   What I would give to have an addrrss in OZ! Welcome to CSG-L. The idea that someone might be tuned in from the other end of the day will make it all the more tempting to log on in the middle of the night.
   If you have any printed material on your thesis, I would like to see it. You can reach me at this address:

Tom Bourbon
Department of Psychology
Stephen F. Austin State University
Nacogdoches, Texas 75962-3046
USA

  Bill Powers will probably respond to your inquiry, but his book is still in print. I'm sure he will send details.

Tom Bourbon <TBourbon@SFAustin.BITNet>

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Date: Thu, 15 Nov 90 22:54:32 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: re random thoughts

Rick Marken,
   Bill's news about Mary was quite a shock. I'm sure that if we all put our messages to her on the net, Bill can take her a feet-long get well message.
   A few days ago, our people in Eugene, Oregon (Phil and Margaret Runkel and Luke Aitken) sent the Powers and me a proposal for the next meeting. I was waiting for Bill to return from his trip so we could huddle via Bitnet and decide whether to go with the arrangements. The Oregonians have an option for us on some lodges on the McKenzie River, in the mountains above Eugene. The setting sounds excellent. As soon as Bill, or MAry if she is up to it, can give me some information about charges for past meetings, we will decide. The dates would be from Wednesday, 2 October 1991 (for early arrival) through Sunday, 6 October 1991.
    There was a recent posting about the next issue of the newsletter which is planned for February. Everyone should send Ed Ford at least a short piece on their work and ideas on living control systems.
   As for publications, I am working -- between my sessions with students -- on the revision of the paper Bill and I had rejected just after the CSG meeting. Several people in the group made helpful suggestions about how to improve the manuscript, and there were extensive and meaningful suggestions from two of our "silent" members of the net -- Michael Hyland and Warren Thorngate. And along with several of my students, I have a manuscript in press with _Perceptual & Motor Skills_ -- I'm not proud, I want it out! This is a simple thing that merely archives the reliability and accuracy of predictions by the model, in tracking tasks (104 replications, with a mean correlation between predicted and actual positions of the subject's handle = 0.996, with a standard deviation of 0.002. Not bad, considering that we used the "old, unimproved" version of the model!) And this one includes the data from four runs in which one year elapsed between the predictions and the tracking sessions. There is nothing earth-shaking, but we document the extreme reliability of the model and get in a few digs at conventional claims that behavior is too variable for behavioral scientists ever to predict it with any precision.
   In the lab, a number of projects are at the "nearly ready" stage. I'll say more about them later.

Take care.

Tom Bourbon <TBourbon@SFAustin.BITNet>

Date: Fri, 16 Nov 90 06:29:01 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti1@VUB.VUB.AC.BE>
Subject: big stuff

Gary Cziko:

I think Tom and Rich are right. This is the list about control theory.
It's not suitable (or useful) to discuss those things like the universe here.

Chen

Date: Fri, 16 Nov 90 08:51:00 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: Wish Mary well

Dear Bill:
I just now read of Mary's life threatening accident; what a shock! I am immensely relieved to read, in your subsequent posting, that she is on the mend. But I'll not rest easy until I hear she is again home safe with you. My thoughts are with you both.
Love, Wayne

Date: Fri, 16 Nov 90 11:42:42 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Clarke G <clarg@SX.AC.UK>

Hello everybody. I feel a bit awkward, it's like wandering into a private party here. The group of people involved seems small and intimate, everyone seems to know everyone else and I'm not sure it was a good idea I came.
I heard about the group through the Cybsys List and I heard there that Bill Powers control theory was comparable to Gregory Bateson's work. Since I think that Steps to an Ecology of Mind and Mind and Nature are essential reading I was interested.
Eaves-dropping on the conversations at this party I have yet to get a clear idea of what control theory is about. What it says about the relationship between an organism and an environment or an individual and a society. It is this latter that interests me most since within A.I. there seems to be an almost totally uninspected committment to methodological individualism when it comes to modelling individuals powers.
I work as Systems Administrator for a number of labs at the University of Essex, in the Dept of Computer Science, mostly Sun kit but with some Apollo workstations too.
My research interests centre on that level of organisation that is usually mapped into either 'deep structures of the psyche' or 'social institutional structures'. In short the material pre-conditions for human behaviour. Like Bateson I think this is a both/and not an either/or matter. It is the way it is sythesised that matters. My aim is to produce such a synthesis; the reality is fuller of holes than a string vest.
Graham Clarke
Computer Technical Officer
University of Essex
Colchester, Essex, England.

Date: Fri, 16 Nov 90 09:49:47 -0600

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Clark McPhail <cmcphail@UX1.CSO.UIUC.EDU>
Subject: Revised coordinates for Mary Powers

In case anyone wants to send cards or flowers to Mary, the following address was provided to me by the hospital in Glenview (not Northbrook) IL:

Mary Powers
c/o Glenbrook Hospital
2100 Pfingsten Road
Glenview, IL 60025

According to the Intensive Care Unit nurse (930am, cst, 11/16/90), Mary "had a good night" and may be moved from intensive care to a non-intensive care room today or tomorrow.

Clark McPhail
Dept of Sociology-326LH
Univ. of Illinois
702 S. Wright
Urbana, IL 61801

Bitnet: cmcphail@uiucvmd
Internet: cmcphailO@ux1.cso.uiuc.edu

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Date: Fri, 16 Nov 90 09:56:19 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Powers Book

In response to Mark Nelson's inquiry about Powers' 1973 book, _Behavior: The Control of Perception_, here's information on it's availability.

Those in the U.S. and Canada can obtain the book from Aldine Publishing Company, 200 Sawmill River Road, Hawthorne, NY 10532. The book is an absolute steal at $38.95 plus $3.00 for shipping. You can also order by phone using MasterCard or Visa by calling 914-747-0110. They have about 100 copies left.

Outside of the of the U.S. and Canada you're supposed to order through De Gruyter, Box 110240, D-1000 Berlin, Germany 11. However, there is an out of print symbol on this book from Germany. This is probably why Mark Nelson could not get the book.

Therefore, if someone from outside the U.S. or Canada wants the book, they should contact Aldine in New York and have them send the book to someone they know in the U.S. or Canada to be then forwarded overseas. If someone overseas has no contact in the U.S. or Canada, he or she can have the book sent to me and I will forward it by surface mail, unless they want to pay me for airmail shipping.

I am hoping to put together a list of key books on control theory with ordering information in the near future. --Gary

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Date: Fri, 16 Nov 90 15:30:58 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mark-olson@UXA.CSO.UIUC.EDU
Subject: muscle testing

Bill:
In your question to me about why I believe in muscle testing, it seemed that you were implying that I shouldn't. It definitely does "work." I've seen it, experienced it, and "practiced" it many times with a variety of people in a variety of situations. No placebo here. My question is "Why does it work?" How does an allergen perceived at one part of the body affect another part of the body's ability to maintain a referenced position? I think this phenomenon is less known because its a type of phenomenon that medical science would not be looking for. That's a guess. I have heard this procedure is used for Olympic athletes in training--not for allergy testing though. It's related to the reason why chiropractors use it. Chiropractors will use this procedure by varying the position of the arm or leg. Some positions will be able to resist, others won't. Through this they are able to determine (if my understanding is correct) where various nerves are "pinched" or "blocked." This is layman's terminology--that's all I know. I just thought maybe someone with more expertise in this field would be able to make a connection with this to control theory.
--Mark Olson

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Date: Fri, 16 Nov 90 19:24:31 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: progress, miscellaneous comments

Thanks for the messages for Mary. They will be relayed. She looks LOTS better today. When I visit tonight she will probably be in a regular room.

Mark Nelson: Welcome aboard. I look forward to hearing about the theoretical issues that your work brings up.

Next meeting: The place looks a little small to me. If 35 people show up we couldn't even eat in the same place. Even with 22 (the reported max for staying at one place), everyone is going to have to double (or quadruple) up to have enough sleeping space. The dining room seats 22. Are we going to predicate the meeting on having 22 people? Of course the location sounds smashing. I vote maybe, but think that we need to look further.

Graham Clarke: The "private party" comment is something we all need to think about: thanks for bringing it up. For newcomers it must be pretty baffling. The CSG is definitely NOT exclusionary on any grounds, but what holds it together is a common understanding of control theory. That's more or less a sine qua non. Therefore it behooves the old-timers to devote some effort to help new-timers with the basic ideas. For some -- like you, Graham, probably -- the quickest route is to program some of the simple models we've been using and get some first-hand experience with control-system phenomena. Set up a tracking task (should be easy with the Sun's mouse) and then experience the joy of watching the control-system model exactly duplicate the mouse movements. What programming languages do you know? We can probably come up with at least the skeleton of a tracking program in source form that you can adapt. None of us have Sun or Apollo workstations, but that shouldn't be a barrier. Ask questions. Maybe out of this could come a tutorial file of general use (with Gary Cziko's system-manager help).

Mark Olson: I asked if you had tried it because although I'm a born

believer, I'm a trained skeptic (trained by bitter experience). Before I spend time trying to explain a phenomenon, I want to know if it's real or just statistical. I want to know things like how many people show the phenomenon, how you find out that there's an allergy, how many trials show the effect and how many don't, how you keep the person from getting extraneous information about the substance -- all that stuff. Once I'm convinced that there's a real phenomenon, it's time to think up explanations.

Accepting your report at face value, I can drop one hint. The spinal control loops that are responsible for the low-level control of arm position receive inputs directly from tactile receptors in the skin of the same limb. These inputs are injected in such a way that they bias the reference-position of the arm, and thereby alter at least some of the signals that tell higher systems where the arm is (I assume vision isn't involved in your experiments). Receptors called "tactile" are called that because they are known to response to touch, but the same spinal effects are obtained from skin receptors in general: heat, pain. Who knows what else can be sensed by receptors in the skin? All it takes is something that can make the sensory receptor fire. So that's a possibility -- but only if you have a bona fide, 100 percent, gold-plated phenomenon. I'm not interested in 80 percent correlations. That's way too low to define a phenomenon.

Off for another visit, bearing messages.

Bill P.

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Date: Sat, 17 Nov 90 10:56:36 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: BARKANA@DUPR.OCS.DREXEL.EDU
Subject: Glad to meet you

Dear CGS-L networkers:
I am Izhak Bar-Kana and my main field is Adaptive Control, with some neural networks and robotics. I subscribed to this list, mainly due to its "Control" name, and intended to be a quiet listener. The discussion is very interesting, and to me, quite surprising.
In particular, I was confused by the discussion on the intelligent Cosmos. It is difficult for me to accept the idea of intelligence before at least some form of life. Actually, some very developed form of life. "Artificial Intelligence" (always better than none) is trying to immitate natural intelligence, but is still an outcome of some superior (human) intelligence.I don't even know if proving that the universe is intelligent is cosmology. To me, it looks more similar to "scientific" arguments about existence or inexistence of God.

About Positive Feedback and evolution. Isn't "adaptation" the word? A simple time-invariant mechanism can perform that much. If the task becomes more difficult, the "tracking errors" (the Performance Index that the system tries to minimize) are used to change the gain , f.e. speed vs. error (as in the case of pursuer) even in the simplest adaptive mechanisms. After a while, what previously was an extremal situation, becomes a normal situation, because the evader is faster.A learning system identifies it as the normal situation, which in my humble opinion is expreseed by development of muscles, etc., as it happens when we train.
But this is only a pretext to introduce myself, and to explain why I will be mainly a quiet listener, at least for a while, until I get the opportunity to read more of your works and speak, at least, the same

language.

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Date: Sat, 17 Nov 90 17:48:00 CST
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: visual direction, Marken effect

Joel Judd:

I am mailing you a reprint of a paper entitled "Saccadic eye movements and the perception of visual direction" (Perception & Psychophysics, 1987, 41 (1), 35-44). Scott Jordan's dissertation research investigates the phantom array described on pages 38 and 39 under the heading NEW EVIDENCE (we demonstrated the phantom array at the CSG annual meeting in Pennsylvania this year). Although many have noted the smeared image of a dim light source while saccading across it (e.g., L. Matin, 1972) that spatially extended smear is singularly uninformative. However, by pulsating the light source the smear is removed, and the resultant phantom array allows one to see what is going on in the nervous system: a discrete shift in the spatial coordinates (local signs) of the retina--not at all what Matin and others have supposed; they have interpreted their data (reams of it) as indicating a VERY GRADUAL CONTINUOUS shift. The discrete shift appears to be a manifestation of a reference signal (intended eye orientation) that is shifting from one value (direction) directly to another.

We expect to present some data at MPA in May demonstrating that the phantom array is a phenomenon that is replicable across naive observers. Please look for us there.

Everyone interested:

Scott is still gathering data, which he claims to be "beautiful." I gather this means his data indicate that the shift immediately precedes the eye movement, just as a reference signal would, but he has yet to analyze his data formally. We will keep you posted.

Rick:

Congratulations, and thanks, for getting another control-theory paper into mainstream psychology (i.e., the journal Psychological Science, the flagship of APS)--a foot in the door, that's what we need. And God bless Estes, too. As I recall, Estes accepted Bill's classic 1978 paper when he (Estes) was the editor of Psychological Review.

Rick, I am confused about the Marken effect. What is it exactly? Two conflicting (cooperating?) control systems (why two people?) with different gain? Different lag times? Predictable disturbances? What? Is it MERELY a matter of stability (dynamic damping)? And what exactly is the practical HUMAN(?) factors application? Please bring us all up to speed. I would like to reintroduce the "endogenous disturbance" thread, again but I am not clear whether it is relevant to the Marken effect.

Warmest regards, Wayne

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Date: Sun, 18 Nov 90 11:59:51 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Phase Lag

All Serious Control System Modelers:

I spent an interesting evening yesterday with Petar Kokotovic. He is a neighbor and good friend who also happens to be one of the top engineering control system theorists in the world (he just won his international organization's medal which is given only every three years).

Of course we talked about living control systems. He is naturally sympathetic to the idea that organisms are living control systems. However, from what I told him of the Powersian models and from what he has read in the _Living Control Systems_ collection which I lent him this summer, his reaction was that these systems are too simple to adequately characterize the complex control systems underlying human behavior.

One point he stressed was what he called phase lag. Imagine a sinusoidal disturbance. Imagine for a moment an open loop system in which you are looking at some action and a response (he used the example of turning up and down the hot water tap in the shower and its effect on the temperature of the water as it comes out of the showerhead). As the frequency of the action increases, there is an increasing phase lag, measured in degrees or radians, between the action and the effect of the action on the observed variable (water temperature). At a certain critical frequency, the lag will become 180 degrees so that when you turn on more hot water the water coming out of the showerhead gets colder, etc. Now, if you close the loop by putting someone in the shower who is trying to control the temperature with disturbances at this high frequency, instability will result with any negative loop gain greater than 1.0 (makes sense; if you are half a cycle behind, negative feedback turns into positive feedback).

He was clear to make a distinction between this phase lag (which can be understood as a type of inertia in a mechanical or a combination of capacitance and inductance in electrical systems) and transport lag (which is what Bill Powers and Rick Marken have been discussing recently).

Sophisticated control systems in engineering, therefore, vary the loop gain according to the frequency of the disturbance. If the frequency is low, high loop gain is fine, but when the frequency (and therefore the phase lag) increases, the loop gain must be turned down to avoid instability. He also mentioned different types of filters can be used to "anticipate" the phase lag (but I remember him saying that this was not feedforward).

One way of understanding phase lag in the computer tracking tasks using a mouse is to increase either the frequency of the disturbance OR the weight of the mouse. In either case, at a certain frequency/weight combination unstability will set in if the gain is not turned down. He even demonstrated this informally by my trying to track his hand with my hand. At a low frequency, I could keep my hand close to his, but when he got faster, I found myself making smaller corrective actions, staying closer to the middle of the range of his motion. I had turned down my loop gain.

As a relative newcomer to control theory, I may have misunderstood and/or

miscommunicated Kokotovic's points, but here they are anyway. My questions are therefore:

1. Is phase lag something that Bill Powers or others have talked/written about and I have missed?

2. Is phase lag considered important in living control systems (since transport lag was originally unpopular but now "in", perhaps phase lag is the next lag to consider).

3. Are the sophisticated aspects of control theory that today's engineers deal with relevant to our interests? If so, is that type of expertise represented in the Control Systems Group? If not, how can we get it.

That's enough for a Sunday morning. If this message doesn't keep us out of cosmology for at least a little while, I don't know what will!--Gary

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Date: Sun, 18 Nov 90 12:22:04 -0600
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Fixed reference levels

Bill Powers:

Concerning fixed reference levels, you recently said:

>So
>what's the problem? I think the problem is spelled S-T-R-A-W-M-A-N. No
>control theorist in our bunch has ever said there is "a fixed reference
>level sitting somewhere in the body." Or anything even close to that.
>

I quote from p. 173 of your 1973 book where you discuss under "Still Higher Levels" the possible source of top-level reference levels.

"There are a few _guesses_ I can make concerning the source of ninth-order reference levels, none very startling. Perhaps these reference levels belong to the class of motivations we term _instincts_. Instincts, after all, should occupy the top level of organization if they are not to interfere with learned behaviors. If one had an "instinctive" way of holding his right arm, he might find that learning to throw a spear is difficult. Even allowing for acquisition of some high-order reference levels, it is still possible that very generalized reference leves are inherited, serving as evolution's guide to behavioral organization."

I believe that there are other places as well in the _Living Control Systems_ collection where you make similar statements which suggest that there are fixed reference levels at higher levels. Since I don't like the whole idea of instinct (though I like inheritance), I'm pleased by the prospect that you may have changed your thinking about this. When you have time, perhaps you should bring us all up to date.--Gary

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Date: Sun, 18 Nov 90 22:11:40 CST
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Transport and Integral lag, Fixed Ref Lev

Gary Cziko -- trying to stir things up a bit, hey? OK by me.

Petar Kokotovic is perfectly correct about phase lags and transport lags. I didn't get into any of that in my books -- only some simple algebra. The algebra represents the steady-state solutions of the differential equations for a linear control system that is known to be stable. But I am familiar with the more advanced design considerations he brings up, more or less.

Has Kokotovic seen the Little Man arm demo? If so, he might be interested in knowing that it now includes the dynamics of the arm, with mass and damping. The transport lag in the "spinal reflex" part of this model (combined tendon and stretch reflexes, which nobody else we know of has modeled) is short, as in the real control systems of the arm (only about 9 milliseconds -- we actually use a longer lag represented by one computing cycle, dt = 0.03 sec). If we made it as short as it actually is the model would have to run very slowly and we'd have to wait too long for our instant gratification. So we're a little off on the numbers there. But we do have phase (integral) lags where they belong in the muscle model and of course the masses of the arm, and viscous damping as well as the phase advance introduced by the muscle spindles (stretch reflex) and recurrent integrating Renshaw (negative feedback) cells on the motor neurons. I have not yet introduced the transport lag of 0.15 - 0.20 sec in the visual part of the model (the lag I have found by matching a model to real tracking data, and talked about a couple of weeks, or was it years, ago). Also, Greg Williams found good references for the shape of the muscle force-length and force-velocity curves (including the effect of joint angle on mechanical advantage) and we have been trying them in the model, successfully but not yet in final form (a little cheating there). Further development of the arm model, by me at least, is on hold right now, partly because the next step is to revise all the screens that allow parameters to be changed. Just a lot of detailed programming to do, and a couple of other projects have to come first. The main one right now being a switch into the nursing mode. Greg will be doing most of the work for a while. We may have something to show by Summer 1991, certainly for the next CSG meeting.

As Kokotovic has already seen, it looks as though the gain of the human tracking system does change with the nature of the load. It also changes with the difficulty of the task (amplitude and frequency content of disturbances), but this might be explained in part by a nonlinearity in the system (I suspect the comparator). I think we'll try that first, because when you introduce active change of parameters the model gets a lot more complicated. But one day soon we'll have to look at the effects of altering the load dynamics and see how the real system handles them. You can't assume just from the falloff of your responses with speed of disturbance that the system's parameters are changing -- that could just be the natural high-frequency cutoff of the control system working, with no change in parameters. The only way to tell is to find the best model and run it. I'm glad we don't have to solve the equations!

All of this is far too esoteric for most readers of my books; Greg and I

are doing it for our own amusement and don't expect this stuff to be interesting to most users of psychological control theory. For most people, it may be nice to know that this level of analysis exists -- it keeps us honest -- but there isn't much in it for the clinician or the social psychologist.

I should add -- all the models that Tom Bourbon, Rick Marken, Ray Pavloski, and I have been using for five years or so employ phase lags. A model with one time-integration at its output (90 degree phase lag) accounts for at least 95 per cent of the variance in human tracking behavior. All these other details, including the transport lag thing, have come up through trying to get that last 5 per cent. I don't know how much farther we want to push this, but I'd say it's almost time to get on with more interesting (higher-level) experiments.

Fixed reference levels:

Yes, at the highest levels in the hierachy, fixed reference levels are a possibility. That's one of the ways of accounting for the highest level of reference signal. There are others: for example, the effective reference signal for a highest-level system (there might be many at the highest level, acting in parallel and controlling for different things) could be the average of all recent perceptual signals. Or it could be genetically set, which does not necessarily mean constant (circadian oscillators, for example). These reference signals could also result from a continuous process of reorganization, as though the system is continually searching for the combinations that will yield the nearest approach to zero overall error, experimentally.

But remember, the highest level of reference signals, in my model, would determine target system concepts, system concept being an extremely general abstract variable. In order to maintain a sense, for example, of a coherent family, one would have to manipulate all lower-level reference signals in a highly dynamic fashion, maintaining principles, programs, sequences, categories, relationships, and so on that include not only your own behavior but that of other people and the non-living world. You'd have to know a hell of a lot about the whole system even to detect the fact that something at the highest level is being held constant, especially as everything at all the lower levels would have to be in constant flux to accomplish that constancy. I think we are going to have to build up our model from the bottom so we can eliminate those features of organization that belong to lower levels before we can see clearly enough what the higher levels have left to do. It would be funny, wouldn't it, if we got up to about the relationship level and found that there was nothing left to explain. We'd have a lot of extra words left over.

Even if the highest reference signals are fixed (I doubt that they are), that's not the same as saying there's ONE fixed reference signal in ONE control system, is it?

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Date: Mon, 19 Nov 90 13:11:14 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: ADAPTIVE, PHASE AND THANKS

Mark Nelson -- I look forward to receiving your technical note

and, after that, your thesis. Thank you for offering to send them.

Izahak Bar-Kana: Welcome to CSG-L, even if you hope to remain a listener. Perhaps we can coax you from that intention. Many of us who began CSG-L are in the behavioral and social sciences, so we lack backgrounds in your areas of expertise. I, and probably several others, would appreciate information from you about good general references on the topic of adaptive control. I am presently working on models of human tracking behavior in which two people, or a person and a control-system model, interact. Two people can easily decide to change from one mode of interaction to another, and one person can easily recognize when the other has changed, then adapt to the new mode. I want my modeled person to develop the same capacity as a real one who detects the mode employed by the real person, then adapt. I'll admit, I am in over my head on the topic of adaptive control, but I suspect there may be some basic ideas there that will help me in my work.

I hope you will reconsider your decision to remain silent -- yours is precisely the kind of expertise many of us lack!

Gary Cziko: Nice job, drumming up someone who could come at us on the subject of phase lags! First, I second Bill Powers' remarks about how the simple models several of us have used for the past few years do include a 90-degree phase lag, so we are at least familiar with the topic. (Bill P. is more than just familiar with it -- but he is an engineer! Some of the rest of us squandered our academic training by learning to be "behavioral scientists!")

But your neighbor's remarks raise another important point. It is precisely because of the effects he described that we STRUCTURE our world in a way that ELIMINATES very-high frequency disturbances. Our equipment and other manipulanda are designed to conform to our range of effective control, much as the frames on a movie screen are made to change at a rate that EXCEEDS our ability to detect their succession. We have created an environment suited to our abilities. We even carry that principle through to the pacing of our conversations and social interactions -- at least when we are functioning well. The "miraculous" absence of multitudes of high-frequency disturbances is one of the clear signs that intelligence is present and I believe it is one of the distinguishing features of "civilization," in any form.

Of course, the non-manufactured parts of the world sometimes rear up and confront us with rapid changes, as do our social systems and our artefacts. Those are the times when we adapt, or we pay the price -- perhaps we die. The destruction of Pompeii and the leveling of San Francisco occurred a frequencies a bit too high for many folks to adapt and to exert effective control.

Keep talking to those neighbors of yours -- Urbana-Champaign has a different collection of folks than does Nacogdoches!

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Intro to Control Theory: Project

Mary will be coming home the 24th or 25th. She is VERY appreciative of the cards and flowers.

I thought I'd write a first draft of an intro to control theory for newcomers to the net. Others can take this and revise, add, etc. until we have a document that will be of some help to those who wonder what the heck

goes on in the CSG. Maybe Greg Williams will undertake to merge the versions. Here goes:

Control theory, as we use the term, means engineering control theory adapted for use as a model of the behavior of living systems. Those who already understand engineering control theory therefore already know part of the story. The rest of the story lies in the way we organize a model of control to explain organismic behavior. Sensors, comparators, and effectors appear in this model just as in ordinary models of nonliving control systems. Where we understand enough of real behavior, the models are set up much like models that others use and for the same purpose: to analyze behavior through simulations. But there are some critical differences.

In a living control system, the reference input is not accessible from outside the system. Engineering diagrams commonly show the reference signal as an input from the outside world, which it is in artificial systems: it's the means by which the human user tells the control system the level at which to keep its controlled variable. In a living control system, the "user" is the whole organism. Reference signals are set by higher systems that are also control systems (the higher systems act by adjusting reference signals for lower systems). In some cases the reference signals are derived from genetically-specified information (for example, the reference signal for body temperature). In the majority of the control systems that exist in the brain, however, the organization is learned within a general matrix of preorganization, and reference signals derive from the operation of a multi-leveled, "massively parallel" system.

One of the basic insights behind our uses of control theory is that all control systems control their own inputs, not their outputs. In engineering, this fact is obscured because the inputs are arranged so as to represent an external variable of interest to the user of the system, generally a variable directly affected by the actions of the system: position, temperature, acceleration, pressure, and so on. But a little thought will show that such variables can be known to the system only as signals generated by sensors; in every case it is the signal, not the external variable, that is under control (just picture what happens when the sensor drifts out of calibration). Our model must be understood from the viewpoint of the system itself, not that of an external user.

The human system knows the external world through millions of sensors. It affects the external world, and thus its inner world of sensory signals, by its actions. The sensory signals also play a part in the production of action: we propose, specifically, that it is the same role played by the sensory signals in control systems. This leads to a new understanding of behavior, in which action and perception are part of a closed control loop, the action serving to maintain the perception at whatever level is currently specified by an inner reference signal. External disturbances tending to alter the signals, the perceptions, result in actions that oppose those effects, thus leading to the spurious appearance that the

system senses the disturbances and simple reacts to them.

This picture is very different from a stimulus-response model, and it is also very different from a cognitive or command-driven model. One level in the model does not tell a lower level what act to perform: it provides an example (in the form of a signal) of the state to which the lower system is to bring its own sensory signal. The lower system itself provides the action needed to match perception to the reference. A sensory signal entering a control system does not cause any particular action to occur; the action is based not on the perception but on the DIFFERENCE between the perception's state and state currently being specified by the reference signal.

This model is very tightly interconnected. A perceptual signal in a given

control system is derived from the perceptual signals in a set of lower-level systems. The derived signal is of a new type; it is a function of the set of lower perceptual signals. This higher-level perception is compared with a reference signal, and the difference is converted to a set of output signals. These output signals enter THE SAME SYSTEMS FROM WHICH THE LOWER-LEVEL PERCEPTIONS CAME, serving as reference signals that specify the states of the lower-level perceptions. All loops are closed: all behavior at all levels is purposive. Every effect generated by any system is controlled in terms of the perception that represents it: nothing organized ever happens open-loop.

The evidence in support of this model ranges from excellent at the lowest levels to sketchy at the highest. Where we know how to do experiments, we construct quantitative working models and match them to behavior by adjusting their parameters. We're trying to expand the scope of these experiments to higher levels, but the going is slow. One factor that encourages us is that all control loops, in this model, can be detected and tested from outside the system, because all loops are closed, ultimately, through the environment. Where the model is wrong we can find out that it is wrong.

The model is also approached in another way, as an organizing principle for reinterpreting phenomena of behavior. Given the basic organization of control as we see it at the lower levels, the question is whether higher levels of organized behavior also make sense in these same terms -- more sense than when interpreted in conventional ways. So far the answer seems to be a unanimous yes. We are trying, however, to extend the method of modeling so it can be useful in areas where quantitative experiments are difficult. In this way we hope to test and buttress the insights of our clinician-members and real-life investigators by linking their work to that of our computer modelers. Both contingents will learn from this interaction. But all have a long way to go. There are more than enough research problems awaiting us at all levels of analysis.

While our uses of control theory have many roots in the past and many resemblances to the work of others, our approach is basically not connected to any mainstream line of development. It is a new departure, almost a reconstruction of behavioral theory from scratch. Some of us are convinced that it amounts to a revolution in the life sciences.

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Date: Wed, 21 Nov 90 07:43:06 -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: "sophisticated" control finale

One more comment on "sophisticated" control theorists and then a segue into another topic. I think that the "sophisticated" control theorists should be recognized for having done some great work. After all, they did recognize certain kinds of behavior as examples of control. They also recognized that these behaviors required an explanation in terms of control theory. When Henry Jex, the "sophisticated" control theorist who I met at the Human Factors meeting, asked how my ideas differed from those of conventional manual control theorists I said -- they don't (am I politic or what). What I said (while thinking on my feet and I still agree with me) is that the "sophisticated" control theorists just didn't go far enough.

They gave up too soon to the "real psychologists". They didn't realize that the control model was a model of ALL behavior (at least all intentional behavior) not just manual tracking. People control the relationship between cursors and targets, cars and roads, altimeters and numbers; but they also control relationships between themselves and their spouses, lovers (hopefully the same as the former -- true in my case) and children. And these relationships are not just difference relationships but power relationships, affection relationships, intellectual relationships, political relationships, etc.. People control simple variables (like distances between lines) and complex ones (like distances between political positions). It's the same thing, it is just more complex to work out the mathematics of the latter kinds of relationships because it is harder to quantify the variables involved. And the variables that are hard to quantify are not the outputs variables (muscles movements, etc) but the perceptual variables (how do you measure the perception of "lovingness" of a relationship?).

This is the segue into the next topic. If a "sophisticated" control theorist had tried to build a model of a person controlling, say, the quality of their educational experience, they would immediately be struck by the problem of figuring out how to get a measure of "quality of educational experience" to compare to a reference level of educational experience. They would then realize that what they must design is a way to perceive a variable called "quality of educational experience". Then they would realize that it is this perceptual variable, and not anything in the "real world" (whatever that is), that is being controlled. Then they would realize that they would have to have the system produce outputs that affect this variable. But those outputs must eventually be things that people can do -- which is produce neural impulses. Somehow these neural impulses must produce the muscle tensions that influence variables such as eye movements, book placements, questions asking and what not that influence the "quality of educational experience" variable. They would also have to realize that people can't just produce educational "outputs" because the world "out there" is always changing; there are disturbances. These disturbances, plus the effects of the efferent neural impulses, are what determine the value of the variable "quality of educational experience". So a person can't just produce the desired levels of educational outputs; only the desired levels of educational inputs. And, lo and behold, we are where Bill Powers got 30 years ago. Behaviors (efferent neural impulses) are the means by which organisms control (often very complex) perceptual input variables. To do so they must follow all the dynamic laws of control -- the laws that "sophisticated" control theorists understand -- but they do so, virtually always, with no training in diffeq.

Finally, in the holiday spirit,I would like to lift my glass and give thanks to the person who single-handedly discovered the nature of human nature. Thank you, Bill Powers, for the enlightenment and the years of fun finding out lovely little facts about a phenomenon that no one else seems to want to study -- purpose. Have a great vacation everyone.

Love

Rick

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Date: Wed, 21 Nov 90 12:16:45 -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

(BREWER_Bill)w-brewer@uiuc.edu
From: (Gary A. Cziko) g-cziko@uiuc.edu
Subject: Brewer vs. Conditioning

On the way back from our meeting last August in Indiana, PA, Clark McPhail mentioned a paper on conditioning by Bill Brewer of our campus here which provides a "massive reanalysis of the conditioning literature" and concludes that there is no convincing evidence for conditioning for humans.

I feel that this is essential reading for all control theorists and so provide the full reference here:

Brewer, William F. (1974). There is no convincing evidence for operant or classical conditioning in adult humans. In Walter B. Weimer & David S. Palermo (Eds.), _Cognition and symbolic processes_ (pp. 1-42). Hillsdale, NJ: Erlbaum.

Brewer concludes that the conditioning findings cannot be explained using concepts from S-R psychology but instead must include concepts from cognitive psychology. But I have seen on this network suggestions that mainstream cognitive psychology is not fundamentally different from the S-R perspective.

This article would provide the basis for lots of interesting discussion on CSG-L. Perhaps Bill Brewer would be willing to participate as well.--Gary

P. S. to Bill (Brewer). I will let you know if this sparks any interest. I will forward to you relevant comments and can forward any of yours to our network CSG-L.

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Date: Wed, 21 Nov 90 10:46:06 -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: Conditioning

Gary--
Great idea. I'll try to get hold of the book with Brewer's article. Sounds like a very strong claim -- no operant or classical conditioning in people. I just wonder if there is any evidence for it in any other organisms.

Actually, I think that we will have to discriminate the phenomenon of conditioning from the explanation. I believe there is "looks like" rather strong evidence that people and other organisms exhibit behavior that is classical and operant conditioning. It's just that what is really going on is just controlling (of course). But let's start the discussion. Maybe this topic will bring Dennis Delprato back into the fray. By the way Dennis, I sent a resume to the folks at Reno. I'd love to talk control theory with t them -- job or no. Have you talked to them lately?

Hasta Luego

Rick M.

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Date: Wed, 21 Nov 90 16:21:44 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: Conditioning

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

WARNING!! The Brewer (1974) chapter entitled "There is no convincing evidence for operant or classical conditioning in adult humans" may be harmful to the advancement of behavioral control systems science. This material was written at the tail end of the Great Awareness Controversy (recall "learning without awareness") that received a death blow when Bandura floundered all over the place with it in his 1969 book. I am not sure that anyone came out and said that the very question was fallacious, but thankfully gradually we became less and less subject to it. The chapter also appeared in the midst of many theorists' attempts to shed inept mechanistic accounts in favor of equally inept mechanical-cognitive constructions.

I don't know who reviewed the chapter (editors were said to be Weimer and Palermo). However, the title is sheer nonsense--it is especially nonsensical given that early in the chapter, the author makes a distinction between "conditioning" (pretty much defined in terms of experimental procedures and results) and "conditioning theory" (said to be the position that conditioning effects are automatic and unconscious). The entire point of the chapter is simply to argue for an alternative "theory" of conditioning effects. This alternative takes the causes of conditioning effects to be "higher mental processes," "awareness," "expectations," "conscious hypotheses," and the like. Thus, there is much in this chapter that control theory finds unsatisfactory: lineal causality, input-processing-output, mind-body dualism. The author takes a justifiable point (i.e., simple mechanical descriptions are inadequate for describing the events of so-called conditioning experiments), and uses this as the basis for good, old-fashioned cognitive- mediation theory. Recall Woodworth's S-->O-->R, little rg & sg, and information processing?

In my opinion, control theorists and researchers can better spend their time developing and testing models that actually produce verifiable results instead of going back to the approach to research that leads to the never-ending disputes that psychology still thrives on.

However, do look for yourself and evaluate my assessment. And, Clark, Certainly G. H. Mead would not condone the approach to mind we see in Brewer--would he?

Dennis Delprato

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Date: Wed, 21 Nov 90 16:12:46 CST
From: Gene Boggess <gboggess@CS.MSSTATE.EDU>
Subject: Introduction

Hmmm .... I subscribed to this list at the recommendation of someone on the CYB-SYS list, and also, as someone else said, feel as if I am eavesdropping on someone else's conversation. I am currently an Asst. Prof.

of Computer Science at Mississippi State University but am a Communication Theorist by training (PhD at U of IL at C-U, 1981) with Master's degrees in Computer Science and Linguistics and a double-major BA in English and Philosophy. I first read something by Powers in 1973; there was an article by him as one of the chapters in the Speech 101 textbook I was teaching from. It was the best chapter in the book, and I wondered who the heck this guy was, but never followed up on my curiosity.

While at the U of Illinois I studied as much Psycholinguistics as I could, including a course in Developmental Psycholinguistics taught by Bill Brewer and Howard Maclay and another Psycholinguistics course taught by Brewer alone. I remember reading the "no convincing evidence for classical conditioning" paper recently mentioned in this list shortly before (or after?) it was published, but it did not make a big impression because no one believed in Behaviorism any more anyway, at leastat that time; we were all committed Chomskyian Rationalists. Now, of course, with the progress made in Connectionism and Neural Networks the pendulum is beginning to swing back, as it always does. I suspect that the work of Charles Osgood (from whom I also took a seminar at the U of I), the "last dinosaur" of the Bahaviorists, will be reassessed as presaging the Connectionists - his work on his 3-stage mediation theory (published by Mouton in several volumes, I believe) seems quite close to a Connectionist explanation of behavior.

This list seems to be working at a much higher level of abstraction than that, so I am not sure how much this all ties in, but since you asked . . . . As for my own research, I am just getting started, after a varied and interesting career as an English teacher, a computer consultant, and a househusband (while my wife got _her_ career started). I am interested in Neural Networks, but find that I am thinking about its philosophical ramifications too much and not working on getting sophisticated computer programs running, so I'm not sure I fit in too well here. On the other hand, I'm teaching massive numbers of students each semester, so they can't afford to fire me - yet!

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Date: Wed, 21 Nov 90 18:52:16 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: Comments on comments

A bit of a tear in the eye today as Mary looked over all those mementos from her friends.

Marken Effect. Years ago, I think, somebody found that joysticks are easier to use if they have a modest centering spring in them. The opinion then, which I saw no reason to dispute, was that this biased the muscles into a higher-slope part of their operating curve. Clearly the Marken Effect disposes of that explanation, because the forces exerted by the hand on the handle aren't altered at all by the other control system. But the other control system does act something like a centering spring.

Wayne, I don't think that Rick has explained (recently) that in the first of the two cases, there is an active control system opposing the person, but in the second, that control system is missing: only a disturbance, a recording of the former control system's output, is present. The person experiences the same disturbance in both cases, but there is a second active control system present only in the first case.

Rick Marken: let's leave the door wide open for sophisticated control system theorists (no quotes). They could help a lot.

Manifesto: It was fun to write for the already-converted, but whether it will be useful to others remains to be seen. It might just get them mad.

Operant Conditioning: I basically sided with Brewer when I saw the article saying that there weren't no such thing as operant conditioning, but I thought his reasons could be improved (and as Dennis Delprato points out, using the "cognitive" explanation instead doesn't do the job).

I have no quarrel with "operant," obviously. It's the "conditioning" part that I reject. That is because I don't believe that environmental objects such as food pellets have any effect on an organism other than their ordinary sensory effects and their physiological effects (nutrition). The reinforcing effect on behavior is strictly imaginary -- nobody has ever observed such an effect passing from the reinforcer into the organism. If you dismiss reinforcement as imaginary, where does that leave the idea of conditioning? All that's left is a phenomenon that requires an explanation, which I agree that control theory provides.

How about one of you psychologists writing an article on "Behaviorism as Modern Animism?" They put all the required purposiveness and intelligence into the environment, don't they? Even though they're reputed to be dead.

Gene Boggess: welcome, and don't go away. We share the labor here: those who program do so. The programmers will keep you honest by insisting on a description complete enough to program. You can help keep them honest by making sure they're programming models of things that actually happen.

Next time I'll try to have a coherent report on a productive two days with Hugh Gibbons (for newcomers: an extremely sharp law professor who uses control theory to explain where law comes from -- so be careful what you say).

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Date: Thu, 22 Nov 90 21:32:38 U
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Mark Nelson <mark@CS.UWA.OZ.AU>
Subject: Re: Control of perception and adaptation
In-Reply-To: Your message of Tue, 20 Nov 90 08:19:08 CST.

I am a novice at Control Theory so some of my terminology may be confusing, but
here goes... :-)

Does this group have a precise definition for ADAPTIVE BEHAVIOUR?

First some of my background to this question.

I have been developing a computer simulated mobile robot navigational control system. One of the dimensions of this control architecture is hierarchical where each level is an augmentation of the lower levels and corresponds to a distinct navigational behaviour or competence.

A navigational behaviour is embodied in a control structure that is designed to
be capable of achieving a particular class of tasks (i.e. navigational goals) such as: to remain at rest, move a certain distance in a randomly chosen direction, approach a particular object, go to a previously known place, etc.

The purpose of a navigational behaviour is to attain the associated goals while
maintaining the survival of the robot. The navigational goal represents the reference condition of the associated navigational behaviour. Maintaining survival means that the robot must not collide with any stationary or moving objects. These are sub-goals which are the reference conditions for components
of the control system called sub-behaviors.

Now with this in mind I have defined the following:

REACTIVE BEHAVIOUR is exhibited by the robot when the control architecture of a particular navigational behaviour is able to satisfactorily perform a specified task. That is, the responses generated by that behaviour satisfy the constraints imposed by the environment upon robot. A typical constraint is the maximum reaction time allowed for the robot before an object which is rapidly approaching the robot collides with it.

But what if a behaviour fails? That is the control system is unable to produce a response which satisfies the constraints in a given situation. An example is when the control system is unable to meet the constraint upon its
reaction time because the type of perceptions and decision making computations inherent in that level of navigational behaviour are too time consuming. Higher
levels of control by definition generate responses slower than simpler, lower level control behaviors.

When a behaviour fails in my control architecture the lower levels of control begin to have an influence upon robot behaviour. The type of responses that are characteristic of lower navigational behaviors are simpler and quicker to generate. Therefore,

ADAPTIVE BEHAVIOUR is exhibited by the robot when the task level behaviour fails and lower level control behaviors have an influence upon robot motion. Therefore adaptive behaviour is produced by possibly several navigational behaviors interacting with each other within the control structure.

The important aspect of adaptation is that the ways in which the robot is reacting to its environment are changed. The nature of reactive behaviour is adapted in an attempt to enable a response to be generated that will satisfy the
environmental constraints.

This type of adaptation causes simpler perceptual processes to become active which thereby causes simpler decision making processes to activate and process the perceptual data resulting in a motion command which is then executed. This
form of adaptation seems to be comparable to what Bill Powers seems to be describing in the following passage:

> ... In order to change the
> KIND of perception being controlled, you have to send a reference signal to
> a different control system that already has an input function of that kind,
> and stop sending one to the old system. ...

Mark Nelson
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Date: Thu, 22 Nov 90 11:07:58 EST

From: BARKANA@DUPR.OCS.DREXEL.EDU
Subject: Engineering and living control systems

Many thanks to Wayne Hershberger and Tom Bourbon for the welcome.
It is not easy to keep quiet, in such an active environment, though I think I must do a lot of reading and listening to you, before I even understand you. I feel like falling in the middle of a discussion and trying to tell people my opinion on things that I didn't even hear.

To Bill Powers and actually to all: I am asking more that claiming. I am not sure I can agree with the apparent contradiction between the engineering diagrams and living control systems. Or, I would better say that I do not understand it. If the problem is driving a car, the input is the way, the trajectory that must be maintained, and the output is the position of the car. Of course, this difference must be measured, and the control system only receives the output of the sensor that measures this difference. In ideal situations, this measure is exact. In other conditions it has noise, bias,
miscalibrations, phase lag ("time-constant") or transport lag ("pure delay"). The control system tries to bring the error signal to zero, and the output is the position of the effector ("actuator").
Between the sensor and the effector (motor) there is a controller that transforms the signal in such a way that stability and performance of the control system is guaranteed. And this is only the simplest control system. If a "brain" is involved, the signal that is transmitted to the effectors can take more sophisticated forms: the brain may know the performance of the control system, may be capable of taking into account its time lags, etc. Furthermore, the brain has stored the final aim of the trip, and may change the root or take other decisions, that could not be taken by a simple autopilot whose only purpose is to keep in line. But I think there must be some separation of the various tasks. And even here, the final point is stored in the brain, only because some real final point is there in the real world, and this is what we call "input," even though the control system can only affect the output of its own sensors, or to its perception of the real world.
If the temperature must be maintained, the input can be considered internal, because it starts in the brain. Still, this signal is transmitted to a control system whose function is to execute and reach this temperature, or to annihilate the difference between the desired temperature, (registered in the brain?) and the temperature of the body. For this control system (or better, regulation system) the input is external. I don't understand how the living control system affects its inputs. May be only a difference in definition? In a tracking system, the position of a target is the input, the resulting position, of the eye, for example is the output, even if the only physical and measured signal is the difference between these two values.
I agree with everything I can claim I understand in Powers Manifesto and the second part of Bill Powers Introductory draft seems to say just that, so may be I miss the main point here.I would appreciate if you could open my eyes here, because I am trying to understand, not to prove that I am right.

To Rick Marken: May be the engineering control people need other tools because they must design the control system, not only understand it. The "sophisticated" control people use lots of Math, because
of the difficult task of proving that a system is stable. Not because they are crazy about stability, but because it is easy to get unstable system with an "ingenious" and "intuitive" control method. When the control becomes nonstationary and nonlinear, such as in adaptive systems, the problems and the proofs are even more difficult. The problem is that if you do not know (prove) that an adaptive system (I mean "engineering" adaptive mechanism) is stable, in
general you discover that it is unstable under these conditions or others. I don't know how much this group is interested or how much time it has to spend on this stuff, unless people are interested in the instability

mechanism of pathological cases .Please, see also the lines on the car driver above. Of course the organism only receives the signal that is supplied by the sensors, but it is more or less the measure of the external signal.
By the way, besides dealing with theories of systems, I am also an engineer, and I can tell you that no engineer would let a motor run, much less a plane fly, without thousands of simulations, no matter what the theory says, and in fact the theory, the complex functions, differential equations, etc., do not say much when a real, large complex system is involved. And I would not dare to compare any complex plane with a living organism, not even mentioning an intelligent creature. So, your learning through observation and simulation is a main engineering tool. But when I want to DESIGN a stable system, and a well-behaved system too, I need mathematical tools that express stability and performance, and their dependence on the various parameters that I may or may not change. And then things start getting tough, like trying to define pornography: It is hard to define, it is easy to recognize when you see it.
Yet, I usually need the differential equations to have reliable simulations, especially if I want to discover when the real plant stops performing satisfactorily. It is not that important whether your simulations are state of the art or not, as long as they are correct and approximate the real thing. I don't know your models, so I hope they are.

To Tom Bourbon: At this stage, I am afraid I can only tell you that the problem is interesting, and that I only start studying it. It is not as much an adaptation problem as it is a learning problem. How to guarantee that a mechanism learns while it performs its task and maintains a stable behavior is not an easy task!I will try to be more specific in future letters. In fact, part of the new trend in "intelligent (automatic) control" tries to eliminate the diff. equations because "the brain does not solve diff. eqns," and tries to imitate the brain, and the algorithm used are just (poor) attempts to reproduce the activities of organism's neural networks.

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Date: Fri, 23 Nov 90 01:24:27 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti1@VUB.VUB.AC.BE>
Subject: Brooks' and Powers' control hierarchies

Mark Nelson:
I am very interested in your work. I mentioned the subsumption architecture of R. Brooks in CSG-L before. In fact, It's one of the research areas in my lab. I have thought about how to integrate Powers' and Brooks' works together. That's why I want to know more details.

Bill Powers talked about the difference between his control hierarchy and others. He said:

>In my hierarchy, higher systems are physically distinct from lower ones and
>>do not work with the same input variables. Instead, the input functions of
>higher systems receive copies of the signals in the input functions of
>lower systems, and apply some typical transformation to them to create new
>signals that represent a different TYPE of variable (so that a collection
>of configuration signals is re-perceived in terms of derivatives, or a

>collection of objects/transitions/events is re-perceived as the state of a
>relationship). Furthermore, every level in my hierarchy is also a level of
>control: there are comparators and output functions at every level. The
>>output functions act not on the external world, but by adjusting the
>>reference signals for systems of the next lower level. The SAME systems
>from which copies of input signals come.
>
>One last difference from other hierarchical models (like Brooks'). I've
>tried to use neurological information as much as possible, and to define
>
>levels that seem possible to find in ordinary human experience. Many other
>hierarchical approaches are more like ad-hoc inventions, organizations put
>together to achieve some immediate purpose without the constraint of
>achieving it the way a living system does. I'm not basically interested in
>robotics, although it can be fun. I'm interested in how human beings and
>other organisms work. For me, the constraint is always to figure out how
>the real system achieves a given behavior, not just to find ANY way of
>achieving it.

From what Bill said, Bill's and Brooks' are very different.
For example, brooks' levels are defined according to the difficulty of
behaviors. But Bill's levels are defined according to the difficulty of
the control functions. In fact, I can imagine that each Brooks' level
corresponds to the whole hierarchy of Bill. Of course, Brooks don't
implement each of his levels using Bill's hierarchy.

How you integrate them together?

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Date: Fri, 23 Nov 90 01:55:45 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: Reply to Bar-Kana

Izhak Bar-Kana: I think that the mental model you are using is the one
traditionally given in engineering texts, the same one that Norbert Wiener
picked up and used in his first book on cybernetics. In that model, "input"
means REFERENCE input. It is shown, usually, entering the comparator as if
from the external world. The feedback signal, on the other hand, is just
"picked off" the output variable through some feedback transducer.

In the model we use in the CSG, when we say "input" we mean the sensory
feedback input, not the reference signal. That is because the sensory
inputs in the organism constitute the "feedback pickoff" that reports, as
analogue signals, the states of external controlled variables. The senses
do not report the intended or desired state of affairs; only the current
actual state of affairs. The reference signal comes not from outside but
from systems super-ordinate to the control system in question, inside the
organism.

So it comes down to how we match the main functions and signals in a
generic control system to corresponding functions and signals in a
particular control subsystem in the organism. The abstract organization is
the same; our model has the same connectivity as the one I believe you are
using, so the control-system analysis itself is unchanged. But the meanings

and the implications are greatly changed.

Apply this to a model of driving a car. The driver sees the current position of the car relative (laterally) to the road. Out of all the information in this image, the brain extracts a position signal that varies as the car moves from side to side. Thus the position of the car is the INPUT variable, not the output variable, in the steering control system. The position signal is compared with another signal that specifies the intended state of the position signal: that, of course, is the reference signal. The driver may select any possible perceivable position on (or off) the road as the reference position. The error signal, reference minus sensory signal, actuates the output of the control system, which is the torque applied by the arm muscles to the steering wheel (this requires two phase advances for stability). That torque is the last variable in the output chain that is due strictly to activities in the brain. From there on, we have mechanical linkages and external disturbances coming into play, which alter and add to the effects of the output and are not themselves part of the behaving system. The result is some position of the car on the road, and thus a state of the perceptual signal representing that position. The feedback effect keeps the perceptual signal in a match with the reference signal (give or take dynamic and static errors). It is not necessary for the brain to contain any detailed knowledge of physical properties and events outside itself other than the controlled variable. Variations in output properties have little effect; disturbances are automatically counteracted without any need to anticipate or sense them (except through their effects on the perceptual signal).

With the reference signal moved inside the control system, we can now "parse" complex behaviors in a new way. In order to alter the position of the car on the road, the brain now needs only to alter the reference signal for the steering system that is now in place. To pass a car, higher systems concerned with relationships to other objects change the reference signal enough to move the car to the other lane, keep it there a while, then move it back. Of course there is also a speed control system operating

independently, with its own input that senses speed and its own actuator that affects speed (the foot on the accelerator pedal). The "passing another car" system alters the reference signal for speed, too, as the driver passes the other car, first increasing it, then decreasing it again. So the higher system uses the lower systems by manipulating their reference signals, just as a human user manipulates an artificial control system by turning the knob that changes its set point.

At the same time this is going on, the driver can use one arm and hand to reach out and change the volume on the car radio, then scratch his neck, all while telling a joke to the passenger. In this model there are many control systems acting concurrently, each controlling just one (perhaps complicated) variable.

There are neuro-anatomical justifications for breaking down behavior into multiple control systems operating independently and in parallel, and organized into levels of control. And I think this picture also helps us to approach the modeling of complex behavior in an orderly way, solving problems of peripheral control to serve as the foundation for exploring systems that are of higher level, more central in the nervous system. We can, of course, pick isolated systems of any level and analyze them as control systems, absorbing lower-level control system properties into their output functions. But the final model must spell out all the stages of control that exist, while, one hopes, maintaining such correspondence to known structures in the nervous system as we know about.

The same model applies to human temperature control. The input variable is the temperature of a sensory ending (in the hypothalamus, I think). The reference signal is variable, as temperature can be maintained actively

anywhere between 98 deg F and about 104 or 105 deg F. I don't know what varies the reference signal, although we know it changes when you get sick. The error signal is translated into shivering and peripheral vasoconstriction if it is positive (sensed temperature lower than reference temperature) and into sweating and peripheral vasodilation if negative: that is the behavior that affects the input, the sensed temperature.

As for simulations: we use them a great deal, where we know how to construct them. They work very well. Stabilization has not yet proven to be a problem, although in the arm model you have seen mentioned the problem was solved just by introducing known properties of the neuromuscular systems in question (we never set foot on a complex plane). I think that the hierarchical structure simplifies stabilization problems, which may be an indication (and may not) of why the whole system is organized that way. We haven't got very far with modeling very complex or high-level behaviors. We're still taking baby steps and learning how to walk. But I think that our approach, probably combined with some of the perceptual models being developed by neural network people, will carry us a good deal further before we have to change the basic structure of the model.

Chen: maybe this exposition will help you in comparing our model with that of Brooks. Please continue.

Mark Nelson in OZ: Maybe thinking in terms of controlled variables instead of tasks will help in your modeling, at least at the lower levels. Adaptive behavior is an important subject that we in the CSG haven't done much with.

We will be watching for your results!

Best -- Bill Powers

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

On Connectionism:

With a number of people recently mentioning connectionism, I wonder how control theorists are any less connectionist than the connectionists.

The control theory models seem to me to be about as connected as you can get, but are closed-loop instead of open-loop and put the reference signal inside the system. The connectionist models are impressive to some in that they can do some interesting things without having a central executive watching over the whole system. But anybody who has seen Bill's (Powers) little man arm demo would see the same thing--coherent, coordinated, purposeful behavior with no central executive (i.e., no little man inside the little man).

Isn't there a way that control theorists can exploit the current popularity of connectionism by showing it's advantages and disadvantages and how the latter can be addressed by closing the loop and putting reference levels inside the system?

On Computer Demos:

Bill (Powers), Your computer demos have been mentioned a couple of times now. Isn't it time for you to put together a little announcement on what is available and how to get them? While you may be reluctant to advertise

your own wares on the net, I think you should anyway.

Rick (Marken): How about your stuff for the Mac. Do you have a package for distribution you can make available our subscribers?

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <arti1!chen@VUB.UUCP>
Subject: Re: Connectionism & Demos

A good idea! In fact, as I suggested before, I want to apply the control theory to pattern recognition. If the control theory is right to explain our brain, of course it should have good applications. Robotics is also one possibility.
Almost everything can be regarded as connectionist networks, as long as they are connected by some units.
I am also interested in seeing some demos.

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Date: Sun, 25 Nov 90 09:28:36 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: Dynamics of Hierarchical Systems: Nicolis

Chen:
I finally got hold of "Dynamics of hierarchical systems" (DHS) by Nicolis. I will try to explain why I don't think it has anything interesting to say about human behavior. I don't follow very much of the mathematics, but that's partly because I haven't really tried. It's hard for me to summon up the effort when I can't see what the math is supposed to accomplish. It's even harder when I see that the math is meant to describe a conception of an organization that I think is imaginary, fitting no real organism. That proves that I am not a mathematician. A real mathematician doesn't care about semantics.

My interest is in understanding the organization of living systems. To me, this means starting with phenomena of behavior and what we know of function and structure, then finding mathematical ways to represent the phenomena in the context of a model that simulates as many of the functions and structures as we can handle. Above all it means defining phenomena and making sure they are real; the mathematics and the simulations are only means toward making sense of phenomena we actually observe.

Nicolis doesn't seem very interested in making sure he is analyzing real phenomena. In DHS, abstract form is the central topic: "By a hierarchical

system we mean, in general, an ensemble of interacting parts which is composed of (and is analysable or decomposable into) successively nested sets of interacting subunits." A little later he says "Therefore, as we move to a higher level, we usually witness a tremendous reduction in the number of degrees of freedom. The higher level receives selective information 'from below', and in turn it exercises (efferent) feedforward control commands on the dynamics of the lower level."

The sloppy writing alone bothers me, but what bothers me more is the casual way in which vastly important premises are laid down without the slightest empirical justification. When Nicolis says "we usually witness" the reduction in degrees of freedom, he doesn't mean "in real organisms": he means "in the kinds of mathematical conceptualizations presented in this book." Rather than starting with examples from real experience and justifying the premises, he simply states his axioms at the same level of abstraction as the analysis that is to follow. This means that all his manipulations are going to be fundamentally empty of content. It means, too, that he continually risks contradicting empirical evidence.

Consider language. One natural "level" in a hierarchy of language perception is the perception of basic auditory forms: phonemes. The English language has, I believe, something like 64 phonemes in its spoken form, plus modifications that depend on context. Given these degrees of freedom at this level in the hierarchy, how many degrees of freedom can there be in a higher system that treats the phonemes as words and the words as sentences and the sentences as structures of meanings and the meanings as a coherent world view (such as physics)? Is there truly a "tremendous reduction in the number of degrees of freedom?" Obviously just the opposite is true: there is a tremendous explosion in the number of degrees of freedom. The number of sentences we can form from those phonemes (or in written form, from the alphabet) is probably beyond the first order of infinity. And the number of conceptual schemes we could concoct is probably even greater than that.

That is because higher levels introduce not just new combinations of the phonemes, but transitions from one to another, relationships among both the elements and their transitions, categories created by forming sets and symbols, sequences of symbols, networks of contingencies (programs and logic that manipulate sequences of symbols), principles, and contexts (system concepts). As each new level is added, the dimensionality of experience is increased by introducing basically new considerations, new points of view that put the preceding levels into a higher-level context. These levels can be identified in experience; we can do experiments to show that control of variables of these different but hierarchically-related logical types does in fact occur. It doesn't matter that we have no mathematical ways of analyzing all of these levels: presumably, that is what we are working toward. What does matter is that we have empirical reasons for proposing these levels, reasons that will give content to whatever mathematical forms we find that approximate what the real system is doing. So I have good reason to believe that some, at least, of Nicolis' first premises are contrary to observation.

A bit later, Nicolis is talking about " Structural hierarchy theory" which is also known as "complexity theory." Of this theory, he says "Specifically, it aims at reducing the quadratically (N^2) rising number of switching elements as a function of the input-output number N needed to perform a given task to the theoretical minimum N log (N)."

So we discover through a passing reference that the system he is talking about is not an analogue system, but a switching system. How does he defend the idea that only switching systems are of interest? He doesn't: it's a foregone conclusion, just like the one contained in the previous casual reference to "feedforward control commands", which also implies one narrow

class of models. The narrow class is narrowed further.

The discussion so far takes us almost through the first page of the Introduction. Perhaps you can see why I did not read the rest of the book as a student would, following each argument, learning the mathematics needed to understand, and so on. Underneath the arguments in this book lies a large collection of assumptions that cry out for empirical justification, but they are treated as self-evident. This is made clear repeatedly throughout the book. The effect on restricting the universe of discourse to one imaginary kind of system is invisible unless you stop and think about it, and ask why Nicolis chose these unspoken premises, and how he defends them with empirical observations. When you do that, you realize that he is setting up an enormous straw man.

Nicolis is arguing as a physicist, although he is in a different field. I am disturbed by an underlying attitude that seems to pervade writings about living systems that come from physicists. There seems to be an underlying assumption that physics provides all the relevant observations we need, all the laws of form, all the generalizations. There's a habit of glossing over details in order to reduce problems to forms that are familiar to physicists. This often entails drawing analogies that even a physicist ought to know are invalid, such as an analogies between quantum processes and processes that take place in a brain on a far, far, larger scale where quantum effects just aren't seen. Prigogine invokes thermodynamics as if it applied to systems with great internal complexity just as it does to collections of simple homogeneous particles. Instead of laying down a firm

empirical foundation and then searching for an adequate mathematical representation of observations, physicists outside their own fields tend to treat the mathematical abstractions as the primary reality, ruthlessly chopping away or simply not looking for observations that don't fit the theory. They try to fit living systems into the forms they have already developed, rather than using phenomena of life as hints toward developing new forms. They have turned the tool of mathematics into a religious ritual. I sometimes wonder how this has affected even their approach to their own subject matter. Quantum physics verges more and more on the mystical, a fact that the mystics haven't missed. Something is upside down here.

In the mathematical world that Nicolis inhabits, control theory is pretty simple stuff. But control theory isn't what we're about: we're exploring a phenomenon, and control theory is just a way of dealing with it. I wish people like Nicolis would spend more of their time thinking about phenomena, and less trying to prove that the mathematical forms that physicists use provide all the information and data that we need.

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Dynamics of Hierarchical Systems: Nicolis
In-Reply-To: Message from "Bill Powers" of Nov 25, 90 at 9:28 am

Something else about Nicolis' approach, and that of other currently "orthodox" self-organizational types, is that, whether they admit it or not, their methods are essentially aimed at phyiscal, pre-biotic, thermodynamic systems. That's Prigogine's whole legacy, and Nicolis is his fundamental collaborator. Analyses at those levels are still underdeveloped, and purely metaphorical when applied to higher levels.

The best current work I'm aware of on hierarchy proper is: Pierre Auger,

Dynamics and Thermodynamics in Hierarchically Organized Systems: Applications in Physics, Biology, and Economics, Pergamon, Oxford, 1989. He's attempting fundamental, yet accurate, formalisms for describing hierarchy in general. Anyone out there read it?

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

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Date: Mon, 26 Nov 90 11:00:36 -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: Darwin's Hammer

Bill (Powers):

I hesitate to start up on evolution again, but it is important to me, particularly since my book is using it as a model to explain all "puzzles of fit" and all knowledge processes, in the tradition of Donald T. Campbell. Perhaps we can keep the discussion limited to evolution without the cosmology and metaphysics.

In your 22 October posting on the origins of life, you wrote:

> 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).

While I do appreciate the point you are making in this posting and feel it is very important, I nonetheless detect a bit of "sleight-of-hammer." When you write: "A species doesn't have to go extinct if it mutates the wrong way; it mutates again right away," it gives the impression that the hammer is not needed. But, of course, individuals who mutate "the wrong way" are hammered (sorry, but that's the real world for you). Organisms with "better" control systems are predominant only because those with "worse" ones are squashed. The application of control theory to evolution suggests that the hammer may not have to be used as often as previously thought. If things are stable and the organisms are in control, there will be less mutations and therefore less hammering. But it doesn't lessen the prime importance of the hammer. If there is better fit, it must be because what fit less well has been eliminated, not because species know what mutations will be more effective in providing better control systems.

What is true, however, is that more advanced organisms can obtain increases in knowledge without getting themselves hammered by using *vicarious* trial and error (or "internal" selection as you mentioned and attributed to Rick Marken). As Popper has noted, a amoeba will usually get hammered if its "theory" about what is noxious and what is nutritious is wrong while people can propose and test theories vicariously without putting their lives on the line (although sometimes they do) as in feeding saccharin to rats to see what happens. Nonetheless, it is still the hammer which eliminates the less fit theories, even if there is less physical squashing going on.

Of course, the hammer itself is a bit of a simplification. Living to a ripe old age and producing hundreds of offspring doesn't look like getting hammered, but it eventually amounts to the same thing if other individuals

are having thousands of offspring.

In fact, I like the image of Darwin's hammer so well that it may well make it into the title of my book, unless you can convince me that increases in adaptive complexity are possible without it.--Gary

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Date: Mon, 26 Nov 90 10:20:36 -0800
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From: marken@AEROSPACE.AERO.ORG
Subject: Various replies

Wow, you folks really know how to spend a vacation. I am writing this on Monday and there was a ton of mail in my box -- as expected. Let me try to quickly reply to the stuff to me.

Bill Powers -- Of course the door is wide open to sophisticated (better, perhaps, engineering) control theorists. As I said in one of my posts, I think they have done some interesting stuff and should be acknowledged for the work and discoveries they have already made about control. What I meant to say was that I think engineering control theorists probably think that we should move into their "state of the art" camp rather than vice versa. The door is definitely open but I think they expect us to do the walking.

As you noted in one of your posts, we are more interested in phenomena than formalisms (although the latter are important and we certainly do do modeling).
The phenomenon we are interested in is control. And what we see is that organisms control variables -- not vice versa. The most important explanatory concept in our model is the reference signal -- a neural signal inside of the organism that specifies the desired states of perceptual variables. I think the importance of this concept is very difficult for engineering control theorists to understand. I submit that this is because it takes control away from the observer and gives it to the control system. I submit that this is one of the main reasons for the difficulty most people have in accepting your version of control theory. Moving the reference signal into the organism moves control from the environment to the organism. Engineering psychologists (like other psychologists) have a vested interest in their ability to control people -- operators. They want to design displays and controls that cause "good performance". The same is true for all psychologists -- it is difficult for them to accept models of behavior that deprive them of control. And this is, of course, understandable since psychologists are control systems too and are disturbed by things that threaten their control of variables they care about -- behavior, for example.

I would love to have some engineering psychologists work with us; their skills would be most helpful. But I just don't think that they are any more likely to be interested in our work (simply because they already know control theory) than any other psychologist. That's what I meant to say. I will certainly work with the engineering control theorists I've met to help them understand what we are doing. But it will be just as difficult to get the point across with them (and get it accepted) as with any behaviorist
or connectionist or anyone else who is less familiar with control theory in the first place.

Izhak -- This is related to the above. I did not mean to give the impression that the math work of the engineering control theorists is unimportant. I think it's great. I know alot about stability from my efforts at building control systems -- including hierarchical control systems. As Bill mentioned, we usually don't have much problem with stability in our models inasmuch as they work. But you do have to mind your gains and lags and dynamics and I know how to do this pretty well. I even know some of the stability theorms. My goal was merely to say that complex math can often disguise (unintentionally) a lack of understanding of the phenomenon being modeled. Again, Bill mentioned how this can happen when he described some work by a physicist on hierarchical models of living systems. This point is important because some people are very impressed by the math and they assume that the people who use it know what they are doing. A related problem is that people who do the math tend to think of people like us, who do use simpler(but sufficient) math, as not being "state of the art" and, hence, not worth considering. Bill said that our control theory models probably seem pretty elementary compared to some of the stuff you see in "dynamic systems theorizing" and whatnot. This is unfortunate because, in my humble opinion, when it comes to understanding living control systems, we understand things MUCH better than those who do the fancy math. The most dramatic and fundamental example of this fact is the "mislabeling" of sensory inputs as reference inputs. Once again Bill discussed the significance of this "little" mistake. I think it is more than a little one--it's the whole enchilada.

Most of my work in control theory has been aimed at showing the importance of recognizing inner references as the cause of the behavior of living systems. Many of my computer experiments and demos are described in various publications. In response to Gary Cziko's query about whether these are available on a MAC disk -- well, yes. They all exist in varying degrees of informality (the programs are not always user friendly). But if there is some interest (well, even if there isn't) I will try to put together a little collection as object code that can be run w/o compiler or interpreter. I will definitely try to have some kind of package available by October for the CSG meeting.

One last little note -- over the weekend I read the chapter on manual control by Wickins (referred to in my ABS paper) to see what had been done on control aiding. I learned that the complex math stuff is not nearly as complex as I thought. I also learned that most of the user aiding schemes have been aimed at improving control of "higher order plants"; that is, systems where the plant output (our cursor position variable) is proportion to the integral (1st order) or double integral (2nd order) of operator output (our handle posiiton).
In most of our studies, the cursor position is proportional to handle position (zero order) and this generally produces the most accurate control. There was mention of the "spring loaded" system that Bill Powers talked about in an earlier post -- this seems the closest to what I did but I will look at the paper describing it. I did set up my tracking demo using the Marken effect user aiding system so that it does 0, 1st and 2nd order control. The aiding system improves control in all cases and it works the same for the control model with lag (where the model is connected to either 0, 1st or 2nd order system). So far, it looks like my user aiding system is the only one that improves performance in a tarcking task with a zero order output system. Still, more research to come.

Back to work

Regards

Rick M.

****

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Date: Mon, 26 Nov 90 07:17:13 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%arti1@VUB.VUB.AC.BE>
Subject: hierarchies

Cliff Joslyn:
The author of "Dynamics of hierarchical systems" is J. S. Nicolis.
His brother is G. Nicolis, who is the main collaborator of Prigogine.
Don't mix them together.
I read a little the book by Pierre Auger long time ago. It's a good
book.

Bill Powers:
I think you are right that the book is quite abstract. I remember its
aim is to analyse the systems in physics and biology, not specifically
for living systems.
About "reduction in degrees of freedom", I think it means that you can
use less parameters to describe a system from high-level point of view.
For example, you can use only temperature, pressure, and volume to
describe a thermodynamical system. You don't consider EACH particle in
the system. Likewise, when you describe the human behaviors, you don't
consider EACH neuron in your brain. You use only some parameters in
high-levels.

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: hierarchies
In-Reply-To: Message from "Chung-Chih Chen" of Nov 26, 90 at 7:17 am

> The author of "Dynamics of hierarchical systems" is J. S. Nicolis.
> His brother is G. Nicolis, who is the main collaborator of Prigogine.
> Don't mix them together.
> I read a little the book by Pierre Auger long time ago. It's a good
> book.

Yes, indeed. But I do understand that they are brothers. As it turns
out, I'm much less familiar w/JS Nicolis than G. Can anyone comment on
the degree of overlap, or if they collaborate? I wouldn't be surprised.

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

Date: Tue, 27 Nov 90 07:49:36 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: Hierachy

Chung-Chich Chen, Cliff Joslyn

Chen says "... you can use less parameters to describe a system from a high-level point of view."

I think Chen has shown me the key to understanding the difference between our approach to hierarchy and the one in Nicolis' book (and in many other writers' works). If I understand Chen correctly, Nicolis is talking about hierarchies of DESCRIPTION, whereas I am speaking about hierarchy IN THE DESIGN OF THE SYSTEM ITSELF. In my system, the "Sensation" level is not just a higher-level way of describing the signals at the "Intensity" level (signals that come directly from sensory endings). It is a separate layer of neurons that receives copies of the intensity signals, and computes signals that are new functions of subsets of the intensity signals.

If there are 1000 independent intensity signals, then there could be as many different sensation signals (with different meanings) as there are ways of making weighted sums of subsets of the 1000 intensity signals. The actual number is limited by the number of neural computers actually operating in parallel at the second level. The degrees of freedom are still limited by the number of independent first-level signals (which is really much greater than 1000), but in different contexts one could have different subsets of first-order signals being represented as weighted sums at the second level. Within each subset the maximum number of independent signals is set by the degrees of freedom, but still higher level systems can control by using one subset of perceptions or another: as a simple example, you might control hand position on one occasion in a roughly x-y-z coordinate system, and on another occasion in a rho-theta-phi coordinate system (depending on external constraints).

I suppose that technically the degrees-of-freedom limit is absolute, but since we begin with millions of degrees of freedom it isn't a practical limit. And because we can independently control perceptions made of orthogonal subsets of lower-level perceptions, there is no "drastic" reduction in degrees of freedom in going up a level. In principle there need be no reduction at all, although of course there can't be an increase.

Cliff, until I can get hold of Auger, how about a summary of what he says about hierarchy?

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Hierachy
In-Reply-To: Message from "Bill Powers" of Nov 27, 90 at 7:49 am

> Cliff, until I can get hold of Auger, how about a summary of what he says
> about hierarchy?

Auger's work is really quite fundamental, and I've been surprised that it hadn't been done previously. (Although I should know that I'm surprised a lot that way). Apparently since the initial flurry of "hierarchy theory" around the time of Pattee's anthology /Hierarchy Theory/ it has been used much more metaphorically than rigorously. Stan

Salthe is another doing current good work in hierarchy, but they are the only ones I'm aware of.

Auger's approach is very simple: let us be given an aggregate system whose dynamics is known, and a division of that system into at least two subsystems. Then the equations describing the "new" whole consisting of the unity of those subunits can be calculated as the sum of two components: the intra-subsystem components and the inter-subsystem components. The simplest case is the most compelling: the Hamiltonian of a two star-cluster gravitational system naturally breaks down into the sum of three Hamiltonians, one for each cluster and one for their gravitational interaction.

He's gone on to apply the principle to ecosystems, population dynamics, and most interestingly for me, thermodynamic systems. If we can consider the phase space as being so partitioned into two, then we naturally get three entropies: one for each element of the partition, and a common "binding entropy" for the whole. Although he's not extended this to the Boltzman form ($\sum p \log(p)$), we're hopeful that that extension will map nicely back into some domain of non-additive uncertainty measures (where the whole is rarely the sum of the parts).

The biggest deficiency of Auger's method (that's too strong, it's just not his question) is that the partition must be *given*. In evolving systems, it can be much more important to *identify* naturally occurring partitions as the arise. Or rather, it makes clear the epistemic task of choosing observables and frames of reference: e.g. although typical star-clusters are highly *significant features*, nothing in the star-cluster example *requires* us to identify those star-clusters as entities. An analysis of the overall Hamiltonian only would reveal no hierarchy.

> I suppose that technically the degrees-of-freedom limit is absolute, but
> since we begin with millions of degrees of freedom it isn't a practical
> limit. And because we can independently control perceptions made of
> orthogonal subsets of lower-level perceptions, there is no "drastic"
> reduction in degrees of freedom in going up a level. In principle there
> need be no reduction at all, although of course there can't be an increase.

I agree that hierarchy requires a non-strict decrease in d.f., nevertheless that reduction is typically why we invoke hierarchy. In Auger's theory that reduction is critical, and in the systems he's studies he's just getting results now verifying the conservation of system properties when considered either at the reduced level or the aggregate level.

In natural systems, hierarchy is typically identified (or defined) in terms of scale dependencies: if we see both fast and slow moving dynamics, and if that is a "significant" difference, then we identify two levels of analysis.

One difference between us may be my own focus on "3rd person" (external, objective) over "1st persion" (internal, subjective) descriptions. But maybe I'm wrong, or that's a huge can of worms anyway.

O-->
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V All the world is biscuit shaped. . .
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Date: Tue, 27 Nov 90 09:01:26 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG

Subject: Modeling

Bill Powers:

I suppose it's time to do some systematic evaluation of the behavior of the lagged control model in the context of "active" and "passive" disturbances.
When you get the time could you post some suggestions about how to do the parameter estimates for best fit. The model I am testing is your lagged control model so there are three parameters -- lag, gain and integration factor. The model is h(t)=gain*(ref-x(t-tau))-int*h(t). My plan was to store a 2000 sample vector of human data and then iteratively adjust the model parameters (gain,tau and int) to minimize the rms difference between model vector and subject vector. What is a nice, simple way to do this?
I remember that you used control systems with outputs as the parameters of the model and adjusting the outputs until the perceived difference between model and subject data was zero (or nearly). Did you use a separate control system for each parameter.

Also, when do we get to hear about your visit with Hugh Gibbon?

Best regards

Rick

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Date: Tue, 27 Nov 90 12:52:11 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: controlling language learning

Dear Rick and Bill,

I was pleased to read some [unintentional?] comments about language and education in recent postings. Since these are near and dear to my heart (and about the only comments I've REALLY understood since listening to others' postings!) I will venture to make a few tentative queries (which will hopefully become more "sophisticated" as my understanding of CT increases).

In fact, what I am attempting to do is apply a CT outlook to the field of Second Language Acquisition and language learning. This is going to require grappling with just such concepts as "quality of educational experience". It is also going to require rethinking/reexamining the basis for so much of language teaching (and teaching in general): methods based on research based on the method of "relative frequencies" to use Runkel's terminology. Since language teaching has become area of interest people have attempted to come to terms with the extraordinary amount of variability in learning outcomes among learners. Most just can't understand why, given a certain "level" of "proficiency" of a group of students and applying a certain method, the results aren't more consistent. In recent years the "goals" of learners have been one reason for differentiating among them. Thus we have the proliferation of programs such as "Teaching English as a Second Language" "English for Business Purposes" "English for Academic Purposes", etc. But the programs themselves are not "goal" oriented in the CT sense of

the word, and they do not deal with the individual any more than previous programs have, Instead, they basically still assume that if the goal is to function in an American university, then if I present this learning program as input, the output will be a learner who can function in an American university.

This perhaps poorly written example is just one of many aspects of language learning/teaching which I think would be served well by being examined in the light of Control Theory. If there is any literature already available on CT in an educational setting and/or a clinical setting which anyone sees as applicable to the area I've described, please clue me in.

Joel Judd
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Date: Wed, 28 Nov 90 08:41:41 -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: Language learning, hierarchy, modeling

Joel Judd

Your post was not only well written but it reflected an excellent understanding
of some of the basic points of control theory (such as specimen based research and the expectation that the same method is not likely to produce the same results at different times or with different individuals). I don't have any great stuff to contribute on your topic (second language learning) right now (though I would like to try to come up with ideas for studies of control of higher order variables,
such as linguistic variables). But I have read a couple of excellent articles by Hugh Petrie on the topic of control theory in education. If Hugh is still on the net perhaps he could post the references (and anything that he might have to contribute on this topic). If not maybe someone else who is listening could post the references to Petrie's writing on control theory in education. Keep posting when you can Joel -- this is a topic that control theorists should pay attention to (see, I write poorly but post anyway).

Cliff Joslyn

I'm afraid I didn't really understand your post. If you are interested in looking at a concrete implementation of the kind of hierarchical system Powers is talking about (from an objective perspective) you might try reading my article called "Spreadsheet analysis of a hierarchical control system model of behavior" that appeared in Behavior Reseach Methods, Instruments & Computers, 22 (4), 329-359 (1990). If you are interested, let me know and I'll send you a copy of the article. If you send a disk to my address below I'll send you a copy of the lotus spreadsheet that contains the hierarchical model (this, of course, goes for anyone else out there in CSG netland as well).

Gary -- thanks for the job announcement at Florida Atlantic. I think it might be too far for me, though it would be fun to be close to Kelso (I'm sure he'll want me there once he sees my paper in Psychological Science which basically says dynamical systems is a shuck).

Bill -- great post on the modeling level. I believe it, maybe. I bet a lot of people will hear what you are describing as being like schemas (at least, the talk seemed similar to the way schema theorists talked about them). It would be a nice overlap. I have been thinking recently that there are some evidences of a modeling level in everyday experience. Don't people who try to control other people have a "model" of people as controllable (at least while they are doing the controlling). Don't they get exasperated

(error) when people don't act the way the model "predicts". I think there may be something to this modeling level that is quite important.

Best wishes to all

Rick M.

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Date: Wed, 28 Nov 90 19:31:20 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: What I mean by hierarchy, and how

Cliff Joslyn --

Here's how the problem looks to me. You and I, all of us, look out on a world made of living things and nonliving things. We are one of the living things, human beings. We have brains that allow us to experience the world and think about it. If we want to understand anything more than the superficial appearances in this world, we have to try to explain how things -- human beings -- brains -- work. In working out explanations, we have to use the very things that we're trying to explain: perceptions, actions, memory, and thought. We want to understand other people, but that is the same as understanding ourselves, because we are examples of the same organization.

This works the other way, too: in understanding ourselves, we come to understand all human beings. The trick, of course, is to know when something you notice about yourself is the sort of thing that is likely to be true of others. You don't have to know in any absolute way; you can always ask other people. But it saves time to try, at least, to distinguish universals.

One of the things we all can do is to make abstractions. Some of us learn to do this in a formal way with all sorts of mathematical rules to keep us from contradicting ourselves. Others do it in a superstitious way and are very sloppy about it. But we all do it. I assume that's true because I am certain I do it, and there's plenty of evidence that others do it, too. So that is one of the universals.

The fact that we make abstractions is different from what those abstractions happen to be about. The making of abstractions is a fundamental process we carry out, presumably in our brains, so in that regard we are alike. But the CONTENT generated by that process doesn't have to be the same in any two people. Through communication and formalisms we try to share abstractions, so it is possible that the content may be nearly the same in selected groups of people. But if we focus on the content too much, we may miss the fact that the same abstracting capacity is present even when the contents disagree. The witch-doctor casting bones to predict the crop is doing the same thing that the agro-scientist at his computer terminal is doing. Only the content is different.

When I set out to look for levels of perceptual organization, I was

looking for universals of this kind, the kind that are content-independent and as universal as having a brain at all. Formalisms are of no help in finding this kind of universal, except in the sense that they demonstrate basic capabilities of the brain just by existing. The only way to discover this kind of universal is to examine experience very closely, trying to see uniformities that don't depend on what, in particular, a person is thinking, perceiving, or doing, but which exist in everything a person thinks, perceives, and does, smart or dumb, right or wrong.

In 35 years of looking, I've come up with 11 levels of perception, with a 12th under consideration. That's three years per level, and the first few were easy. The reason this takes so long is that there isn't any system or algorithm that will tell you what a level ought to be. The only way to find a level is to catch yourself using it, and to recognize it as a mode of perception instead of getting hung up on what it is about. This is difficult and unscientific. But I think it's a fundamental step we have to take in order to find out what the brain does that we have to explain with our theories and formalisms.

So when I say that there's a universal level of perception concerned with configurations, I don't mean that all people see triangles and squares, or hear chords the same way, or experience the same taste-configurations as familiar, or have similar body images. I'm saying only that they all experience configurations of some sort. The same goes for sensations, a level down, and transitions, a level up. By saying "down" and "up" I'm defining a direction in a hierarchy, and by saying "hierarchy" I'm claiming that these levels are related in the sense that one is derived from the other: the higher can't exist unless the lower exists. I don't have any formal way of proving that this is so. It's just that when you look closely to see what a perception is made of, this is what you find. Configurations decompose into sensations, but sensations don't decompose into configurations. I claim that this is what anyone human will find on close inspection.

I don't know why these levels exist. Maybe reality is organized that way; maybe it's just our way of bringing order into experience. What I am interested in is finding a model in which these levels appear in proper relationship, and that helps explain experience. Part of this modeling effort involves something I call a brain, with all its computing properties and so on. A good part of the effort -- although I'm not very good at this -- will be to try to guess how the brain-model ought to be organized so it can, by its own rules, compute these sorts of levels of perceptual information (and by acting on the world, control perceptions at all these levels).

Maybe, in the end, we will find that the best way to represent the brain model is in terms of Hamiltonians, although my view is that if that happened it would be an incredibly lucky guess. I don't think that any particular product of the brain has a priori truth in it, and Hamiltonians are certainly a product of the brain, in my model. I think we have to start with unexplained phenomena; I claim that my levels are unexplained phenomena, at least until we find a model that can represent the machinery that creates them.

It may be significant that with my obsession with modeling, the last level I would recognize would be the level that makes models. On the other hand, maybe it isn't significant. Maybe there aren't any levels. The only way we can see if this idea is any good is to build a model about it and see if it does better at explaining experience than some other approach does.

So, Cliff -- what phenomena do you see, that require explanation?

Addendum: Maybe the model level is what I've been calling the system concept level, and belongs at the top.

Gary: Stark, Neurological Control Systems. I thought I had the book on my shelf but can't find it -- maybe I lent it out.

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Date: Thu, 29 Nov 90 02:16:48 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Hierarchy and "my phenomena"

Yes, Bill, it is as I suspected: your approach seems to me fundamentally subject-based, while mine is primarily object-based (in my terms, flames off until I define these). I recognize these as complementary modes of explanation, without any "post-modern" prejudice against 3rd-person descriptions.

Yet what we share is, as you suggest, the method, the process of model-construction, of abstraction. Assume a meta-model of model construction based on a reciprocal, cybernetic relationship between prediction and corroboration. Of course neither prediction nor corroboration can be a priori: prediction requires a model in a language, and corroboration requires observation and a testing mechanism. All models require certain assumptions about the fundamental categories of prediction and observation. In turn, models lead to meta-models, where the language and observation and testing mechanisms of the prior model themselves become the object of modeling. (In future papers my colleagues and I will attempt to elaborate on this position as "cybernetic foundationalism").

So, to answer your question, my questions are fundamentally about cosmic evolution, the physical change of the universe, especially the origin of life, the ecosystem, culture, and mind. I hold evolution as a fundamental Good, and fearing that it is imperiled, want to model evolution so as to predict its future course and take action to assure its continuance.

My questions are what I would call "scientific". The assumptions I make are the "common body" of (relatively) consensually held scientific knowledge, which includes a lot of physics and chemistry, a fair quantity of biology, a little ecology, economics, etc., and (unfortunately) precious little psychology. This body of knowledge in turn rests on assumptions we make about the validity of sense data, common sense reasoning, the robustness of measuring instruments, and the huge body of theory developed over millenia. Thus there is a very elaborate and rich set of assumptions that is required to ask and answer my kinds of questions, which are fundamentally about things that are *not us*.

To answer you very directly, I do not and cannot "see" the phenomena that require explanation. They occured millenia ago, they are not in front of me. To even ask the question: "how did life arise?" requires an understanding of chemistry, astronomy, and genetics. I don't *see* these phenomena: I *believe* these phenomena occurred because of an elaborate *inference* itself based on a lot of science.

It strikes me that you take a different approach, not assuming these foundations, but rather going in the other direction, deeper, not higher, beginning not with the whole edifice of science, but exploring

the fundamental properties of sense data perception, so-called common sense theory formation, etc. These are questions primarily about *us*, about psychology.

As I said above, these are undoubtedly complementary modes of description, and both "valid" (cf. Bateson "Conscious Purpose vs. Nature"). Perhaps where we disagree is when you say:

>If we want to understand
>anything more than the superficial appearances in this world, we have
>to try to explain how things -- human beings -- brains -- work.

Now since surely we know very little about brains, then if you are right the we only know the superficial appearances of the world. And if you assert that we *do* only know the superficial things, then either knowing profound things is impossible, or at least we can never tell whether something we know is superficial or profound. So yes, we all want to explain everything, from quarks to minds. It just depends on which direction you want to go in: assume minds to explain quarks, or assume quarks to explain minds. Because surely to explain anything we must assume something.

Unfortunately, this distinction maps back to a realist/anti-realist dichotomy. Someone in my position assumes a somewhat realist position, that we can construct theories of "so-called reality" that are somewhat independent of our perspective. Those concerned exactly *with* that perspective (psychologists) focus on the fact that the objective questions rest on assumptions, which must remain unfounded, and tend to deny naive realist claims. I hold that this is a red herring, and hope that that will not be an issue on this group (I feel bad about just this amount of clutter). (But I will take up the argument if desired).

I see the processes of evolution as essentially universal over time, and it includes hieararchy everywhere. Note that this is hierarchy in terms of the *content* of the theories, not just in their form or the process of their construction (the psychology). This includes the descriptions of astronomical systems in terms of their Hamiltonians. And I agree with you that the essential property of hierarchy is monotonicity: something at the higher (lower) level is necessary for the lower (higher), but not vice versa.

O-->
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Date: Thu, 29 Nov 90 07:57:06 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: Science & hierarchy

Cliff --
Your thoughtful communication of Nov. 28 should draw a lot of comments. I'll keep mine short.

Our pursuits are indeed complementary. Questions about *us* are basically questions about the human observer-actor, which includes, of course, the scientist: the maker, reader, and interpreter of instruments. Our instrument-readings are only as "robust" as our understanding of the reader who gives meaning to what would otherwise be just numbers. But as you imply, this is a bootstrap operation. We use models to make models. To spend all one's time "going meta" is a mistake, just as it is to spend all one's time simply applying received wisdom. In order to build models of

reality (which includes, of course, "brains"), we must apply what science exists and is understandable to us. To test these models we assume their reality and devise procedures to challenge them. But to complete this process we must also periodically challenge the assumptions on which our modeling stands, so that we can continue to separate That from This: to separate the contribution of an external reality from the contribution of our own properties to the overall result that we call observing and analyzing. My position in the realist/antirealist dichotomy, as you can see, is about where the / is.

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Date: Thu, 29 Nov 90 08:59:20 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: language (cont.)

Rick Marken (and all)

Thanks for the return and suggestions - a little positive reinforcement always helps (oops, pardon the terminology). Yes, it will probably be necessary for my dissertation to include some "data", so I will be interested in brainstorming ideas for at least a small attempt at investigating higher order variables in a language learning context.

Regards - Joel Judd

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Date: Thu, 29 Nov 90 09:34:52 -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: Eating Disorders

Here's a question that should bring us (at least some of us) back to reality. I realize that this question is really for the clinicians who are not on this network (or perhaps one or two have sneaked on?), but here goes anyway.

My wife is a physician at the university health center here and often has to deal with anorexics (who practically starve themselves and vomit after eating) and bulemics (who go on eating binges of overeating and likewise vomit afterward). We had a discussion about one of her anexoric patients this morning and I promised to bounce a question about eating disorders off the network.

My wife contends that the anorexics see themselves as fat even when they are seriously underweight. So CT would see them as simply having a very extreme reference level which is in fact dangerous to their health. She also mentioned that anorexics often feel that much of their life is out of their control and so they overcompensate by strictly controlling the quantity of food they put in their mouth.

Any other ideas about understanding their behavior and hints to aid reorganization from a control theory perspective?--Gary

P.S. I would really like to get the clinicians involved in networking, either by adding them to this network or starting up a sister one. Any ideas about this? Anyone have experience with using email through a commercial service such as CompuServe or GEnie? (As I mentioned earlier, we can easily communicate with anyone on CompuServe, but I don't what other services provide acces to bitnet and/or internet.)

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Date: Thu, 29 Nov 90 10:57:15 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Science & hierarchy
In-Reply-To: Message from "Bill Powers" of Nov 29, 90 at 7:57 am

I agree very much with your comments.

> My position in the realist/antirealist dichotomy, as you can
> see, is about where the / is.

Indeed, and the significance of being on one side of the line or the other *at a particular time* *for a particular problem*. I try to be as slippery as possible, being a realist when it's appropriate and an anti-realist where it is not.

O--->
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| Systems Science, SUNY Binghamton, Box 1070, Binghamton NY 13901, USA
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Date: Thu, 29 Nov 90 11:13:46 -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: sundries

Cliff --

As I noted earlier, cosmic issues tend to give me a headache. But I've got to ask what you mean by this intriguing statement:

> I hold evolution as a
>fundamental Good, and fearing that it is imperiled, want to model
>evolution so as to predict its future course and take action to assure
>its continuance.

What do you mean by "evolution" -- the fact of speciation over time or the models used to expalain it?

Why is evolution -- fact or theory -- good? It just seems to have happened and, for all we know, is still happening and will continue to happen. I can see that it might be interesting, or puzzling or satisfying -- but Good?

How might evolution stop? Even nuclear war is just another speck of an event in the eons of evolutionary history.

How is all this related to control theory (or what you call cybernetics)? Have you modeled any of this stuff and tested predictions of the models?

I'll just say that, off the top of my head, Good, for me, is when perceptions at any level of my hierarchy match the (possibly "bad") reference levels I have for those perceptions. Thus, I experience good sensations, configurations, sequences, relationships, programs, models?, principles and

system concepts. I think that the word "good" is used to describe any perception that matches one's own reference level for it. Ethical "good" is, I think, when we see people acting in accordance with principles that we hold dear (ie --that we have reference levels for). A "good" person, for example, is one who seems to act in a way that is consistent with our reference levels for honesty, altruism, and whatever other principles we are interested in (remember, these are just words -- the perceptions that correspond to these things for one person may differ from those that correspond to the same words for another person).

Perhaps we could get other CSGers involved in a polite (principle perception) little discussion of ethics.

General Net Item --

The most recent (November) issue of Psychological Science has a feature section of electronic publishing (that's us folks). It might be worth a look. There is a psychology list led by Steven Harnad (of BBS fame). It might be worthwhile to post some articles to that list to see how "real" psychologists respond to the work of CSG people. On the other hand, we could make those folks aware of our existence. I forget -- how do you subscribe to CSGNet?

Gary Cziko - It would be nice to get some clinicians on the net. But I also think it would be nice to get Ray Pavloski on the net -- given all our talk about sophisticated control theory, he is one who might be able to contribute and, perhaps, profit from one of these discussions. Why not give him a buzz and tell him how to get on the net?

Regards

Rick M.

****

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Date: Thu, 29 Nov 90 15:07:38 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Re: sundries

Rick (Marken):

You asked:

>I forget -- how do you subscribe to CSGNet?

I call it CSG-L now since that is our mailing name (L stands for "list"). Since others may have also forgotten: just send me a note, or call me, or send me a letter or carrier pigeon. OR, just send the following message to LISTSERV@UIUCVMD (bitnet) or LISTSERV@VMD.CSO.UIUC.EDU (internet):

SUBSCRIBE CSG-L last name, first name, institution
as in
SUBSCRIBE CSG-L MARKEN Rick, Aerospace Corp., Los Angeles

>It would be nice to get some clinicians on the net. But I
>also think it would be nice to get Ray Pavloski on the net -- given
>all our talk about sophisticated control theory, he is one who might
>be able to contribute and, perhaps, profit from one of these discussions.
>Why not give him a buzz and tell him how to get on the net?

I buzzed Ray about a month ago and he said he would soon have email access.
Haven't heard from him since. If others want to convince Ray to get
moving on this, call him in Pennsylvania at his office (412)349-1373 or
home (412)357-2374.--Gary

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Date: Fri, 30 Nov 90 00:07:52 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: sundries
In-Reply-To: Message from "CSG-L@VMD.CSO.UIUC.EDU" of Nov 29, 90 at 11:13 am

> > I hold evolution as a
> >fundamental Good, and fearing that it is imperiled, want to model
> >evolution so as to predict its future course and take action to assure
> >its continuance.
>
> What do you mean by "evolution" -- the fact of speciation over time or
> the models used to expalain it?

Evolution, in general, is a kind of change. It is change described (but
not defined) by (at least) an increase in complexity of the forms, and
their embedding within each other in a hierarchical manner. This is a
form of monotonic growth: earlier forms are retained. Speciation over
time is an example of spec-ial (of the species) change. Speciation over
time which leads to an increase in the complexity of the organisms is
spec-ial evolution. No doubt our models also evolve in time, but the
evolution of something is different from the evolution of a model of the
evolution of that thing.

> Why is evolution -- fact or theory -- good?

I don't know. I don't think that ethical judgements can be derived from
factual judgements (although they can be constrained by them). Thus a
fundamental good cannot be derived, it must be asserted as an act of
will. I choose to find Evolution a Good, and from this Good I derive
other goods, like the continued existence of humans, my family, myself,
and you.

> It just seems to have happened
> and, for all we know, is still happening and will continue to happen.

I agree completely that it "just happened" (I think that attitude is
important). And I know that it is continuing to happen. But it may
very well cease to happen, at least *in some contrained frame of
reference*, like: on this planet, in my lifetime, in the context of
humans and human society, etc. Thus another way of stating a smaller
version of my view is that if human evolution is a dead-end (we are
de-selective) then that would be Bad.

> How is all this related to control theory (or what you call cybernetics)?
> Have you modeled any of this stuff and tested predictions of the
models?

Yes, that's the $64K question. At this point my colleagues and I have a theory, that (in general) evolution proceeds in discrete steps of emergence of levels of control, called "meta-system transitions" (MSTs). Each step of evolution adds a level of control in a control hierarchy. A (much simplified) schema for this is (assuming living organisms):

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      Society is the control of thought;
        which is the control of learning (associations of mental
                                          representations)
        which is the control of instincts;
        which is the control of reflexes;
        which is the control of the movement of parts of organisms;
        which is the control of the positions of parts of organisms.
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It's difficult to model such things. While we have no hope of modeling the whole, we are in the process of modeling the fundamental process of the MST. The rest is theory (an abstract model) which will make predictions about the body of consensually-held scientific knowledge, and predictions of "scientific truths" to come (e.g. in the biology of the origins of life).

I'll post a full paper if we want more elaboration on this.

> I think that the word "good" is used to describe any
> perception that matches one's own reference level for it. Ethical "good" is,
I

Sounds like hedonism to me: it's good if it feels good. Do you question why good things feel good? Do you entertain the idea that certain things feel good so that the organism will do certain things so that it will survive and procreate? This pushes the question of what is good back to the question of what survives, and in turn what evolves.

> There is a psychology list led by Steven Harnad (of BBS fame). It
> might be worthwhile to post some articles to that list to see how "real"
> psychologists respond to the work of CSG people.

There's also the newsgroups comp.psychology, comp.ai.philosophy, and comp.simulation.

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

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Date: Thu, 29 Nov 90 08:42:03 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti1@VUB.VUB.AC.BE>
Subject: hierarchy

Bill Powers:
There are always lots of debates on whether mental models are analog or propositional (or both). I don't know how you address this problem in your hierarchy.

Mark Nelson:
Last week I asked you how you integrate Brooks' and Powers' works together. Until now I haven't seen your reply.

Did I ask a silly question so that you don't want to answer?

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: Fri, 30 Nov 90 06:48:35 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <arti1!chen@VUB.UUCP>
Subject: Re: sundries

Cliff Joslyn:
Sometimes I feel you talk like a philosopher. Can you explain what
"Cybernetician at large" and "All the world is biscuit shaped" really mean?

Chen

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Date: Fri, 30 Nov 90 08:38:50 -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: Evolution of Control Systems

Cliff Joslyn:

One good thing about *no*t living in California is that I get a chance to
put in my two cents before Rick Marken gets out of bed.

>At this point my colleagues and I have a
>theory, that (in general) evolution proceeds in discrete steps of
>emergence of levels of control, called "meta-system transitions" (MSTs).
>Each step of evolution adds a level of control in a control hierarchy.

I find this idea quite appealing, and I believe it is consistent with
control theory as understood by the Control Systems Group (CSG). However,
I believe you are in for a whole lot of trouble (from CSG at least) when
you say:

>A (much simplified) schema for this is (assuming living organisms):
>
> Society is the control of thought;
> which is the control of learning (associations of mental
> representations)
> which is the control of instincts;
> which is the control of reflexes;
> which is the control of the movement of parts of organisms;
> which is the control of the positions of parts of organisms.

Those last two lines in particular make no sense at all within control
theory. Therefore, one might conclude that you either do not understand
the basic insight of control theory (behavior as the control of perception)
or you understand it and have some reasons for rejecting it. If the
former, you need to read Powers 1973; if the latter, it would be of great
interest to us to share with us your reasons.

Another problem is the lumping together of biological and cultural
evolution and the different type of controls that result. Culture may

appear to control aspects of human behavior (the system of law is a good example) but it can only do this through the interaction of human beings as autonomous control systems. Nobody outside of me can reach in and change my reference levels. Society cannot control my thought. But growing up in a particular society and culture present problems which may lead me to reorganize my control systems in new and (usually) culturally appropriate ways. The idea that society can control individuals' thoughts and actions by nonviolent means has been proven wrong many times and remains a dangerous myth. Control theory provides the first real insight into the fallacy of this myth.

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Date: Fri, 30 Nov 90 10:16:34 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: sundries
In-Reply-To: Message from "Chung-Chih Chen" of Nov 30, 90 at 6:48 am

> Sometimes I feel you talk like a philosopher.

Sometimes I do.

> Can you explain what
> "Cybernetician at large" and "All the world is biscuit shaped" really mean?

Not much.

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

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Date: Fri, 30 Nov 90 10:29:35 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Evolution of Control Systems
In-Reply-To: Message from "CSG-L@VMD.CSO.UIUC.EDU" of Nov 30, 90 at 8:38 am

> >A (much simplified) schema for this is (assuming living organisms):
> >
> > Society is the control of thought;
> > which is the control of learning (associations of mental
> > representations)
> > which is the control of instincts;
> > which is the control of reflexes;
> > which is the control of the movement of parts of organisms;
> > which is the control of the positions of parts of organisms.
>
> Those last two lines in particular make no sense at all within control
> theory. Therefore, one might conclude that you either do not understand
> the basic insight of control theory (behavior as the control of perception)
> or you understand it and have some reasons for rejecting it.

I suspect more the former than the latter. Although I've been talking about Powers' ideas w/him and others for a while, his book(s) have been on my very long list of critical reading for a long time.

I hesitate to take this discussion much further, since I have not read Powers. Please understand, I launched into all this because Bill gave a broad, interesting response to a much more technical and specific discussion of Auger's hierarchy theory, and I've been encouraged to elaborate. In this group I'm not necessarily prepared to defend all my ideas appropriately. Also, I'm a "control theorist" only in the general sense of doing cybernetics, systems theory, and natural philosophy about the evolution of levels of control, not as a technical control modeler like most of you.

Here's a thought: as a benefit to myself and other newcomers (non-CSG'ers) could you briefly state why my last two stages are absurd according to Powers' control theory?

> The idea that society can control individuals' thoughts and actions
> by nonviolent means has been proven wrong many times and remains a
> dangerous myth. Control theory provides the first real insight into the
> fallacy of this myth.

Actually, I had criticized our first stage (above) on very different grounds: there is a difference in type between that stage and all the others. In particular, many non-human societies exist, and human society existed contemporaneously with human origins. All other stages indicate a temporal evolutionary process.

On your point, this would require further elaboration on your part and education on mine.

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

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Date: Fri, 30 Nov 90 08:10:24 -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: Marken effect

Marcos--

Welcome to CSG-L. As the Marken who discovered the Marken effect I am certainly pleased by your interest in it. There are several posts related to what came to be called the Marken effect. I'm not a net expert and I did not personally save all the posts but maybe Gary Cziko, the CSG-L manager, could help you get to them. I am planning to write up a report on this effect but this may take several weeks. But I may distribute a pre-publication version over the net.

It looks like you posted directly to me. If you post your request for info about the Marken effect to CSG-L you might get some help from people who may have some archive copies of early articles. I would love to know why you are interested in the effect -- but let's carry on the discussion on CSG-L so others can join in if they care to.

Brst Regards

Rick M

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Date: Fri, 30 Nov 90 10:10:29 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Behavior:Control of perception

Gary --

I'm up!! I think your $.02 worth is worth considerably more than that. Your comments about societal control were right on target. Let me just say a few things that are related to Cliff's comments about control of movements and positions. (so this is also for Cliff).

I believe that Gary found Cliff's comments about "control of movement and position" absurd because they imply that control systems control outputs. Gary knows that control systems really control their inputs (perceptions). The outputs of a control system depend not only on effects produced by the control system but also on effects external to the system -- these are disturbances. Disturbances can enter a control loop at any point; they could even be added to the neural signals in the control loop. These disturbances can influence every variable in a control loop; But the loop is organized so it always acts to keep the perceptual signal matching the reference. The disturbance may change the amount of output required to keep the perception at the reference, it may change the relationship between external variable and perceptual variable but it cannot affect the relationship between perception and reference -- the closed loop sees to that. So a control system doesn't really control movement or position or reflexes or whatever. It doesn't even control a variable in the outside world. The thermostat doesn't control "heat" in the room -- it controls the voltage that represents heat as represented by the metallic strip. If you change the heat transducer, (Metallic strip) you get a new voltage (perception) for the same heat -- but the control system still keeps the voltage at the reference -- which may mean that you experience a hotter or colder room.

The control system controls only one thing -- a perceptual input signal. This signal may be a representation of simple or complex variables outside of the control system. When we look at the control system we will see that system influencing our perceptions -- perceptions of movements and positions and "instincts" (really programs perceptions). But to know what the system is actually controlling, we must learn how our own perceptions are related to the perceptions being controlled by the control system.

Let me give a concrete example; this is from my paper "Behavior in the first degree" in Hershberger's Volitional Action book (North-Holland,1989). I had a subject control the "size" of a quadrangle on a computer screen. The subject's mouse movements (left/right) affected the width of the figure; the height of the figure was continuously changing (disturbed). In order to keep the size of the figure constant the subject had to change its width to compensate for changes in height. Simple enough. The question was -- what is the subject controlling. What is "size". Well, it is a variable, but not an output-- it is a perception -- the subject's; the perception could depend on various aspects of the display such as height x width (area) or height + width (perimeter) or diagonal length, etc. There are other possibilities as well, such as temporal relationship between changes in the length of height and width.

My study showed that the controlled perceptual variable corresponding to "size"
was more like height x width than height + width. The point is that a word like "size" describes many possible perceptual variables -- as does the word "movement" or "reflex". A control theorist never takes for granted that s/he knows what an organism is doing simply because a word can be found to describe his/her own perception of what the organism is doing. Saying that the subject is controlling "size" would not satisfy a control theorist. Saying the subject was controlling area (measurable variable) would be better; but you would always want to test to make sure that your guess about what variable is controlled is an accurate one.

The fact is that this is the way control works -- control is always organized around the control of a perceptual representation of some objective state of affairs (a state of affairs that must be influenced by but not necessarily understood by the control system). Thus, if you are interested in the evolution of control I think it is important to understand the nature of control and the nature of systems that can exhibit this phenomenon. For people who are interested in organismic control (living control systems) I can think of no better place to start than the works of William T. Powers.

Best Regards

Rick

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Date: Fri, 30 Nov 90 11:04:50 -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: Ethics

Joel Judd (and Cliff) --

I see I might have got myself in some trouble by saying the following:

>I'll just say that, off the top of my head, Good, for me, is when perceptions
>at any level of my hierarchy match the (possibly "bad") reference
>levels I have for those perceptions. Thus, I experience good sensations,
>configurations, sequences, relationships, programs, models?, principles and
>system concepts. I think that the word "good" is used to describe any
>perception that matches one's own reference level for it. Ethical "good" is,
I
>think, when we see people acting in accordance with principles
>that we hold dear (ie --that we have reference levels for). A "good"
>person, for example, is one who seems to act in a way that is consistent
>with our reference levels for honesty, altruism, and whatever other
>principles we are interested in (remember, these are just words -- the
>perceptions that correspond to these things for one person may differ from
>those that correspond to the same words for another person).

Cliff says:

>Sounds like hedonism to me: it's good if it feels good.

Implying that hedonism is a bad thing.
I like it -- but don't get scared -- read on.

Joel says:

>How would CT avoid some kind of "reference-level relativism"? I understand
>Powers' explanation that on one level a control system simply acts so as to
>reduce error (the book described it as "having no pride", I believe) and
>then on another level our actions may be "good" or "bad", since a High
>School student, for example, may act so as to maintain a reference level
>most would consider "irresponsible" or "unintelligent". So how do these
>characterizations of "good" and "bad" come about - a combination of our
>perceptions mingled with others'?

First, let me say that I made the remark about "bad" references only because some of the perceptual goals people select are bad from the perspective of others (because they are disturbances to perceptions they are trying to control) or because they really hurt the person who selects them. I think ethics gets involved in both of these situations.

Let me say right off that what people really mean by ethics are rules about how everybody should behave. I think that, from the point of view of control theory, the notion that such rules could exist is just absurd (there's that word again). There is no particular way that a control system can behave in order to control the variables that it must control -- and there is no control system that behaves this way. Absolute ethics is based on a stimulus-response notion of human nature (you can quote me). Control systems usevariable means to produce consistent ends -- and this is true at all levels of teh control hierarchy. The problem with absolute rules comes up all the time -- if the rule is "respect your father and mother" then how do you select the appropriate action to deal with a parent who is sexually molesting you (produing, I imagine, a massive perceptual error)?

There is only one ethic that comes out of control theory (I think) and that is " Respect the fact that other people are control systems". This does not mean that you let people do whatever they want. By doing so they may be preventing you from satisfying your own references -- you are a control system too and you get respect as well. "Respect" does not mean being a door mat. In the best case it means COOPERATION. Respect for another person's control system-ness is a two way street. But one way is definitly to let control systems control what they want if it doesn't really interfere with you in a significant way. Thus, killing and stealing are out in most cases -- but if a control system decides that it can only satisfy its references by killing and raping (I suppose these are the psychopathic criminal types) then maybe there is no alternative but to kill that control system (or try to). But I think cases like this are EXTREMELY rare. But still, they point up the fact that there really are no absolute rules for how to behave -- there never have been and there never will be. But this is not a problem -- people havn't gone haywire yet. They actually manage pretty well by just respecting each other, to the extent that they do, as autonomous control systems. They vary in their degree of respect -- but on an every day, getting along kind of way they seem to do pretty well, whether they believe in absolute rules or not.

More interesting ethical questions come up when control systems do things that disturb variables you care about but do not threaten your ability to control intrinsic variables (Life sustaining variables). These are the things that Joel Judd mentioned -- the "unintelligent" choices of some younger control systems. Apparently some control systems are disturbed by other control systems running around toking joints and wearing green hair. I know control

systems (my parents) who are very disturbed when I order pork chow mien at a restaurant (an example of one of my "bad" reference signals). I think the ethics of these situations are kind of interesting -- I mean, unless you believe that god said "no green hair" and "no pork". The fact is, dealing with other control systems means cooperation and that often means compromise. I try to avoid Chinese restaurants when I eat with my folks. I'm willing to compromise. But this does not come out of some grand scheme of ethics but a strategy for keeping the level of conflict in my control hierarchy "low".

I have much more to say about this -- and I'd love to hear other thought's about this. I will just say that I think an ethics (a nice way of getting along with people) can be built on the idea of respect for control systems; as Hugh Gibbon (the lawyer) argues -- it really already has. Western legal systems are pretty much based on this idea. The problem is "who gets included as a control system". We are able to have wars and other kinds of shit as long as we are able to consider "them" different than us.

Also, I think it is important to remember that getting along with other control systems (behaving ethically) means giving up some control. The best social control systems tend to be the biggest assholes.

Hasta Luego

Rick

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Date: Fri, 30 Nov 90 14:27:00 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Warning -- RSCS tag indicates an origin of SMTPUSER@UBVMSC
From: Hugh Petrie <PROHUGH@UBVMS.BITNET>
Subject: Education and Models

This post is from Hugh Petrie. Yes, I am still on the net and enjoying it enormously, although the budgetary situation in New York ensures that I have very little time to participate. There are, however, a couple of recent posts to which I should respond.

EDUCATION REFERENCES
Rick Marken kindly referred some of you to some of my writing on control theory and education. The following are some references:

THE DILEMMA OF ENQUIRY AND LEARNING, (Chicago, University of Chicago, 1981. This is probably my most extended treatment of issues in education and control theory and it adds evolution as well for those who have that bent.

If the book is out of print at the University of Chicago, let me know, I have about 8-10 copies which I could make available at cost.

"Against Objective Tests: A Note on the Epistemology Underlying Current Testing Dogma", in Mark N. Ozer (ed.) A CYBERNETIC APPROACH TO THE ASSESSMENT OF CHILDREN: TOWARD A MORE HUMANE USE OF HUMAN BEINGS (Boulder, Colorado, Westview Press, 1979)

"Program Evanluation as an Adaptive System," in R. Wilson (ed.), NEW DIRECTIONS FOR HIGHER EDUCATION; DESIGNING ACADEMIC PROGRAM REVIEWS, No. 37 (San Francisco, Josse-Bass, 1982)

"Action, Perception, and Education, Educational Theory, 24, 1974.

"A Rule by any Other Name is a Control System," Cybernetics Forum, VIII (Fall/Winter, 1976)

"Testing for Critical Thinking," PROCEEDINGS OF THE PHILOSOPHY OF EDUCATION SOCIETY 1985, 1986 (I can send copies of this if you can't find it.)

"Metaphor and Learning," in Andrew Ortony (ed.) METAPHOR AND THOUGHT (Cambridge, Cambridge University Press, 1979) (I think that there will be a new edition of this coming out shortly. I have substantially revised the original paper.

As I said, DILEMMA is the most complete source. "Action, Perception, and Education" would be an early, fairly unsophisticated version, but short. The others are all more specialized.

If I may, I would note that there are two major educational implications of control theory that have exercised me thus far. First, the oft-noted fact that the reference signal is inside the organism, implies

that you cannot simply hand over any ideas or concepts or skills. You must start with where the student is and, if through testing for what the student is controlling for already, you determine that the student has "got it wrong", the only way to proceed is to try to introduce disturbances as a teacher which, given your hypothesis about what the student is controlling for, may not be able to be counteracted by the student's control system. If not, there may be some reorganization and the teacher is there to suggest more adequate reference signals which the student may use to reduce overall error. Note that if the teacher's efforts are unsuccessful, the student will go on as before removing error as best as possible. The thing about schooling is that you sort of have to assume that at some higher level of the system the student has a reference signal about the good life and how school or education will help and so on. You can see how one can account for dropouts, criticisms of school as irrelevant and so on. you can also see how control theory potentially gives the basis for really understanding such things as constructivism, teacher as coach, and so on.

The second major insight I think I have had is the importance of perceptual learning in education. If one is trying to establish a control system, or rather encouraging a student to establish a control system, at any level, then one has to pay lots of attention to trying to make sure the assumed existing systems at lower levels get grouped in appropriate ways. For example, to teach a child a triangle--configuration, one has to make sure the perceptual inputs are grouped in accordance with the standard way we approach geometric figures. Of course, the test for the controlled quantity allows the teacher to monitor to see if the student has yet "got it". If one moves up some levels, the problems get more serious. If we want to teach about honesty, we have to provide clear example and be sure that the student learns to perceive what honesty is. My concern, if I may pose a question to the group, is that the formation of the perceptual input portion of the system is typically given short shrift, perhaps, because for the modeling we have been doing, the perception is not at issue. I contend that for most learning tasks, it is central.

Let me end this section with a commentary on how public policy is also prone to the misinterpretations of control theory that have been mentioned on the net with respect to cognitive psychologists and sophisticated control engineers. The State of New York education department is proposing that we radically revamp our system of education to specify goals(reference signals), monitor progress toward the goals (perceptual input and comparison with the reference signal) and "free" educators to find whatever means are most appropriate to reach those goals (design output functions for lots of different settings). The good news is that this really could be understood as an attempt to control perceptions. The bad news is that the reference signal is still being "inserted" into the heads of the educators by the policy community rather than depending on what the teachers have in their heads regarding what educational goals ought to look like. I will be criticizing the New York plan next week in testimony to the Board of Regents, relying on control theory, but, of course, not being able to mention it directly.

MODELS I also have just one reaction to the suggestion by Powers and Gibbons that models may be a new level in the hierarchy. I simply have not had time to think this one through, but my initial reaction is skeptical. We all have lots of models of lots of things in our heads about a whole variety of different things--triangles, driving a car, cases in law, the politics of the state of New York, my school of education, and so on. My intuition is that models are what some others call concepts and may be more or less elaborate depending on how much of the rest of the hierarchy of control systems in a given person they connect to. In other words, might not a model of any given thing at a given level, e.g., triangle, just be the name we give to the substantive grouping of control systems which in my mind] allow me to deal with triangles?

Well, enough. I say nothing for months and then go on at too much length. I hope this is helpful to those who are interested in education.

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Date: Fri, 30 Nov 90 23:05:14 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>
Subject: Comments on comments

Cliff:

The prediction-corroboration loop is a control system.

"Superficial" knowledge is knowledge gained by observing apparent causal or coincident relationships without any generative model of underlying processes. Statistical studies yield superficial knowledge. I presume you're not much interested in statistical studies of the effect of A on B. So I presume that what you're interested in isn't superficial.

A person who thinks that organisms are stimulus-response machines will look for ways in which "reactivity" or "irritability" might originate. A person who thinks they are control systems will look for ways in which closed-loop control relationships might originate. And so on. A theory of origins is highly dependent on what you think got originated. What model of organisms do you use as That Which Needs Explanation by a theory of origins?

In the latest post I've seen (30 Nov), it seems to me that you're using the term control a bit loosely. If A controls B, then for any disturbance acting on B, A alters its action so as to prevent B from changing significantly. A also, at the same time, determines the state in which B will be maintained (the state can be dynamic). I don't see how this applies

to "society controls thought." If I change my thought, does society act on me to restore my thought to its original state? I don't see how society can even know the state of my thoughts, much less affect them in some way. Are you sure you're not talking about something like "affect" or "influence" or "contribute to?" That's quite different from "control."

A "reflex," if you're using the term in the usual way, is a low-level behavioral control system. The lowest level is the control of applied force, via the tendon reflex. These systems make the mass of moving parts appear smaller to higher-level control systems. Next is the control of muscle length via the gamma system and muscle spindles, an indirect and not too reliable measure of limb or body relative position. Then we have postural control based on position sensors in joints (coupled with visual monitoring of position) -- and THEN motion or path control, which I call control of transitions. So motion control seems to be hierarchically above position control. That is, to maintain a specific movement it is necessary to alter position continuously, but position can be maintained without entailing any controlled motion. The reverse is not true: in order to control motion, it is necessary to alter positions. In the brain, centers concerned with motion are anatomically superordinate to those concerned with posture. I think that the "inside" view gives better information here than the "outside" or objective view of behavior. In physical models of motion, position is the integral of motion. In the nervous system, it doesn't seem to be organized that way, at least as far as controlled variables are concerned.

"Instincts," as nearly as I can understand what the term might mean, are inherited controlled variables (or inherited reference signals relating to controlled variables). I distinguish these from variables in the hierarchy of learned behavioral systems. You can find this sort of thing in my writings on "reorganization." It's not a hierarchical level in my model of behavior, but more of a meta-control system concerned with intrinsic variables, the ones Ashby called critical or essential variables in

connection with "superstability." We're born knowing how to do that sort of control. We're not born knowing how to control much else.

If you were to read my stuff, we might save some time in finding common ground.

Rick Marken:

What makes pleasure good and pain bad? Intrinsic reference levels, I say. A high reference level for a perception defines that perception as good; a low or zero reference level defines it as bad. Reorganization, which controls critical variables having little to do with cognition or motor behavior, determines where important semi-permanent reference levels end up being set. I suppose that means mostly higher-order reference levels, because the lower ones are continually being adjusted. But even combinations of lower-level perceptions can get "good" and "bad" labels: for example, the result of drinking milk right after eating grapefruit. Bad sequence. Yucky taste -- meaning, perhaps, like a poison. This is an added comment, since you already know this.

Good on the ethical stuff. Hugh Gibbons (with an s -- he's a primate, but not that kind) has laid out the role of law neatly: law is our way of formalizing coercion. Justifying law is a matter of deciding when coercion yields a net increase in respect for the will of the individual (all individuals). Coercion, of course, is the only way that one person can actually thwart another's autonomous control. Law is not a matter of rules. It is a dynamic process that continually re-evaluates the borderline between justified and unjustified coercion. If Gibbons is right, the law expresses a very powerful ethical axiom that identifies respect for the will as the highest good (for them as has it). I think that's basically

what you said, too.

Joel Bennet:

See the report on the Hugh Gibbons trip. Meaning was proposed to consist of the non-verbal experiences to which words and word structures point. Second-language acquisition, according to this concept, requires attaching the terms of the second language not to terms of the first language, but to nonverbal experiences. Think of that picture of a pig that you see in some butcher shops and in cookbooks. Here in Joy of Cooking is an analogue picture of a pig, marked off into areas. Each area has a discrete linguistic symbol pasted over it (that an anatomist might have trouble recognizing): snout, shoulder butt, picnic shoulder, hock, fatback, loin, spareribs, etc. Strip away the printed words and what you have left are their meanings. Strip away the dotted boundaries that arbitrarily categorize segments of the pig, and what's left is one meaning of the word "pig." Replace the boundaries with different boundaries, put labels in a different language on them, and you have a second-language set of meanings. In Lower Slobbovia, for example, there is just one vertical dotted line through the center of the pig, the part in front being labelled "Good" and the other part being labelled "Mine."

Those are category-meanings. There are meanings at all levels. If I say "the cup is on the saucer", the meaning is a visual relationship that can be perceived without talking about it. If I say "The sky is falling" the

meaning is whatever picture-in-action came into your head when you first heard that story. If I say "honesty is the best policy" the meaning is a mental model of social principles that is very complex and contingent, but nonverbal.

I think that linguists tend to dismiss this kind of proposal as "mere semantics." But people interested in building translation-machines seem to be coming around to the view that artificial translation won't work without the machine having nonverbal experiences in the background. I think that's because the nonverbal experiences ARE the meanings of the linguistic entities. Verbal definitions allow you to substitute phrases for words, but in the end you have to attach the definitions to nonverbal meanings or you just have an endless and meaningless trip around the dictionary. When we read a dictionary, our minds teem with nonverbal meanings. When a machine looks up a word, all it gets is more words.

Hugh Petrie -- welcome, stranger, where have you been? You should have lots of helpful comments in Gary Cziko's area. I'm very pleased that you are insinuating control theory into High Places, even though, as most control theorists find, you have to avoid calling it control theory lest you scare everyone off.

I'm also skeptical about whether the modeling level (a) belongs between programs and principles, and (b) is different from the system concept level. I do think there is a "putting it all together into one coherent dynamic picture" level, and I think that level (plus, perhaps, all the others) amounts to a working model of whatever the subject matter is. There can, of course, be many such models operating at once or alternatively, and given the state of human affairs there's certainly no requirement that these models be cognizant of each other or consistent with each other. I think, in fact, that the human race is in rather a mess at these higher levels. Maybe when we've evolved a bit further, the mess will move to a higher level still. Maybe then we'll stop fighting wars over whose system concept is better than whose.

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